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Essays on Labor Economics and Experimental Economics

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by

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ABSTRACT

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In this work, I present three essays on Labor Economics and Experimental Economics. In the first essay, co-authored with Ashwin Rode, we explore whether and to what extent differences in prejudicial attitudes can be associated with the variation in black-white labor market gaps across U.S. metropolitan areas. Prejudicial attitudes are quantified using novel data on racially charged internet searches. We find a racially charged search rate that is one standard deviation higher is associated with almost a 23% higher black-white gap in annual income and 35% higher hourly wage gap.

In the second essay, I explore the effect of the Dot-Com recession on college graduates. Recent recessions in the United States and other countries have been associated with large negative demand shocks to specific industries, such as finance and real estate in the Great Recession and information technology in the recession of the early 2000s. Such recessions can have highly disproportionate impacts on recent college graduates in the affected industries. This essay documents these effects by studying the labor market outcomes of science and engineering students who graduated before and after the burst of the Dot-Com bubble. Overall, scientists and engineers graduating in the bust had on average 13 percent lower earnings during the first year after graduation compared to those graduating during the boom; for IT-related majors such as computer science (CS) and electrical and computer engineering (ECE) majors, these losses amounted to 17 percent. Furthermore, the loss in earnings for these IT majors, associated with the bust, persists over a 10 year period even though other majors experience a narrowing of the earnings gap over the same time. I

find strong evidence that the gap in earnings for the IT majors is largely driven by differences in hourly wages. Additionally, there is some evidence that the IT students graduating during the bust period were more likely to leave the IT field and to have lower job mobility, which may have contributed to their earnings losses over the long run.

In the third essay, I study the theory of Last-Place Aversion and delve into the deeper causes of this economic behavior. The theory of "last-place aversion" suggests that low-income individuals might oppose redistribution because it could differentially help the group just beneath them. However, distinctions in income groups aren't always clear in the real world, and whether individuals actually identify themselves with a certain rank can be a key factor in influencing behavior. I study the relationship between the behavior associated with last-place aversion and the salience of income rank in a laboratory experiment and using a US-wide voting survey. In a modified version of the dictator game with simple payoffs that are shown to each member in a group, I find no difference in the propensity to donate to the bottom-ranked individual among any of the other ranks. Additionally, using data from a nationwide election survey, I find the group making just above the minimum wage, but less than the median, oppose an increase in the minimum wage. More interestingly, the propensity to oppose an increase in the minimum wage increases across the states with a greater level of inequality. That is, when income categories are made salient due to higher inequality, it influences behavior that is suggestive of last-place aversion. The results suggest that group-associated behaviors are only valid to the point that groups are easily distinguishable.

Prejudicial Attitudes and Labor Market Outcomes

Ashwin Rode and Anand J. Shukla

1 Introduction

Despite decades of progress since the Civil Rights movement, blacks continue to fare worse than whites in various labor market outcomes. Vigdor (2006) finds that as recently as 2000, the average annual labor income of blacks was less than three-fourths that of whites, even after controlling for education and experience. It has also been documented that blacks fare much worse than whites during recessions, ranging from the Great Depression to the recent Great Recession (Sundstrom 1992; Hoynes et al. 2012).

Whether the black-white gap is primarily driven by discrimination or unmeasured differences in skill is a matter of ongoing debate.[1] However, few researchers have actually attempted to directly measure prejudicial attitudes and link these measures to the black-white gaps in labor market outcomes. In this paper, we use a novel measure of prejudicial attitudes − Google searches for a well-known racial epithet − to examine geographic variation in the black-white gaps in wages, annual earnings, and annual hours worked. In particular, we seek to understand whether and to what extent differences in prejudicial attitudes can be associated to the variation in these gaps across U.S. metropolitan areas. We find that even after controlling for education, occupation, and experience, metropolitan areas with higher search rates for the epithet have a significantly higher wage gap. A search rate that is one standard deviation higher is associated with a 23% higher annual income gap and a 35% higher hourly wage gap. Furthermore, there is evidence to suggest that prejudicial

1 See Neal and Johnson (1996) and Lang and Manove (2011) for two alternative perspectives in this debate.

attitudes have a greater link to the labor market outcomes of less educated workers. Our results are robust to the inclusion of a range of other controls, including search rates for related terms that do not signify racial prejudice, region fixed effects, changes in employment rates, and racial composition. Additionally, we are able to show that there is no relation between our prejudice measure and overall inequality (measured as the difference between the 90th and 10th percentile). However, given our data, it is not possible to rule out reverse causation.[2]

Our work contributes to an emerging literature that links direct measures of prejudicial attitudes to racial differences in outcomes.[3] In the pre-Civil Rights U.S. South, Sundstrom (2007) finds larger black-white earnings gaps in areas where plantation institutions were historically more prevalent and where white voters exhibited segregationist preferences. Using responses from the General Social Survey (GSS), Charles and Guryan (2008) provide evidence linking white prejudicial attitudes towards blacks and black-white wage gaps at the state level, however the association holds in a specific way. They find that a higher wage gap is not explained by a higher average degree of white prejudice in a state, but rather by a higher degree of prejudice in the left tail of the state's prejudice distribution. Charles and Guryan construe this relationship as supporting Becker's (1957) model of employer prejudice, which postulates that because the market sorts blacks away from the most prejudiced whites, outcomes for blacks should particularly depend on the left tail of the white prejudice distribution (Charles and Guryan, 2011). Insofar as Google search rates can be

2 While in principle, one might use time-series variation to shed light on causality, this is not possible given the nature of our data. Google search data is only available for years 2004 and later. Because wage gaps move slowly over time, it is not feasible to exploit time-series variation over such a short time span.

3 See the survey article by Charles and Guryan (2011). See also the recent work by Carlsson and Rooth (2007), which combines the correspondence study approach of Bertrand and Mullainathan (2004) with survey-based measures of prejudicial attitudes.

interpreted as a measure of the average degree of prejudice, our results stand in contrast to those of Charles and Guryan (2008).

The use of Google search data to quantify prejudicial attitudes was pioneered by Stephens-Davidowitz (2014), who finds that a metropolitan area's rate of racially charged searches is a strong negative predictor of its vote share for black Democratic presidential candidate Barack Obama in both the 2008 and 2012 U.S. presidential elections.[4] When prejudicial attitudes are instead measured by GSS responses, the comparable estimates of the same effect are considerably smaller in magnitude and are statistically insignificant (Mas and Moretti, 2009). It has long been recognized that individuals tend to withhold socially unacceptable attitudes, such as prejudice against blacks, from surveys (Tourangeau and Yan, 2007; Berinsky, 1999; Berinsky, 2002; Gilens et al., 1998; Kuklinski et al., 1997). In contrast, evidence suggests that internet searchers are unlikely to self-censor, as they are online and typically alone (Conti and Sobiesk, 2007; Kreuter et al., 2008). Internet searches thus represent a promising new way to gauge prejudicial attitudes that can yield insights beyond those offered by surveys. Our work extends the use of data on racially charged searches to address the broader question of black-white inequality in the labor market.

The rest of the paper is organized as follows: Section 2 explains our use of Google searches as a measure of prejudicial attitudes and its advantages over survey-based measures; Section 3 describes the empirical approach; Section 4 presents the results; Section 5 presents additional findings from robustness checks; Section 6 concludes.

2 Google Search Data

2.1 Motivation

[4] Stephens-Davidowitz (2014) controls for the area's vote share for the previous Democratic presidential candidate, John Kerry.

Due to both the widespread availability of the internet and the dominance of Google as a search engine in recent years, Google searches plausibly offer a representative picture of attitudes, perceptions, and trends. In 2007, almost 70% of Americans had access to the internet at home (Current Population Survey, 2007). More than half of internet searches in 2007 were performed on Google (Burns, 2008). While Google searchers are somewhat more likely to be affluent, large numbers of all demographics use the service (Hopkins, 2008).

Aggregate data from millions of Google searches tend to be consistent with ground realities.[5] For example, search rates for the word "God" explain 65% of the variation in the percent of a state's residents professing belief in God.[6] Search rates for "gun" explain 62% of the variation in gun-ownership rates across states (Stephens-Davidowitz, 2014).[7] These correlations hold despite the fact that searches containing the terms "God" and "gun" are imperfect indicators of underlying religiosity and gun ownership, respectively.[8]

A number of authors in disciplines ranging from finance and economics to epidemiology have exploited Google searches to capture broad attitudinal and behavioral patterns. Finance studies have utilized Google search data as a measure of investor attention (Da et al., 2011; Vlastakis and Markellos, 2012). Google search data has also been used in labor economics to quantify job search activity (Baker and Fradkin, 2014; Garthwaite et al., forthcoming). Furthermore, Google searches have been shown to predict outbreaks of Lyme disease (Seifter et al., 2010) and influenza (Ginsberg et al., 2009).

[5] As of March 2013, Google had an estimated 900 million unique monthly visitors worldwide. The second largest search engine, Bing, only had an estimated 165 million (eBizMBA, 2013).
[6] Data on belief in God can be found at http://www.pewresearch.org/fact-tank/2016/02/29/how-religious-is-your-state/.
[7] Data on gun ownership can be found at http://www. washingtonpost.com/wp-srv/health/interactives/guns/ownership.html.
[8] The top search query including "God" is "God of War", a video game. The top search query including "gun" is "Smoking Gun", a crime tabloid website (Stephens-Davidowitz, 2014).

Following Stephens-Davidowitz (2014), we obtain data on searches during 2004-2010 in U.S. metropolitan areas[9] containing the words “nigger” or “niggers” to proxy for prejudicial attitudes. A Google search query for these terms returns over two-hundred thousand webpages, the vast majority of which contain racially degrading content. Searchers of these terms tend to be looking for entertainment featuring derogatory depictions of African-Americans.[10] The top results for such searches are virtually all textbook examples of antilocution (i.e. a majority group sharing stereotype-driven jokes using coarse language outside a minority group’s presence). This is characterized as the first stage of prejudice in Allport’s (1979) classic treatise on the subject.

For racially charged searches to be a valid proxy for prejudicial attitudes, it is not necessary that every searcher for the epithet harbors prejudice, nor is it necessary that every individual harboring prejudice searches for the epithet. All that is required is that prejudice makes one more likely to search for the epithet. If this holds, then areas in which prejudicial attitudes are more prevalent will include the epithet in a greater percentage of searches.[11] However, to address possible confounding reasons individuals may search for the epithet, we also control for search rates of related terms that do not necessarily signify prejudicial attitudes.

[9] A metropolitan area, as defined by Google, corresponds to a Nielsen media market (Stephens-Davidowitz, 2014). For example, the metropolitan area of Albuquerque encompasses much of the state of New Mexico; the Los Angeles metropolitan area includes cities as far afield as Ventura and Riverside. A Nielsen media market comprises one or more counties. Unlike metropolitan areas as defined by the Census Bureau, Nielsen media markets cover the entire area of the United States.

[10] Kennedy (2003, pg. 22) writes that “nigger” is the “the best known of the American language’s many racial insults ... the paradigmatic slur.” It is unlikely that African-Americans are searching for “nigger” or “niggers” in large numbers. The common term used in African-American culture and in rap lyrics is “nigga(s)” (Rahman, 2011).

[11] The use of a single word or phrase, even one that can be used for different reasons, to proxy for an underlying attitude builds on the work of scholars who have analyzed newspaper text. Saiz and Simonsohn (2008) argue that newspaper articles about a city containing the word “corruption” can proxy a city's corruption. Gentzkow et al. (2011) show that, historically, Republican (Democratic) newspapers contain significantly more mentions of Republican (Democratic) presidential candidates.

Google searches have two main advantages over survey-based measures in capturing prejudicial attitudes. First, the large number of individuals who use Google dwarfs the sample size of any survey. Although the annual sample size of the GSS is around 3000 individuals,[12] the number of individuals surveyed regarding racial prejudice in a given geographical area may be exceedingly small. For example, in the state of Wyoming between 1990 and 2004, the GSS only asked 8 individuals whether they supported a ban on interracial marriage (Stephens-Davidowitz, 2014).[13] Another advantage of Google searches is their ability to elicit socially taboo sentiments. While survey respondents can be reluctant to admit to socially unacceptable attitudes (Tourangeau and Yan, 2007; Berinsky, 1999; Berinsky, 2002; Gilens et al., 1998; Kuklinski et al., 1997), Google searchers, who are online and typically alone, can express taboo thoughts with relative ease (Kreuter et al., 2008).[14] Indeed the large number of searches for pornography and sensitive health conditions suggests that Google searchers routinely express interests that they might hesitate to express in other venues (Stephens-Davidowitz, 2014).

The deficiencies of survey-based measures in capturing socially unacceptable attitudes may lead to systematic mismeasurement of such attitudes. Although Stephens-Davidowitz (2014) documents a positive correlation between racially charged searches and GSS-based measures of prejudice, he suggests that the GSS could under-report prejudice in predictable ways. For example, he finds that a state's vote share for Democratic presidential candidate John Kerry in 2004 is negatively associated with the percentage of whites supporting a ban

[12] See http://publicdata.norc.org:41000/gssbeta/faqs.html#10.
[13] Mas and Moretti (2009) use the percent of survey respondents who replied "yes" to this question as a proxy for prejudicial attitudes at the state-level, which is the highest resolution of location in the GSS data.
[14] Survey evidence suggests that individuals do not self-censor their Google searches and are not hesitant to search even for topics they would not want their parents or future employers to know about (Conti and Sobiesk, 2007).

interracial marriage, but finds no correlation between the Kerry vote share and the racially charged search rate. A potential explanation for this discrepancy is that racial prejudice is more socially unacceptable among Democrats. Thus, under-reporting of prejudice in surveys will be more severe in areas with more Democrats, and survey data will falsely reveal a negative correlation between percent Democrat and prejudice against blacks. Such issues of potentially systematic mismeasurement underscore the need for an alternative to survey-based measures of prejudicial attitudes. For reasons outlined earlier, Google searches are a promising alternative.

2.2 Construction of Proxy Variable

Data on searches for particular words or phrases for U.S. metropolitan areas are publicly available through Google Trends.[15] Google Trends does not report raw search volumes, rather it reports an index for each metropolitan area based on a random sample of searches drawn from the universe of searches during a user-specified time span.[16] In our case, a sample would be drawn from the universe of searches conducted during the years 2004-2010. The index is calculated through a two-step procedure. In the first step, Google calculates a search rate for the user-defined term(s) for each metropolitan area. The numerator of the search rate is the number of searches in the sample that come from a given metropolitan area and contain the user-specified term(s); the denominator is the total number of searches in the sample that come from a given metropolitan area. In the second step, Google Trends divides the search rates for each metropolitan area by a common factor such that the metropolitan

[15] www.google.com/trends

[16] Google Trends covers search data from 2004 to the present. User-defined time spans can range in length from a single day to the entire covered time period.

area with the maximal search rate is assigned a value of 100. Formally, for metropolitan area j,

$$SearchIndex_j = 100 * \frac{[\frac{Google\ searches\ containing\ search\ term(s)}{Total\ Google\ searches}]_j}{[\frac{Google\ searches\ containing\ search\ term(s)}{Total\ Google\ searches}]_{max}}$$

Google Trends reports only the search index and not any of its components. However, the search index for a metropolitan area is indicative of its search rate for a term, the only difference being due to scaling.[17]

In order to address possible sampling variation, we downloaded the search index for all metropolitan areas 1150 times and averaged each metropolitan area's 1150 samples. However, sampling variation does not appear to be a significant problem. All our summary statistics and results remain essentially unchanged even if only half of the samples (i.e. 575 samples) are used.

Table 1 lists the 10 metropolitan areas with the highest and lowest average search index for the epithet (henceforth referred to as $PrejudiceIndex$). A cursory glance at the top 10 metropolitan areas reveals a preponderance of Rust Belt cities along with a few Southern cities. On the other hand, the bottom 10 cities tend to be situated in the Rocky Mountain or Pacific regions. Figure 1 illustrates how $PrejudiceIndex$ varies across the map of the U.S. and is broadly consistent with the rankings in Table 1. (For the full list of metropolitan areas and their $PrejudiceIndex$ values, see Table A1 in Appendix A).

[17] The scaling factor does vary slightly with each sample, however as discussed below, sampling variation does not appear to be a significant problem.

It should be noted that Google Trends reports an index of zero for metropolitan areas whose absolute search volume is below an unreported threshold. A index value of zero does not represent bottom-coding of the search rate but rather a lack of information; we omit such metropolitan areas from our analysis. However, this omission is unlikely to bias our results as the metropolitan areas we do include contain the vast majority of the total U.S. population. Moreover, we argue in Section 3 that the metropolitan areas in our sample look similar to the U.S. as a whole.

A metropolitan area's racial composition does not appear to be correlated with its $PrejudiceIndex$. As Figure 2 illustrates, there appears to be at best a weak positive association between a metropolitan area's $PrejudiceIndex$ and the percentage of its population that is black. However, $PrejudiceIndex$ is correlated with other socio-economic attributes.[18] Prejudicial attitudes appear to be more widespread in areas with lower education and income levels. There is a strong negative correlation between $PrejudiceIndex$ and the share of a metropolitan area's working-age population that has completed at least 4 years of college education (Figure 3). A strong negative correlation also exists between $PrejudiceIndex$ and average white income (Figure 4). On the other hand, $PrejudiceIndex$ is positively correlated with the share of the working-age population employed in manufacturing-, construction-, and repair-related occupations (Figure 5).[19] These correlations point to the larger question- how do prejudicial attitudes form?- which is beyond the scope of this paper.

[18] The socio-economic variables are taken from the American Community Survey, as described in Section 3.
[19] Stephens-Davidowitz (2014) also documents a correlation between PrejudiceIndex and GSS-based measures of prejudice.

Next, we combine Google search data at the metropolitan area level with individual-level data on labor market and demographic variables to examine the association between prejudicial attitudes and labor market outcomes.

3 Empirical Approach

Individual-level demographic and employment data are obtained from the American Community Surveys (ACS) from the years 2006-2011. The ACS is a repeated cross-section survey conducted annually by the Census Bureau; because the ACS reports data from the previous year, our data cover the years 2005-2010.[20] Using data from multiple years ensures that there are sufficient numbers of both blacks and whites in each metropolitan area. We focus on three outcome variables: the natural log of annual income, the natural log of hourly wage, and annual hours worked.[21] In order to avoid potentially confounding effects of low labor force participation by women and the elderly, we limit our analysis to black and white males aged 18 to 64.

Table 2 summarizes the average nationwide black-white gaps in the three outcome variables from the ACS data. The first column simply reports the raw gaps, while the second column reports the residual gaps that remain after controlling for individual demographic characteristics (age, age squared, education, and occupation).[22] Each of the gaps is reduced

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[20] We obtained the ACS data via the Integrated Public Use Microdata Series (IPUMS-USA).

[21] Annual income is directly reported by the ACS. It is top-coded at the 99.5th percentile within each state; the top-coded observations are kept in our study. Annual hours worked is calculated by multiplying usual hours worked per week by the number of weeks worked. The hourly wage is calculated by dividing annual income by annual hours worked.

[22] Education and occupation are controlled for non-parametrically. Dummy variables are included for categories of education and occupation. The education categories (denoting highest level of education completed) in the ACS are: (1) N/A or no schooling; (2) Nursery school to grade 4; (3) Grade 5, 6, 7, or 8; (4) Grade 9; (5) Grade 10; (6) Grade 11; (7) Grade 12; (8) 1 year of college; (9) 2 years of college; (10) 4 years of college; and (11) 5+ years of college. The occupation categories are: (1) Managerial and Professional (includes individuals that require a certain degree, skill, or qualification such as managers, engineers, physicians, etc.); (2) Technical, Sales and Administrative (consists of individuals such as technicians, sales representatives, and clerks); (3)

by approximately half after the controls are added.[23] The magnitudes of the residual gaps are broadly consistent with those of previous studies that have relied on other data sources. The ACS data reveal a residual annual income gap of 26%, which is similar to the result found by Vigdor (2006). The average nationwide residual hourly wage gap is approximately 16% and the residual annual hours gap is almost 200 hours.

The third column in Table 2 reports the average residual gaps in the three outcome variables for the 82 metropolitan areas that have a non-zero value of $PrejudiceIndex$. The average gaps for this subset of metropolitan areas, which house nearly 80% of the US population, are virtually identical to the average nationwide gaps. Thus the omission of metropolitan areas for which $PrejudiceIndex$ is unobserved is unlikely to bias our results. The fourth column in Table 2 reports the average residual gaps for the 61 metropolitan areas used in our preferred empirical specification. In the preferred specification, we control for searches of other terms besides the epithet. An additional 18 metropolitan areas are omitted as they report an index value of zero for searches of one or more of these other terms. The gaps are again virtually identical to the average nationwide gaps. The 61 metropolitan areas used in the preferred specification contain over 70% of the U.S. population.

Table 3 separately reports the average residual nationwide black-white gaps in the three outcome variables by education category (no college, some college, ≥ 4 years of college). The residual annual income gap and hourly wage gap is on average highest among individuals with 4 or more years of college, a finding that accords with Bound and Freeman

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Service (includes barbers, maids, waiters, etc.); (4) Farming, Forestry, and Fishing (includes individuals that are farmers); (5) Precision production, Craft, and Repair (includes mechanics, construction workers, tailors, etc.); (6) Operators, Fabricators, and Laborers (includes machine operators and other manufacturing related jobs); and (7) Non-occupational responses (includes military persons and unemployed).

[23] It is of course possible that a gap remaining after individual characteristics are controlled for might be due to discrimination. However, it is also possible that even a raw gap can reflect discrimination, insofar as discrimination affects the education and occupation opportunities of blacks.

(1992).[24] However, individuals with 4 or more years of college have on average the lowest residual annual hours gap.

Like Charles and Guryan (2008), our empirical approach involves two steps. In the first step, we use individual-level data to obtain a black-white gap in the outcome of interest for each metropolitan area. We use the location information provided in the ACS to place individuals in a metropolitan area as defined by Google.[25] In the second step, we regress the metropolitan area gaps on the prejudicial search index from Google. In both steps, our preferred specification uses data from the aforementioned 61 metropolitan areas that account for over 70% of U.S. the population.

Let Y_{ijt} denote an outcome of an individual i residing in metropolitan area j and surveyed in year t. The specification for the first step is

$$\textbf{(1)} \qquad Y_{ijt} = \alpha + \beta_j * Black_{ijt} + \lambda * x_{ijt} + \vartheta_t + \epsilon_{ijt}.$$

The vector x_{ijt} contains an individual's age, age squared, education, and occupation. In addition we control for the year an individual was surveyed (ϑ_t) to account for shocks common to all individuals in a given year. The variable ϵ_{ij} denotes the idiosyncratic error term. The coefficient β_j captures the average residual black-white gap in metropolitan area j.

In the second step, the unit of observation is the metropolitan area. The values of the dependent variable are the $\hat{\beta_j}$s from the first step, and the explanatory variable of interest is the metropolitan area's search index for the epithet ($PrejudiceIndex_j$). The specification is

$$\textbf{(2)} \qquad \hat{\beta_j} = \eta + \psi * PrejudiceIndex_j + \theta * Pctblack_j + \delta * OtherSearches_j + Region_j + \zeta_j.$$

[24] Bound and Freeman (1992) document the widening of the black-white earnings gap among college graduates during the 1980s. They attribute this trend to weakened affirmative action and a growth in the supply of black college graduates, as well as sectoral shifts in the demand for workers. More recent work by Weinberger and Joy (2007), which focuses exclusively on college graduates, finds that racial wage differentials differ dramatically by college major.

[25] Google's metropolitan areas correspond to Nielsen media markets, which cover the entire area of the U.S.

Each observation is weighted by the metropolitan area's population in 2010.[26] The variable ζ_j denotes the idiosyncratic error term. We also include fixed effects for Census regions.[27] Following Charles and Guryan (2008), we control for the black percentage of a metropolitan area's population ($PctBlack_j$) in order to account for race-composition effects. Additionally, in order to account for possible non-racist reasons why the epithet might be searched, we follow Stephens-Davidowitz (2014) in controlling for Google search rates of other terms that do not necessarily have a racist connotation. Specifically, we control for searches of the term "African American" to capture the extent of internet searches on topics of interest to blacks. We also control for searches of "nigga" or "niggas", terms that are used by blacks themselves and figure in rap songs. Finally, we control for searches of a salient expletive in order to capture search activity directed towards profanity in general.[28] The vector $OtherSearches_j$ contains index values for these three terms.[29] On top of the main results, which we discuss in the next section, we also conduct a separate analysis by education categories- No college, some college, and ≥ 4 years of college- which we discuss in Appendix A.

4 Results

Our preferred specification includes the full set of controls in the first step (age, age squared, education, occupation, year of survey) and second step (percent black, searches for other

[26] Population figures at the county level were obtained from the Surveillance Epidemiology and End Results, National Cancer Institute.
[27] Each metropolitan area is classified into one of four Census regions: Northeast, South, Midwest, or West. Census regions comprise groups of states. Three metropolitan areas spanned more than one region: Cincinnati (Midwest/South), Louisville (South/Midwest), Philadelphia (Northeast/South). We classified these based on the Census region of the state in which the metropolitan area's principal city is located in. By this rule, Cincinnati falls in the Midwest, Louisville falls in the South, and Philadelphia falls in the Northeast.
[28] Following Stephens-Davidowitz (2014), we control for searches of the term "fuck".
[29]As with searches for the epithet, we take an average of 1150 samples for each metropolitan area in order to obtain an index for each of the three terms.

terms, Census region fixed effects). Because our primary object of interest is ψ, the coefficient on $PrejudiceIndex$, we focus on the results from the second step. (Results from the first step can be found in Appendix A, Table A2.)

A higher $PrejudiceIndex$ is associated with a significantly higher black-white gap in annual income (Table 4). The results from our preferred specification indicate that a prejudicial search rate one standard deviation higher (i.e. about 14 index points higher) is associated with a 6.2 percentage point higher residual annual income gap (Column 4, Table 4). This represents approximately a quarter of the 26% average nationwide residual annual income gap.

An alternative explanation for these results could be that the long-term decline of low-skill manufacturing jobs in certain cities (e.g. Rust Belt cities) might have especially hurt the less educated among blacks, who were unable to leave these cities due to high costs of relocation.[30] We test this idea by including an additional regressor: a metropolitan area's percent change in employment from 2000 to 2010.[31] While we find that a decline in long-term employment is associated with a higher gap in annual income, there is virtually no change in the coefficient on $PrejudiceIndex$ (Column 5, Table 4).

Similar results hold for the black-white gap in hourly wage (Table 5). According to our preferred specification, a prejudicial search rate one standard deviation higher is associated with a 5.6 percentage point higher residual hourly wage gap (Column 4, Table 5), which represents almost 35% of the 16% average nationwide residual hourly wage gap. These

[30] Indeed educational attainment is strikingly low among blacks residing in Rust Belt cities, which also happen to rank among the top in racially charged internet searches (Table 1). For instance, in Flint, Michigan, fewer than 10% of blacks have a Bachelor's degree or higher, compared to a national average of 16%.

[31] We use data from Local Area Unemployment Statistics on the number of employed by county from the Bureau of Labor Statistics. Similar to the earlier matching method, we aggregate these counties to correspond to the Nielsen Media Market definitions of metropolitan areas.

results remain virtually unchanged when percent change in employment is added to the regression.

Examining the annual income gap in levels rather than logs allows for the inclusion of individuals with zero income (i.e. individuals who did not work). Table 6 shows that a higher prejudicial search rate is associated with a higher black-white gap in annual income even when measured in levels, although the result is statistically insignificant in the preferred specification (Column 4, Table 6). Adding percent change in employment does not affect the magnitude of the estimated coefficient on $PrejudiceIndex$. However, the coefficient does become marginally significant (Column 5, Table 6).

The only gap that does not appear to be strongly associated with $PrejudiceIndex$ is the gap in annual hours worked. As Table 7 reveals, the coefficient on $PrejudiceIndex$ tends to be statistically and economically insignificant. However, a decline in employment is associated with a significantly higher gap in annual hours worked.

5 Robustness Checks

It has been argued that the relative economic standing of blacks *vis-a-vis* whites is inadequately captured by black-white wage gaps, as these gaps typically ignore the disproportionately high rates of incarceration among blacks (Pettit and Western, 2005). Although incarcerated persons are included in the ACS data we use, information about their labor income is often missing and hence they are not fully represented in our analysis. However, this would likely bias our results downward. Figure 7 shows that metropolitan areas with higher values of $PrejudiceIndex$ also tend to have larger black-white gaps in incarceration rates.[32] If high incarceration rates of blacks cause the black-white wage gap to be downward biased (as argued by Pettit and Western, 2005), then the bias would be most severe in areas with high rates of racially charged searches. As a result, the positive association we find between racially charged searches and wage gaps would be even stronger if the bias was corrected.

Another possible concern is that our results reflect overall inequality rather than prejudicial attitudes against blacks in particular. Areas with higher values of $PrejudiceIndex$ may have greater wage inequality overall, for reasons that are not necessarily related to anti-black prejudice. We examine this possibility by considering other dependent variables that measure different forms of inequality. Specifically we look at the Hispanic-white wage gap among males, the male-female wage gap among whites, and the difference in the natural logarithm of a metropolitan area's 90th and 10th percentile of annual income among males.[33] The first two of these variables capture specific types of inter-group inequality, while the last variable captures inequality more generally and has been widely used in previous work (Juhn et al., 1993).[34] We find that $PrejudiceIndex$ is positively associated with Hispanic-white and male-female wage inequality, and the magnitude of association is qualitatively similar to what is observed for the black-white wage gap (Table

[32] The relationship is even more prominent when excluding the two outlier observations with the largest gaps in incarceration rates (U.S. Dept. of Justice, Bureau of Justice Statistics. Census of Jail Inmates: Individual-level Data, 2005).

[33] All these quantities are calculated using ACS data for persons aged 18 to 64. The wage gaps are adjusted as per Equation 1. In calculating the 90-10 percentile difference, annual income has been normalized to 1983 dollars using the consumer price index. The average adjusted Hispanic-white annual income gap among males in the 61 preferred sample metropolitan areas is 7%, while the corresponding hourly wage gap is 13%. The average adjusted male-female annual income gap among whites in the 61 preferred sample metropolitan areas is 47%, while the corresponding hourly wage gap is 26%. The 90-10 percentile difference in the natural logarithm of annual income for males in the 61 preferred sample metropolitan areas is 2.71.

[34] The explanatory variable of interest is the same as before (i.e. search rates for the epithet), and we also control for search rates of the same additional terms. While it would be interesting to consider searches for terms that specifically indicate other forms of prejudice, it is not clear that any such terms are nationally salient. For example, the anti-Hispanic epithet "wetback" has a relatively low search volume and is commonly searched only in parts of the southwestern U.S. (source: Google Trends).

8). These results suggest that $PrejudiceIndex$ could be capturing broad attitudinal patterns that extend beyond anti-black prejudice. However, while $PrejudiceIndex$ does seem to be positively associated with various forms of inter-group inequality (black-white, Hispanic-white, male-female), it appears to be unrelated to overall inequality. When the dependent variable is the 90-10 difference, the estimated coefficient on $PrejudiceIndex$ is statistically and economically insignificant (Table 9).

6 Discussion and Conclusion

The findings in this paper provide evidence for a link between racial prejudice and black-white gaps in labor market outcomes. Racial prejudice is quantified using novel data on racially charged internet searches. Metropolitan areas with higher racially charged search rates have wider black-white gaps in annual income and hourly wage, and this association is somewhat stronger among less educated workers. Our results contrast with those of Charles and Guryan (2008), who quantify racial prejudice using survey responses and find that geographical variation in wage gaps cannot be explained by variation in the average level of prejudice.

The positive association between the average level of racial prejudice and the black-white wage gap, while not predicted by Becker's (1957) model, is consistent with search-theoretic models of labor market discrimination such as those of Black (1995) and Lang et al. (2005). In Black's model, the presence of discriminatory employers, who do not employ blacks, increases the search costs of black workers to find non-discriminatory employers. The non-discriminatory employers thus enjoy monopsony power, which they exploit by offering lower wages to black workers in equilibrium. Lang et al. (2005) show that black-white wage gaps can exist in equilibrium even if employers cannot discriminate in wage offers but only in hiring decisions. In their model, labor market frictions magnify even a small degree of prejudice into a dramatic wage gap. Although we have no direct evidence that blacks face higher job search costs in more prejudiced metropolitan areas within the U.S., the results of Carlsson and Rooth's (2012) correspondence study in Sweden do support such a mechanism. They find that resume submissions with Middle Eastern names received substantially fewer callbacks than comparable resumes with Swedish names, and the gap in callback rates is significantly greater in regions with higher average levels of prejudice as measured by survey responses. These results suggest that higher search costs for black workers may be a mechanism through which high average prejudice levels can lead to high black-white wage gaps.

We find that prejudicial attitudes are associated with a higher wage gap even among individuals with the same level of education. However, it is possible that prejudicial attitudes affect the education decisions of blacks by dampening the incentive to invest in human capital (Lundberg and Startz, 1983). The negative relationship between the index of racially charged Google searches and the percentage of blacks with a college degree supports this idea (Figure 6). In the least prejudiced metropolitan areas, nearly 30% of blacks hold college degrees, while in the most prejudiced areas this number is under 10%. Differential human capital accumulation bears on our results in two ways. First, insofar as prejudicial attitudes lead blacks to choose lower levels of education, our results would understate the true effect of such attitudes as we focus only on *residual* wage gaps that remain after controlling for education levels. Second, there may exist differences in human capital between blacks and whites that are not captured by educational attainment (i.e. college major, school quality),[35] and these unobserved differences could also be the result of prejudicial attitudes. Thus the adverse effect of prejudicial attitudes on human capital accumulation by blacks could be still be a viable mechanism to explain our findings.

The cross-sectional positive relationship between racially charged internet searches and the black-white gaps arguably represents a long-run equilibrium phenomenon. Our results are not driven by temporary idiosyncratic spikes in internet searches as we measure internet search rates over a 6 year time span.[36] Although this time span encompasses various phases of the business cycle, the inclusion of year dummy variables controls for the impact of business cycle fluctuations. Moreover, the inclusion of Census region dummy variables accounts for unobserved, time-invariant factors common to broad regions of the country. We also account for long-term changes in employment at the metropolitan area level. While declines in long-term employment tend to be associated with larger gaps in all outcomes, racially charged internet searches remain a robust predictor of annual income and hourly wage gaps. Despite all the steps we have taken, the possibility of reverse causation cannot be ruled out. It could very well be the case that racially charged internet searches are a response to black-white labor market inequality rather than the other way around. On a deeper level, the cross-sectional association we observe could be the manifestation of a process in which prejudicial attitudes create racial inequality, which in turn helps to perpetuate those attitudes. More research is required to shed light on such issues.

[35] These differences have been documented in Weinberger (1998).
[36] Also, it is well known, racial wage gaps tend to change only slowly over time (Vigdor, 2006).

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Figures and Tables

Figure 1: Google Metropolitan Areas and their $PrejudiceIndex$

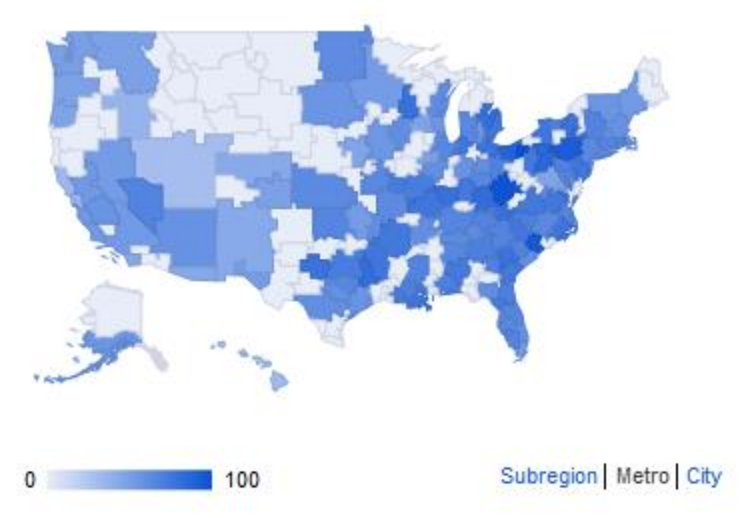

Figure 2: Relationship between the percent of population that is black and prejudicial searches

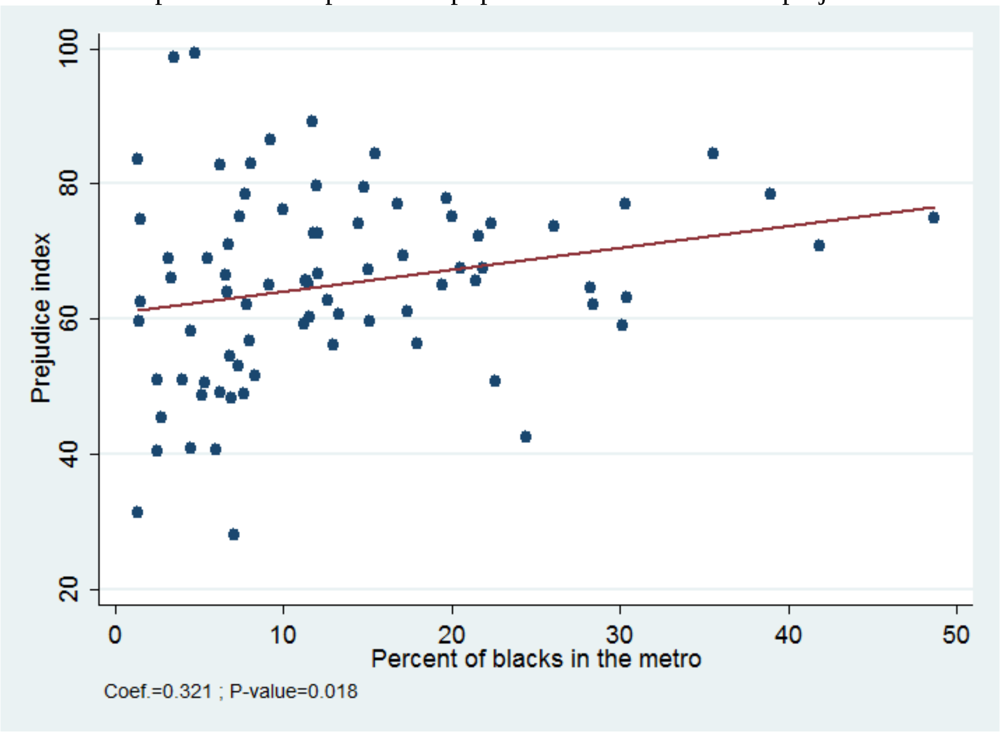

Figure 3: Relationship between share of metropolitan area population with at least four years of college education and prejudicial searches

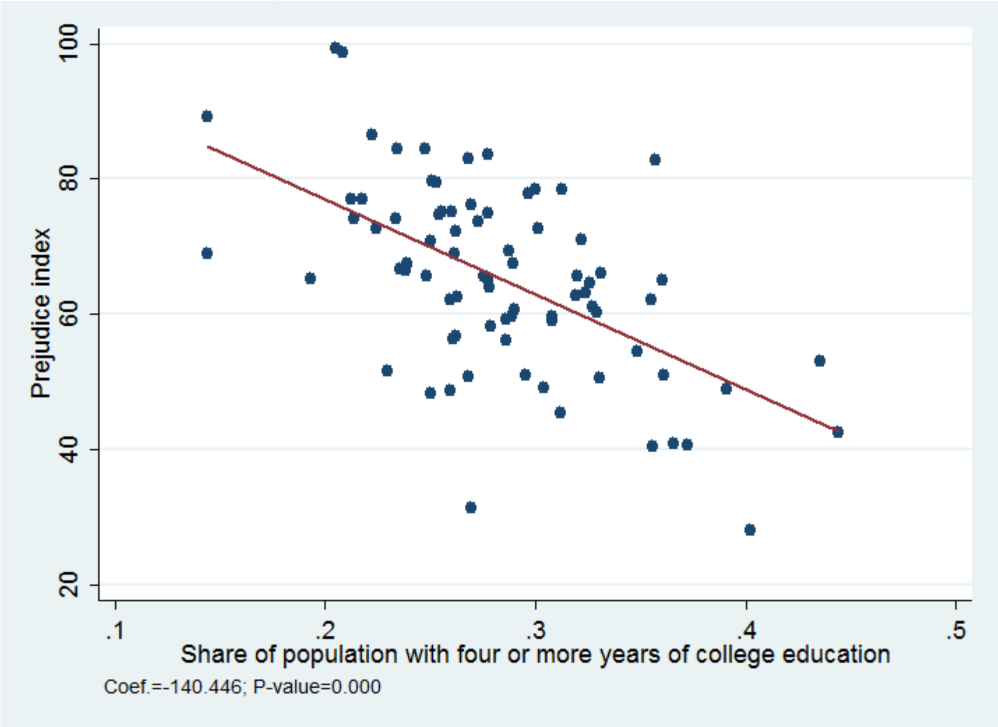

Figure 4: Relationship between average white income in metropolitan area and prejudicial searches

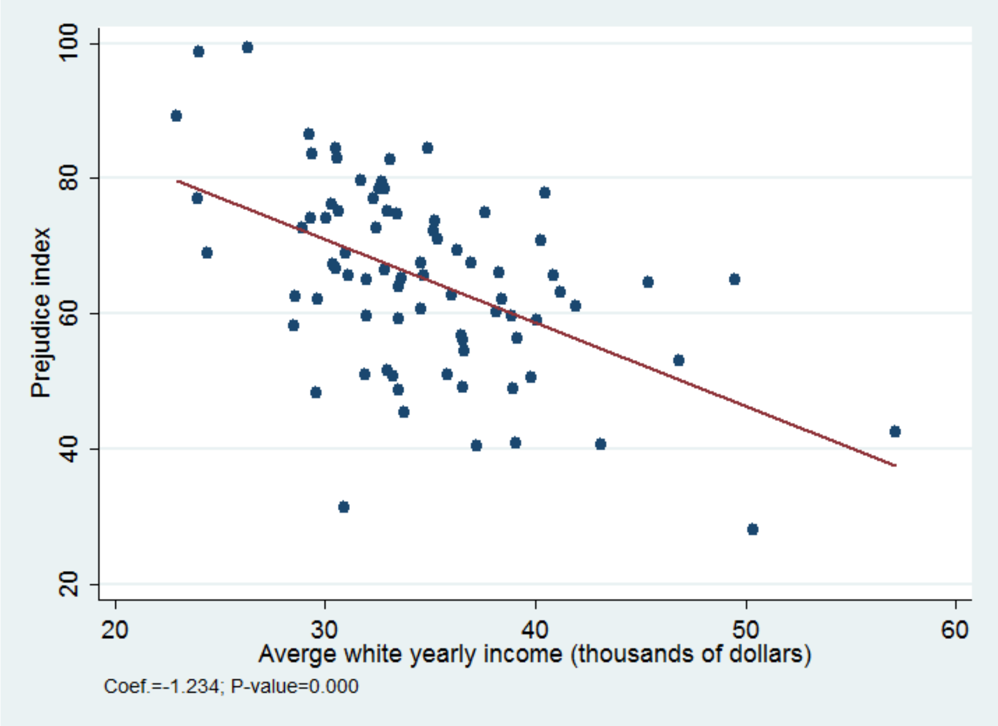

Figure 5: Relationship between share of metropolitan area population that is in manufacturing-, construction-, and repair-related fields and prejudicial searches

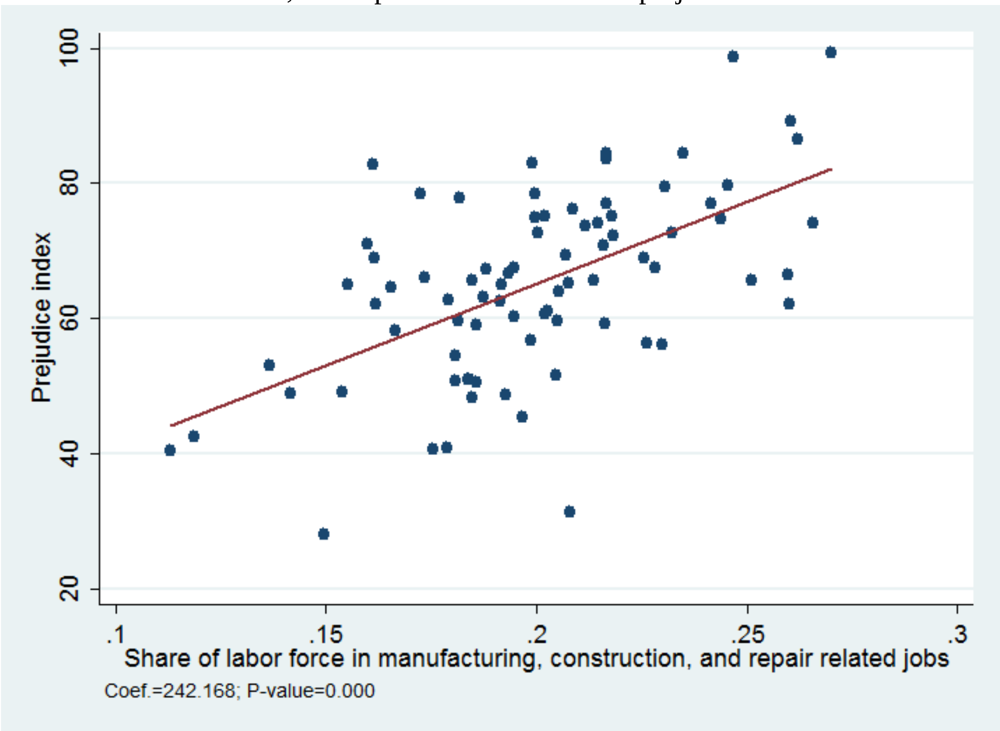

Figure 6: Relationship between share of metropolitan area black population with at least four years of college education and prejudicial searches

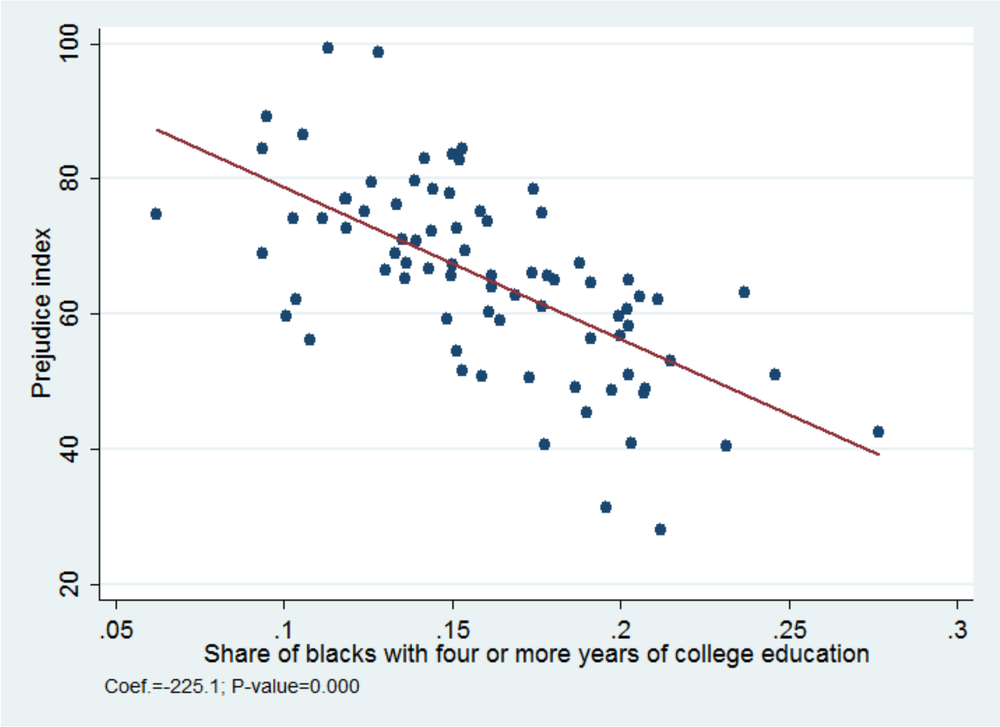

Figure 7: Relationship between black-white incarceration gaps and prejudicial searches

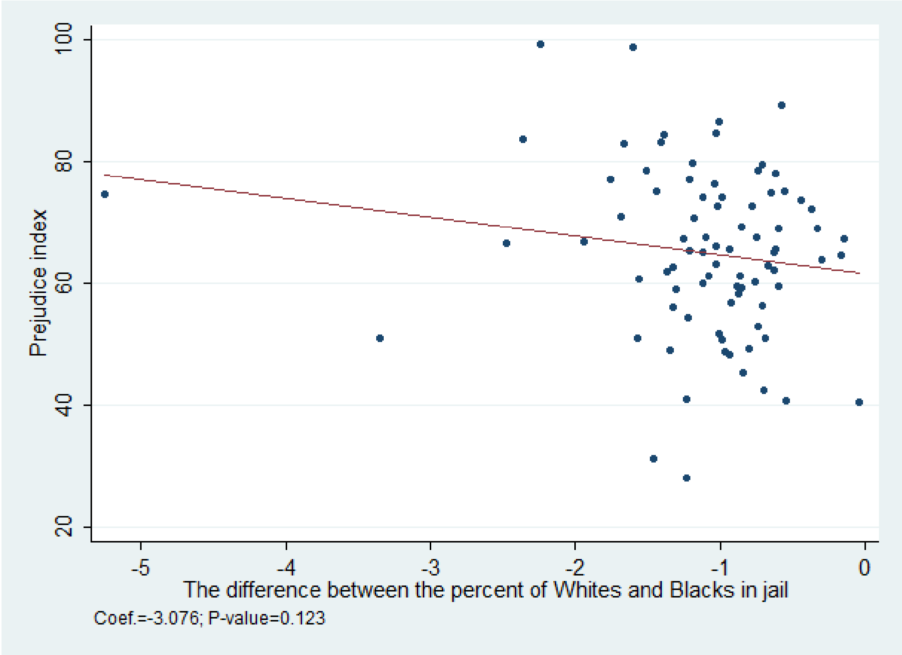

Table 1: Search Index for the Racial Epithet by Metropolitan Areas

Top Ten Areas	Index	Bottom Ten Areas	Index
Wilkes Barre-Scranton, PA	99.3	Austin, TX	48.9
Johnstown-Altoona, PA	98.7	Phoenix, AZ	48.6
Flint, MI	89.2	San Antonio, TX	48.3
Toledo, OH	86.6	Portland, OR	45.3
Baton Rouge, LA	84.5	Washington, DC	42.3
Roanoke-Lynchburg, VA	84.3	Denver, CO	40.9
La Crosse-Eau Claire, WI	83.5	Minneapolis-St Paul, MN	40.6
Springfield-Holyoke, MA	83	Honolulu, HI	40.4
Lexington, KY	82.9	Salt Lake City, UT	31.2
Louisville, KY	79.7	San Francisco, CA	27.9

Note: Metropolitan areas with an index of zero are excluded as the search volume is too low according to a threshold set by Google and therefore Google codes the index as zero. The excluded metropolitan areas are all relatively small in population. All index values represent averages of 1150 samples of index values.

Table 2: Residual Black-White Gaps in the U.S.
based on American Community Survey Data (2006-2011)

Outcome	Raw Gap	Residual Gap	Residual Gap	Residual Gap
Ln(Annual Income)	-0.434***	-0.263***	-0.261***	-0.263***
	(0.003)	(0.002)	(0.011)	(0.011)
Ln(Hourly Wage)	-0.269***	-0.152***	-0.157***	-0.161***
	(0.002)	(0.007)	(0.008)	(0.008)
Annual Hours	-433.2***	-209.6***	-192.3***	-192.8***
	(1.9)	(1.58)	(8.8)	(9.1)
Sample	Entire U.S.	Entire U.S.	82 metropolitan areas	61 metropolitan areas

Note: Standard errors in parentheses. The residual gaps are the gaps that remain after controlling for individual demographic characteristics (age, age squared, education, and occupation). The superscripts*,**, and***denote significance at the 10, 5, and 1 percent levels respectively.

Table 3: Residual Black-White Gaps by Education Level

based on American Community Survey Data (2006-2011)

Outcome	No College	Some College	≥ 4 yrs. College
Ln(Annual Income)	-.267***	-0.213***	-0.298***
	(0.014)	(0.011)	(0.016)
Ln(Hourly Wage)	-0.123***	-0.149***	-0.226***
	(0.010)	(0.008)	(0.013)
Annual Hours	-233.7***	-154.2***	-148.3***
	(11.7)	(10.9)	(8.0)

Note: Standard errors in parentheses. The residual gaps are the gaps that remain after controlling for individual demographic characteristics (age, age squared, education, and occupation) for the metropolitan areas in our preferred specification (i.e. metropolitan areas with non-zero values of the search index for every term we control for). The superscripts*,**, and***denote significance at the 10, 5, and 1 percent levels respectively.

Table 4: Racial Prejudice and the Black-White Gap in Ln(Annual Income)

	Dependent Variable: Black-White Gap in Ln(Annual Income)				
	(1)	(2)	(3)	(4)	(5)
PrejudiceIndex	-0.0035***	-0.0048***	-0.0054***	-0.0044***	-0.0043***
	(0.0012)	(0.0009)	(0.0014)	(0.0015)	(0.0011)
Percent Black		0.0059***	0.0066*	0.0116***	0.0105***
		(0.0015)	(0.0034)	(0.0039)	(0.0033)
Percent Change in Employment					0.0045***
					(0.0014)
Demographic Controls	Yes	Yes	Yes	Yes	Yes
Year Controls	Yes	Yes	Yes	Yes	Yes
Google Controls	No	No	Yes	Yes	Yes
Region Fixed Effect	No	No	No	Yes	Yes
R2	0.15	0.31	0.4	0.6	0.66
Observations	82	82	61	61	61

Note: Standard errors in parentheses. The unit of observation is metropolitan area. The dependent variable is the residual black-white gap in ln(annual income), after controlling for individual demographic characteristics (age, age squared, education, occupation) and year of survey. The superscripts*,**, and***denote significance at the 10, 5, and 1 percent levels respectively.

Table 5: Racial Prejudice and the Black-White Gap in Ln(Hourly Wage)

	Dependent Variable: Black-White Gap in Ln(Hourly Wage)				
	(1)	(2)	(3)	(4)	(5)
PrejudiceIndex	-0.0023**	-0.0027**	-0.0039***	-0.0040***	-0.0039***
	(0.0008)	(0.0008)	(0.0008)	(0.0007)	(0.0007)
Percent Black		0.0016	-0.0013	-0.0064**	0.0062**
		(0.0013)	(0.0025)	(0.0026)	(0.0025)
Percent Change in Employment					0.0009
					(0.0008)
Demographic Controls	Yes	Yes	Yes	Yes	Yes
Year Controls	Yes	Yes	Yes	Yes	Yes
Google Controls	No	No	Yes	Yes	Yes
Region Fixed Effect	No	No	No	Yes	Yes
R-squared	0.13	0.15	0.38	0.73	0.74
Observations	82	82	61	61	61

Note: Standard errors in parentheses. The unit of observation is metropolitan area. The dependent variable is the residual black-white gap in ln(hourly wage), after controlling for individual demographic characteristics (age, age squared, education, occupation) and year of survey. The superscripts*,**, and***denote significance at the 10, 5, and 1 percent levels respectively.

Table 6: Racial Prejudice and the Black-White Gap in Annual Income

	Dependent Variable: Black-White Gap in Annual Income				
	(1)	(2)	(3)	(4)	(5)
PrejudiceIndex	-44.319*	-61.771***	-82.813***	-52.69	-50.895*
	(26.603)	(23.183)	(27.664)	(31.421)	(26.309)
Percent Black		80.679**	45.144	149.737	132.287
		(37.577)	(72.352)	(91.689)	(83.513)
Percent Change in Employment					68.374**
					(33.504)
Demographic Controls	Yes	Yes	Yes	Yes	Yes
Year Controls	Yes	Yes	Yes	Yes	Yes
Google Controls	No	No	Yes	Yes	Yes
Region Fixed Effect	No	No	No	Yes	Yes
R-squared	0.04	0.1	0.21	0.34	0.37
Observations	82	82	61	61	61

Note: Standard errors in parentheses. The unit of observation is metropolitan area. The dependent variable is the residual black-white gap in annual income, after controlling for individual demographic characteristics (age, age squared, education, occupation) and year of survey. The superscripts*,**, and***denote significance at the 10, 5, and 1 percent levels respectively.

Table 7: Racial Prejudice and the Black-White Gap in Annual Hours Worked

Dependent Variable: Black-White Gap in Annual Hours Worked					
	(1)	(2)	(3)	(4)	(5)
PrejudiceIndex	-2.806*	-3.686**	-2.673	-1.076	-0.949
	(1.563)	(1.467)	(2.116)	(1.729)	(1.382)
Percent Black		4.070***	6.071	6.210	4.981
		(1.233)	(4.455)	(3.718)	(3.110)
Percent Change in Employment					4.813***
					(1.563)
Demographic Controls	Yes	Yes	Yes	Yes	Yes
Year Controls	Yes	Yes	Yes	Yes	Yes
Google Controls	No	No	Yes	Yes	Yes
Region Fixed Effect	No	No	No	Yes	Yes
R-squared	0.08	0.14	0.12	0.29	0.34
Observations	82	82	61	61	61

Note: Standard errors in parentheses. The unit of observation is metropolitan area. The dependent variable is the residual black-white gap in annual hours worked, after controlling for individual demographic characteristics (age, age squared, education, occupation) and year of survey. The superscripts*,**, and***denote significance at the 10, 5, and 1 percent levels respectively.

Table 8: Racial Prejudice and Other Inter-group Differences: Female-Male Gaps and Hispanic-White Gaps

	Female-Male		Hispanic-White	
	Ln(Annual Income)	Ln(Hourly Wage)	Ln(Annual Income)	Ln(Hourly Wage)
PrejudiceIndex	-0.0068***	-0.0069***	-0.0055***	-0.0041***
	(0.001)	(0.0009)	(0.0013)	(0.0010)
Percent Hispanic			-0.0009	-0.0018**
			(0.0012)	(0.0007)
Demographic Controls	Yes	Yes	Yes	Yes
Year Controls	Yes	Yes	Yes	Yes
Google Controls	Yes	Yes	Yes	Yes
Region Fixed Effect	Yes	Yes	Yes	Yes
R-squared	0.64	0.762	0.394	0.559
Observations	61	61	61	61

Note: Standard errors in parentheses. The unit of observation is metropolitan area. The dependent variable is the residual gap specified at the top of each column (after controlling for individual demographic characteristics and year of survey). The superscripts*,**, and***denote significance at the 10, 5, and 1 percent levels respectively.

Table 9: Racial Prejudice and Inequality

Dependent Variable: Difference in Natural Log of 90th and 10th Percentiles of Annual Income

	(1)	(2)
PrejudiceIndex	0.0019	-0.0014
	(0.0018)	(0.0023)
Percent Black		-0.0007
		(0.0048)
Google Controls	No	Yes
Region Fixed Effect	No	Yes
R-squared	0.027	0.356
Observations	61	61

Note: Standard errors in parentheses. The unit of observation is metropolitan area. The superscripts*,**, and***denote significance at the 10, 5, and 1 percent levels respectively.

Appendix A

Table A1: Full List of Google Metropolitan Areas with their Average Prejudice Index	
Google Metro	*PrejudiceIndex*
Albany-Schenectady-Troy	70.9
Albuquerque	51
Atlanta	63
Austin	48.9
Baltimore	64.5
Baton Rouge	84.5
Birmingham	73.7
Boston	52.9
Buffalo	76.2
Charlotte	67.6
Chicago	61.1
Cincinnati	65.6
Cleveland	79.5
Columbia	78.5
Columbus, OH	62.8
Dallas-Fort Worth	59.5
Dayton	72.6
Denver	40.9
Des Moines	66
Detroit	72.1
Flint	89.2
Fresno	69
Grand Rapids	62
Green Bay-Appleton	74.6
Greensboro	65.7
Greenville-Spartenburg	74.1
Harrisburg-Lancaster-Lebanon-York	66.5
Hartford	65.6
Honolulu	40.4
Houston	56.3
Indianapolis	59.3
Jackson, MS	74.9
Jacksonville	67.5
Johnstown-Altoona	98.8
Kansas City	60.2
La Crosse-Eau Claire	83.5
Las Vegas	65.2

Lexington	82.9
Little Rock-Pine Bluff	75.1
Los Angeles	56.7
Louisville	79.7
Madison	51
Memphis	70.7
Miami	50.8
Milwaukee	56
Minneapolis-St Paul	40.6
Mobile	74.1
Nashville	60.6
New Orleans	77.1
New York	64.9
Norfolk-Portsmouth	59.9
Oklahoma City	65
Omaha	54.3
Orlando	67.3
Philadelphia	77.9
Phoenix	48.6
Pittsburgh	78.4
Portland	45.3
Portland-Auburn	59.5
Providence	63.9
Raleigh-Durham	62.2
Richmond-Petersburg	59
Roanoke-Lynchburg	84.3
Rochester	72.6
Sacramento	51.6
Salt Lake City	31.2
San Antonio	48.3
San Diego	49.2
San Francisco	27.9
Santa Barbara	69
Seattle-Tacoma	50.6
Spokane	62.5
Springfield-Holyoke	83
St Louis	69.3
Syracuse	75.1
Tampa	66.7
Toledo	86.6
Tucson	58.1
Waco-Temple-Bryan	76.9
Washington	42.3

West Palm Beach	61.2
Wilkes Barre-Scranton	99.3

Table A2: First Stage Regressions for the Preferred Specification

	Annual Income	Ln(Annual Income)	Ln(Hourly Wage)	Annual Hours
Albany-Schenectady-Troy*Black	-13,419.37***	-0.4249***	-0.2096***	-303.30***
	(784.07)	(0.0151)	(0.0156)	(12.12)
Atlanta*Black	-11,951.25***	-0.2677***	-0.1905***	-150.66***
	(946.11)	(0.0152)	(0.0158)	(11.71)
Austin*Black	-13,982.12***	-0.4080***	-0.2735***	-173.14***
	(941.58)	(0.0143)	(0.0154)	(11.27)
Baltimore*Black	-9,726.87***	-0.1793***	-0.1135***	-169.38***
	(869.02)	(0.0150)	(0.0156)	(12.30)
Baton Rouge*Black	-7,670.33***	-0.1830***	-0.1901***	-60.41***
	(713.30)	(0.0139)	(0.0146)	(11.28)
Birmingham*Black	-11,166.70***	-0.3167***	-0.2404***	-142.81***
	(752.41)	(0.0144)	(0.0151)	(12.64)
Boston*Black	-11,924.53***	-0.2609***	-0.0989***	-231.19***
	(946.94)	(0.0156)	(0.0160)	(11.00)
Buffalo*Black	-13,929.57***	-0.4890***	-0.2343***	-385.82***
	(724.59)	(0.0154)	(0.0157)	(14.24)
Charlotte*Black	-12,197.26***	-0.3383***	-0.2483***	-159.32***
	(859.06)	(0.0148)	(0.0153)	(11.21)
Chicago*Black	-10,774.70***	-0.2948***	-0.1377***	-289.08***
	(801.91)	(0.0154)	(0.0159)	(14.17)
Cincinnati*Black	-14,008.33***	-0.4518***	-0.2352***	-308.79***
	(812.25)	(0.0155)	(0.0157)	(12.16)
Cleveland*Black	-13,458.97***	-0.4332***	-0.2613***	-303.83***
	(732.82)	(0.0150)	(0.0154)	(12.53)
Columbia*Black	-11,559.17***	-0.3504***	-0.2797***	-91.86***
	(748.91)	(0.0139)	(0.0143)	(12.72)
Dallas-Fort Worth*Black	-11,203.11***	-0.2655***	-0.2002***	-134.76***
	(899.66)	(0.0149)	(0.0156)	(12.16)
Denver*Black	-14,230.27***	-0.3538***	-0.2231***	-190.09***
	(1,019.03)	(0.0156)	(0.0161)	(10.93)
Detroit*Black	-11,859.27***	-0.3858***	-0.1782***	-375.45***
	(735.63)	(0.0152)	(0.0158)	(14.65)
Flint*Black	-13,544.66***	-0.5942***	-0.2874***	-465.84***
	(622.36)	(0.0147)	(0.0153)	(17.31)
Grand Rapids*Black	-14,136.25***	-0.5735***	-0.2851***	-446.81***
	(671.74)	(0.0145)	(0.0149)	(14.43)

Greensboro*Black	-12,963.43***	-0.3780***	-0.2773***	-187.02***
	(777.09)	(0.0145)	(0.0151)	(12.38)
Greenville-Spartenburg*Black	-11,216.84***	-0.3368***	-0.2577***	-202.79***
	(694.61)	(0.0141)	(0.0146)	(12.81)
Harrisburg-Lancaster-Lebanon-York*Black	-10,742.71***	-0.3269***	-0.2077***	-169.73***
	(768.20)	(0.0141)	(0.0147)	(11.46)
Hartford*Black	-10,346.52***	-0.2994***	-0.1184***	-254.04***
	(864.55)	(0.0150)	(0.0155)	(11.15)
Honolulu*Black	9,409.15***	0.0400***	-0.0334***	882.67***
	(739.00)	(0.0133)	(0.0093)	(26.01)
Houston*Black	-10,428.72***	-0.2346***	-0.1882***	-122.77***
	(862.28)	(0.0148)	(0.0156)	(12.58)
Indianapolis*Black	-12,535.39***	-0.3529***	-0.2081***	-255.18***
	(842.97)	(0.0150)	(0.0155)	(11.31)
Jacksonville*Black	-9,894.68***	-0.2303***	-0.1920***	-94.50***
	(751.89)	(0.0140)	(0.0145)	(12.61)
Kansas City*Black	-12,157.94***	-0.3115***	-0.2355***	-200.61***
	(822.79)	(0.0148)	(0.0154)	(11.73)
Las Vegas*Black	-7,543.74***	-0.0847***	-0.0754***	-117.39***
	(841.46)	(0.0160)	(0.0161)	(11.64)
Los Angeles*Black	-9,730.21***	-0.2128***	-0.0873***	-255.25***
	(901.23)	(0.0152)	(0.0161)	(13.55)
Louisville*Black	-13,574.55***	-0.4265***	-0.2688***	-237.97***
	(803.60)	(0.0153)	(0.0156)	(12.40)
Madison*Black	-14,058.28***	-0.5670***	-0.3294***	-321.74***
	(930.52)	(0.0148)	(0.0153)	(11.41)
Memphis*Black	-11,219.30***	-0.3046***	-0.2253***	-194.95***
	(726.25)	(0.0145)	(0.0150)	(12.33)
Miami*Black	-11,728.84***	-0.2680***	-0.2204***	-163.72***
	(783.93)	(0.0155)	(0.0157)	(11.95)
Milwaukee*Black	-13,185.76***	-0.4580***	-0.2386***	-362.40***
	(687.29)	(0.0142)	(0.0148)	(13.24)
Minneapolis-St Paul*Black	-13,638.04***	-0.3797***	-0.2067***	-282.09***
	(910.27)	(0.0157)	(0.0160)	(11.71)
Mobile*Black	-9,916.46***	-0.3520***	-0.2652***	-167.85***
	(631.16)	(0.0138)	(0.0142)	(14.92)
Nashville*Black	-11,090.95***	-0.3288***	-0.2457***	-105.01***
	(864.00)	(0.0145)	(0.0151)	(11.85)
New Orleans*Black	-8,534.20***	-0.2286***	-0.2120***	-88.93***
	(663.84)	(0.0145)	(0.0149)	(12.94)
New York*Black	-8,359.95***	-0.1571***	-0.0652***	-187.31***
	(877.39)	(0.0161)	(0.0163)	(12.56)

Norfolk-Portsmouth*Black	-6,454.32***	-0.1681***	-0.1524***	88.45***
	(737.89)	(0.0127)	(0.0131)	(13.47)
Oklahoma City*Black	-13,758.80***	-0.4808***	-0.3451***	-153.41***
	(818.84)	(0.0145)	(0.0152)	(12.55)
Orlando*Black	-11,777.64***	-0.3110***	-0.2397***	-188.85***
	(773.51)	(0.0149)	(0.0155)	(11.97)
Philadelphia*Black	-10,329.58***	-0.2398***	-0.1222***	-250.63***
	(791.55)	(0.0154)	(0.0158)	(13.00)
Phoenix*Black	-11,309.54***	-0.2339***	-0.1685***	-163.93***
	(921.67)	(0.0146)	(0.0156)	(12.98)
Pittsburgh*Black	-13,654.86***	-0.4898***	-0.2834***	-303.87***
	(789.86)	(0.0162)	(0.0163)	(12.17)
Portland*Black	-13,754.30***	-0.3142***	-0.1942***	-198.11***
	(953.61)	(0.0155)	(0.0160)	(12.27)
Providence*Black	-9,915.39***	-0.2364***	-0.1740***	-133.88***
	(828.03)	(0.0142)	(0.0147)	(12.57)
Raleigh-Durham*Black	-12,591.74***	-0.4094***	-0.2994***	-79.94***
	(853.64)	(0.0142)	(0.0146)	(12.61)
Richmond-Petersburg*Black	-10,420.15***	-0.2601***	-0.1832***	-154.79***
	(800.41)	(0.0145)	(0.0150)	(11.44)
Rochester*Black	-12,799.08***	-0.4258***	-0.1575***	-369.13***
	(711.03)	(0.0149)	(0.0151)	(13.52)
Sacramento*Black	-9,858.11***	-0.2365***	-0.0595***	-337.87***
	(784.93)	(0.0146)	(0.0156)	(14.13)
San Antonio*Black	-13,604.40***	-0.4198***	-0.3104***	-69.47***
	(917.24)	(0.0144)	(0.0147)	(12.93)
San Diego*Black	-4,499.06***	-0.1315***	-0.1068***	111.31***
	(829.59)	(0.0127)	(0.0128)	(17.07)
San Francisco*Black	-9,682.37***	-0.2497***	-0.0534***	-312.47***
	(932.31)	(0.0156)	(0.0163)	(13.12)
Seattle-Tacoma*Black	-9,569.60***	-0.2764***	-0.1445***	-91.35***
	(884.90)	(0.0144)	(0.0148)	(12.83)
St Louis*Black	-12,737.81***	-0.3566***	-0.2082***	-261.33***
	(793.92)	(0.0160)	(0.0161)	(12.54)
Syracuse*Black	-14,196.50***	-0.5027***	-0.2446***	-410.04***
	(689.66)	(0.0147)	(0.0151)	(14.57)
Tampa*Black	-11,105.89***	-0.2864***	-0.2210***	-162.82***
	(762.02)	(0.0146)	(0.0151)	(12.63)
Washington*Black	-7,060.29***	-0.0859***	-0.0225	-109.85***
	(1,126.55)	(0.0160)	(0.0164)	(10.29)
West Palm Beach*Black	-11,673.50***	-0.2843***	-0.2097***	-163.00***
	(760.21)	(0.0154)	(0.0154)	(11.32)
Wilkes Barre-Scranton*Black	-12,660.01***	-0.5529***	-0.2190***	-496.53***

	(723.38)	(0.0142)	(0.0150)	(13.59)
Age	4,882.01***	0.2108***	0.0902***	127.57***
	(202.39)	(0.0039)	(0.0017)	(1.91)
Age2	-52.53***	-0.0022***	-0.0009***	-1.52***
	(2.29)	(0.0000)	(0.0000)	(0.02)
(Education 2) Nursery school to grade 4	-1,219.22**	0.0686*	-0.0487**	187.18***
	(564.07)	(0.0385)	(0.0192)	(18.75)
(Education 3) Grade 5, 6, 7, or 8	413.97	0.1411***	0.0070	172.71***
	(390.20)	(0.0282)	(0.0155)	(12.93)
(Education 4) Grade 9	2,754.40***	0.1502***	0.0694***	95.99***
	(412.09)	(0.0199)	(0.0144)	(12.34)
(Education 5) Grade 10	4,682.68***	0.1213***	0.1251***	22.65
	(465.72)	(0.0188)	(0.0123)	(14.27)
(Education 6) Grade 11	8,826.38***	0.0304*	0.1645***	28.01*
	(611.46)	(0.0156)	(0.0122)	(14.32)
(Education 7) Grade 12	9,152.46***	0.3869***	0.2738***	233.40***
	(442.82)	(0.0207)	(0.0142)	(24.50)
(Education 8) 1 year of college	12,608.19***	0.4637***	0.3651***	259.76***
	(464.22)	(0.0170)	(0.0142)	(25.41)
(Education 9) 2 years of college	13,803.49***	0.5911***	0.4135***	353.14***
	(489.08)	(0.0200)	(0.0147)	(28.26)
(Education 10) 4 years of college	32,308.58***	0.8407***	0.6269***	419.00***
	(1,237.55)	(0.0224)	(0.0185)	(25.99)
(Education 11) 5+ years of college	58,327.86***	1.0167***	0.7986***	502.66***
	(2,418.09)	(0.0245)	(0.0201)	(28.44)
(2) Technical, Sales and Administrative	-16,936.81***	-0.2646***	-0.2126***	-124.70***
	(427.64)	(0.0066)	(0.0052)	(2.83)
(3) Service	-28,987.57***	-0.5726***	-0.4215***	-252.92***
	(509.24)	(0.0202)	(0.0112)	(11.11)
(4) Farming, Forestry, and Fishing	-33,358.76***	-0.7073***	-0.5350***	-257.86***
	(856.78)	(0.0133)	(0.0067)	(13.96)
(5) Precision production, Craft, and Repair	-22,779.46***	-0.2107***	-0.1903***	-126.41***
	(743.94)	(0.0108)	(0.0102)	(7.15)
(6) Operators, Fabricators, and Laborers	-28,721.95***	-0.4510***	-0.3613***	-194.58***
	(850.35)	(0.0085)	(0.0074)	(7.74)
(7) Non-occupational responses	-48,758.09***	0.1805***	-0.2280***	-1,426.88***
	(1,440.29)	(0.0290)	(0.0212)	(51.60)

Year fixed effect?	Yes	Yes	Yes	Yes
R-squared	0.28	0.39	0.33	0.35
N	2,542,114	2,129,194	2,129,194	2,542,260

* p<0.1; ** p<0.05; *** p<0.01

Note: Each column represents a different dependent variable in the specification of equation (1). The omitted education category is No education or N/A, and the omitted occupation category is Managerial and Professional. Standard errors in parentheses are clustered at the metropolitan area. The superscripts *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively.

Results by Education Category

In Tables A3, A4, and A5, we examine whether the link between prejudicial attitudes and black-white gaps in annual income, hourly wage, and annual hours worked, respectively, differs by education level. Three education categories are considered − no college, some college, and more than 4 years of college. As is the case with the results for the entire sample, $PrejudiceIndex$ is not a significant predictor of the gaps in annual hours worked in any education category (Table A5). However, with regard to the other two labor market outcomes, prejudicial attitudes appear to play a larger role for less educated workers.

A higher $PrejudiceIndex$ is associated with significantly higher black-white gaps in annual income across all education categories. However, the magnitude of the association is decreasing in educational attainment. A prejudicial search rate one standard deviation higher (i.e. about 14 index points higher) is associated with a 6.3 percentage point higher residual annual income gap among those with no college, but only a 4.8 percentage point higher residual annual income gap among those with ≥ 4 years of college (Table A3).

A higher $PrejudiceIndex$ is also associated with a significantly higher black-white gap in hourly wage across all education categories (Table A4). Although no clear pattern is evident in the coefficients on $PrejudiceIndex$ across education categories, a pattern is evident when the associations represented by these coefficients are compared against the nationwide average hourly wage gaps by education category. For the category of ≥ 4 years of college, a one standard deviation higher prejudicial search rate is associated with a 21% higher residual hourly wage gap, relative to the nationwide average.[37] In contrast, for the categories of some college and no college, the corresponding numbers are substantially higher at over 40% of the average nationwide gap.

[37] The average nationwide residual hourly wage gap is approximately 23% (Table 3).

Similar to our earlier results, we find that declines in employment from 2000 to 2010 are associated with larger gaps. Interestingly, this relationship tends to be more pronounced among the lower educated groups compared to the ≥ 4 years of college group. This supports the idea that low-skilled black individuals are more affected by economic downturns, possibly because they cannot afford to relocate to more economically thriving areas. Indeed, declining employment appears to be the primary factor associated with a larger gap in annual hours worked for less educated workers; the $PrejudiceIndex$ variable is not a significant predictor of this gap among workers in any education category.

Table A3: Racial Prejudice and the Black-White Gap by Education Level in Ln(Annual Income)

Dependent Variable: Black-White Gap in Ln(Annual Income)

	No College	Some College	4 Yr. College
PrejudiceIndex	-0.0045**	-0.0044***	-0.0034**
	(0.0015)	(0.0011)	(0.0013)
Percent Black	0.0145***	0.0061*	0.007*
	(0.0044)	(0.0030)	(0.0035)
Percent Change in Employment	0.0043**	0.0058***	0.0029**
	(0.0020)	(0.0014)	(0.0012)
Demographic Controls	Yes	Yes	Yes
Year Controls	Yes	Yes	Yes
Google Controls	Yes	Yes	Yes
Region Fixed Effect	Yes	Yes	Yes
R-squared	0.58	0.65	0.55
Observations	61	61	61

Note: Standard errors in parentheses. The superscripts *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively.

Table A4: Racial Prejudice and the Black-White Gap by Education Level in Ln(Hourly Wage)

Dependent Variable: Black-White Gap in Ln(Hourly Wage)

	No College	Some College	4 Yr. College
PrejudiceIndex	-0.0039***	-0.0048***	-0.0036***
	(0.0008)	(0.0009)	(0.0012)
Percent Black	0.0086***	0.0039	0.0048
	(0.0032)	(0.0026)	(0.0032)
Percent Change in Employment	0.0000	0.0017	0.0018*
	(0.0011)	(0.0011)	(0.0010)
Demographic Controls	Yes	Yes	Yes
Year Controls	Yes	Yes	Yes
Google Controls	Yes	Yes	Yes
Region Fixed Effect	Yes	Yes	Yes
R-squared	0.7	0.67	0.62
Observations	61	61	61

Note: Standard errors in parentheses. The superscripts *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively.

Table A5:A16 Racial Prejudice and the Black-White Gap by Education Level in Annual Hours Worked

Dependent Variable: Black-White Gap in Annual Hours Worked

	No College	Some College	4 Yr. College
Prejudice Index	-1.216	-0.182	0.375
	(1.677)	(1.395)	(1.002)
Percent Black	5.908	3.383	3.840
	(3.728)	(3.303)	(2.736)
Percent Change in Employment	4.822**	6.408***	1.486
	(2.209)	(1.405)	(1.123)
Demographic Controls	Yes	Yes	Yes
Year Controls	Yes	Yes	Yes
Google Controls	Yes	Yes	Yes
Region Fixed Effect	Yes	Yes	Yes
R-squared	0.24	0.48	0.34
Observations	61	61	61

Note: Standard errors in parentheses. The superscripts *, **, and *** denote significance at the 10, 5, and 1 percent levels respectively.

Appendix B

Description of Data Sources

- *Google Trends*: Data were downloaded from www.google.com/trends. Search rates by U.S. metropolitan area were queried for the words “nigger(s)”, “nigga(s)”, “African-American”, and “fuck”, over the period 2004-2010. Google Trends reports the search rates in the form of a index. The procedure for obtaining the data is described in Section 2.2.
- *Individual-Level Data*: The following variables were obtained from the American Community Surveys (2006-2011) via the Integrated Public Use Microdata Series (IPUMS-USA). \begin_inset Separator latexpar\end_inset
    - *Demographic Variables*: AGE (age in years), EDUC (education level categories detailed in footnote 22), RACE (to identify blacks), and HISPAN (Hispanic origin).
    - *Geographic Variables*: COUNTY (county of residence) and REGION (Census region of residence).
    - *Labor Variables*: OCC1990 (occupation categories detailed in footnote 22), INCWAGE (wage and salary income), WKSWRK1 (weeks worked last year), UHRSWRK (usual hours worked per week). Construction of our outcome variables from these variables is explained in Section 3.
- *Neilsen Media Markets*: The metropolitan areas in Google Trends correspond to Neilsen’s media markets. We grouped each individual into a media market based on the county of residence. Data on the counties contained in each media market was obtained from Gentzkow and Shapiro (2008). See http://www.icpsr.umich.edu/icpsrweb/DSDR/studies/22720.

County Population Figures (2010): Population of counties in 2010 was obtained from http://seer.cancer.gov/popdata/download.html.

Effects of a Sector-Specific Recession on Highly Skilled Graduates: A Case Study of the Dot-Com Recession

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1 Introduction

Macroeconomic conditions when graduating from college not only have short-term consequences, but recent empirical work has reported that these initial conditions can have persistent long-term effects on labor market outcomes such as earnings and job placement.[38] However, recent recessions in the United States and other countries have been associated with large negative demand shocks to specific industries, such as finance and real estate in the Great Recession and information technology in the recession of the early 2000s. Such recessions can have highly disproportionate impacts on recent college graduates in the affected industries. Considering that many of the science and engineering majors are geared for careers and occupations in specific sectors, we would expect sector-specific demand shocks to play a crucial role in the labor market of these high-skilled graduates.

In this regard, the recession of the early 2000s was unique in that a major contributor was the busting of the Dot-Com bubble – herein, the Dot-Com Recession. This recession was widely attributed to the downturn in the information technology (IT) industry and it had a particularly severe impact on its labor market compared to other industries. We would, therefore, expect the impact of the recession to be more severe on majors that have a higher propensity to work in this sector. In this paper, I study the short- and long-term impact of the

[38] See for example: Kahn, 2010 for the U.S.; Oreopoulos, von Wachter, and Heisz, 2012 for Canada; Liu, Salvanes, and Sørensen, 2012 for Norway; Kwon, Milgrom, and Hwang, 2010 for Sweden.

Dot-Com Recession on the labor market outcomes of college graduates and how that impact varies across majors with different levels of concentration in the IT industry.

Previous empirical work that studies college graduates has found scarring effects of graduating in a bad economy of around 7-10 years, with a faster recovery for high-earning majors (Oreopoulos, von Wachter, and Heisz, 2012- herein OWH; Altonji, Kahn, and Speer 2013- herein AKS).[39] Furthermore, results in AKS suggest that majors that are concentrated in certain occupations (i.e. Architecture or Computer Science) are *more* sensitive to downturns in the economy given their restricted job options. In such a case, we would expect the labor market outcomes to be even more dependent on the severity of the downturn in a specific industry related to the major. The Dot-Com Recession provides a natural case study to analyze the overall impact of the recession combined with the differential impact across majors with a higher propensity to be in the IT industry.

In my analysis, I restrict my sample to the students who graduated just 24 months before (boom cohort) and 24 months after the crash (bust cohort), and examine the long-term differences in labor market outcomes for these two groups. Interestingly, the supply response, in terms of the composition of majors, to the recession, was minimal in the short-run. For example, the supply of new computer science (CS) degree holders- who have the highest propensity to be employed in the IT sector- doesn't reduce for almost four years as students are locked into their majors since freshman or sophomore year.

I use the Scientists and Engineers Statistical Data System (SESTAT), which provides data on a cross-sectional representative sample of science and engineering college graduates

[39] Scarring effect refers to the persistent gap in labor market outcomes such as earnings for those individuals that graduated in a downturn compared to those that didn't graduate in a bad market.

almost every two years.[40] My results indicate that scientists and engineers graduating in the bust had on average 13 percent lower earnings during the first year after graduation compared to those graduating during the boom and this loss is higher for majors with a higher level of concentration in the IT industry. This differential effect is particularly strong for two major fields that have the highest share of students working in the IT industry- computer science and electrical and computer engineering (ECE). Even though there is narrowing of the earnings gap over 7 years (comparable to AKS), these IT majors (CS and ECE) have a persistent earnings gap even up to ten years after graduation, relative to the boom graduates.

I explore mechanisms that could possibly explain the persistent earnings gap for these IT majors. Unlike AKS, I do not find any significant differences in hours worked between the boom and bust cohorts to explain the gap in earnings. However, I do find a substantial drop in hourly wage for the bust cohort that persists over the entire experience profile. Specifically, the IT majors graduating during the bust have an initial disadvantage of 8.4 percent lower hourly wages in the first year after graduation that firmly persists over the ten year period. Additionally, these graduates are slightly less likely to be in a supervisory position (comparable to Kwon et al., 2010) and are also slightly more likely to stay in a job. Since at least a third of the earnings growth in early career is associated with job mobility (Topel and Ward, 1992), higher tenure at jobs could prove to be detrimental. Furthermore, IT majors graduating in the bust are more likely to move out of better quality firms (measured by size of firms). This is interesting because job mobility into better quality firms has been found to be a key contributor in reducing the earnings gap over the long run (OWH, 2012). Lastly, my results suggest that some of the IT bust graduates move out of the IT occupations

[40] The survey years include: 1993, 1995, 1997, 1999, 2001, 2003, 2006, 2008, and 2010.

all-together over the experience profile, compared to their boom cohorts. This supports earlier work which shows that industry or occupation mismatch are almost always long-term (Neal, 1999) and create persistent earnings differences (Liu et al., 2012).

The strongest of these mechanisms, the reduction in hourly wage gap, suggests that perhaps the biggest contributor is the overall IT labor market that never quite recovered from the bust in the bubble. The macroeconomic trends in employment suggest the IT sector never quite reached the peak after 2000. That, combined with the large supply of new CS graduates that continued to enter the market due to the delayed supply response would have created a particularly loose labor market. Indeed, the starting hourly wage for new graduates dropped to its pre-boom level after the bust and never quite reached the peak observed during the boom years. It comes as no surprise then that the IT graduates from the bust cohort had a higher propensity to leave the IT occupation all-together.

The analysis conducted in this paper contributes to the existing literature and ongoing research in two main ways. First, the differential effects of the recession on IT majors expands on the recent work by AKS and suggests a greater role for industry-specific demand shocks that could explain labor market outcomes of many science and engineering majors geared towards specific occupations and industries. Secondly, the persistence of earnings differences due to the continued labor market looseness well after the bust gives credence to the notion that not only initial conditions matter, but the subsequent industry conditions play a crucial role in determining lifetime earnings and job placement.

The rest of the paper is structured as follows. In section 2, I provide a brief background on the Dot-Com Recession and the overall labor market. In section 3, I empirically analyze the long-term effects of graduating in the bust on earnings for all majors.

In section 4, I explore various possible mechanisms that could explain persistent long-term differences in labor market for IT majors. Lastly, in section 5, I conclude the paper.

2 The Dot-Com Recession and the IT industry

The past two decades has seen an unprecedented change in US productivity which has largely been attributed to the adoption of telecommunication and information technology (Oliner et al. 2007). During the 1990s, the use of computers became pervasive throughout US and internet connectivity became readily available. This led to a massive growth and investment in internet-based commerce companies. This era of investment- the Dot-Com bubble- is reflected in the NASDAQ Composite Index (see Figure 1), which saw its value increase five-fold during the boom time, peaking in March 10, 2000 at which point the bubble burst and dropped almost 10% within a few days.[41] After a small recovery, based on the persistent high hopes of the economy, the market crashed substantially during the following year leading to the lowest point since the boom in October 2002. This economic downturn was not just limited to the internet companies, but it had an overall impact on the economy. According to the US Labor Department, 1.735 million net jobs were shed in 2001. This period is officially declared a recession and commonly referred to as the recession of the early 2000s or the Dot-Com recession.[42]

To get a better understanding of the impact of the recession on the labor market, I explore the trends in employment in five large sectors- Health, Manufacturing, Construction,

[41] The NASDAQ Composite is weighted using the market capitalization method, where larger companies are given more weight. Even though the tech companies make up less than 50% of the companies listed in the NASDAQ Composite, the major part of the variation in the late 1990s is attributed to the tech companies because of their market capitalization. For example, Apple, being the most valuable company, alone makes up around 12.5% of the index value (Plaehn, n.d.).

[42] According to NBER, the recession (the contraction period) started in March 2001 and ended in November 2001.

Financial Services and the IT sector from 1990-2010 (see figure 2).[43] Employment in the IT sector corresponds directly with the fluctuations in the NASDAQ Composite- increasing during the 90s, while declining proportionally 2000 onwards. Manufacturing is the only other industry that experienced a considerable loss in employment during the recession. However, the disproportionate impact on the IT industry makes this phenomenon somewhat of a "natural experiment" to study the college students most likely to work in the IT sector.

3. Long-term effects of graduating in the Dot-Com Recession

3.1 Data

The Scientists and Engineers Statistical Data System (SESTAT) provided by the National Science Foundation (NSF) consists of data from three surveys: The National Survey of College Graduates (NSCG), the National Survey of Recent College Graduates (NSRCG), and the Survey of Doctorate Recipients (SDR). The combined data contains a rich set of information on labor market, demographic, and educational characteristics of individuals with at least a bachelor's degree. The focus of the surveys is mainly on the Science and Engineering (SE) fields, including social sciences, and the fields generally not represented are business, arts, and humanities (see table 1).

Each of the three surveys have different sampling methods based on their respective population, but the questionnaire itself is fairly consistent among the three. The NSCG consists of a sample of individuals from the Census that are reported to have a bachelor's degree and are under the age of 76. The NSRCG and SDR consist of only those individuals that recently graduated (usually within 2 years) from college with a bachelor's or master's

[43] Data on employment is collected from the Bureau of Labor Statistics- Current Employment Statistics. The IT sector reported in this graph is simply the Information sector defined by the North American Industry Classification System (NAICS), which consists of: Software Packaging, Telecommunication, Data Processing, and also Newspaper and Broadcasting.

degree (NSRCG), or a doctorate degree (SDR).[44] Together, these three surveys represent a cross-section of all scientists and engineers for each of the survey years, and this is reflected in the survey weights for each individual in each survey.[45] In this study, I use all the available surveys from the last two decades: 1993, 1995, 1997, 1999, 2001, 2003, 2006, 2008, and 2010. All the three surveys combined in SESTAT are represented in each of the survey years with the exception of 2001 when only the NSRCG was conducted.[46]

3.2 Identification Issues

In figure 3, I show the national trends in the bachelor's degrees by major-field as a share of the total degrees awarded each year.[47] To be parsimonious on space, these majors are chosen to reflect the SE fields represented in table 1. The most noticeable trend line is the one related to computer science, which has nearly a 300 percent increase in the number of degrees conferred during the late nineties and doubling in the share of total degrees. This increase is followed by a commensurate decrease in degrees conferred towards what seems to be the "normal" trend. Interestingly, biology also experiences large fluctuations in the supply of new degrees as a share of total degrees.

The trend in CS degrees conferred does seem to indicate that interest in the major is tied to the bubble, but with a slight delay. In fact, the share of degrees conferred in CS is still

[44] See www.nsf.gov/statistics/srvyrecentgrads for further details on sampling design and the construction of sample weights.
[45] The weights assigned to NSRCG and SDR individuals are much smaller than the weights for individuals in the NSCG such that the combination of the three surveys gives a representative sample of college-educated individuals in the US.
[46] The lack of other surveys in 2001 has no impact on the empirical study as the main analysis is restricted to only those students graduating between 1998 and 2002. This means that only the NSRCG from 2001 would be useful as it gives a representative sample of recent bachelor's graduates, while the other surveys (if conducted) would give a sample of students graduating before 1998.
[47] For data on the total degrees conferred in the US, I use the Integrated Postsecondary Education Data System (IPEDS) from 1991-2010. This dataset contains information on all degrees conferred from almost all institutions in the U.S. For the current analysis, I focus only on bachelor degrees conferred by four year colleges and universities.

growing after the bust all the way till 2003. The best explanation for this delay comes from Richard Freeman's Cobweb model (See Freeman 1971, 1975a, 1975b, 1976a, 1976b, 1977), where the hypothesis is that students make decisions on college majors based on labor market conditions at the time of entering college and these decisions are reflected four years later in the number of graduates in each major. This stickiness may be further exaggerated for specialized majors- such as Architecture and Computer Science- where switching to another major would be quite difficult. Figure 4 shows the percent of students at four year colleges intending to major in CS in their freshman year along with the share of degrees conferred nationwide.[48] As the cobweb model suggests, the trend in degrees conferred is a shadow of the entrants four years prior. Similarly, it may be the case that the quality of students is changing in response to the bubble (i.e. fewer IT majors from elite colleges).

It is difficult to gauge exactly the reasons for the delay, but the most obvious reason is that once committed to a major in the first or second year, there may be financial or institutional restrictions to change the major. I take advantage of this delay in supply response to *minimize* any selection into majors and to compare students experiencing different states of the economy who are, ceteris paribus, the same. I use the crash in the NASDAQ on March 2000 as my main source of variation and consider only those students graduating within 24 months before and after this month.[49] The stock market is regarded as one of the best leading variables of recessions with a one to three quarter horizon (Estrella and Mishkin, 1998). Additionally, the stock market allows me to measure the economic downturn avoiding the general equilibrium effects that are associated with the unemployment

[48] I use The American Freshman: National Norms (1989-2011) data provided by Higher Education Research Institute. This is the largest freshmen survey done in the U.S. with a representative sample of over 200,000 students in each year.
[49] Defining March 2000 as month zero, the complete sample consists of 49 months from March 1998 to March 2002.

rate. Accordingly, my analysis consists of the six survey years where I observe students graduating in this 48 month window: 1999, 2001, 2003, 2006, 2008, and 2010.

3.3 Methodology

In specification 1, I estimate the effect of graduating in the bust period on log earnings, combined with the differential effect on the level of concentration in the IT industry for each major. To simplify the language, I will refer to those graduating before the bust and after the bust as the boom cohort and bust cohort, respectively. I measure concentration of majors in the IT industry using the 1997 SESTAT survey data on industry of employment, the earliest year during which this information is available (see table 1).[50] Using data from 1997, the early part of the boom period, allows me to address any temporary shifts in employment into the IT industry that may distort the true likelihood of a student from a particular major going into the industry.[51]

Specification 1:

$$y_{imct} = \alpha + \beta_1 Bust_c + \beta_2 Bust * exp_{it} + \beta_3 ITshare_m + \beta_4 ITshare * exp_{imt} + \beta_5 ITshare * Bust_{mc} + \beta_6 ITshare * Bust * exp_{imt} + \beta_7 exp_{it} + \theta \boldsymbol{X}_i + T_t + S_s + u_{imct}$$

For individual *i* that graduated with major *m*, in cohort *c* (1 if Bust), y_{imct}, measures the outcome variable, observed in year *t*. I define potential experience as the number of years since graduation minus one, observed in year *t*. This definition of experience allows me to address any endogeneity related to actual labor market experience influenced by labor market conditions. In addition to the linear definition of experience in specification 1, I explore a

[50] This measure is derived from the SESTAT variable EMBUS, which asks individuals to categorize their employer's main business into one of fourteen industries. These industries to a great extent reflect the NAICS two-digit industry classifications of which information technology is a distinct category.
[51] A glance at the industry data from 1999, the peak of the bubble, suggests that the proportion of majors working in the IT industry has not changed much from 1997.

dummy variable representation of high and low experience (variable HiExp equals 1 if 6 or more years of experience). I also control for the following individual characteristics in $\boldsymbol{X}_i$: six dummies for mother's education, female dummy, minority dummy, and ten dummies for birth regions.[52] The combination of these background variables should exogenously control for most of the heterogeneity associated with individual ability and geographic location.[53] Additionally, I include dummies for survey years, *T*, to account for any contemporaneous effects, and survey type (*S*) to account for any differences in the sampling of each of the three surveys. Lastly, I use the SESTAT integrated survey weights provided for each observation.

The *Bust* variable is a simple dummy variable for the bust cohort and the coefficient β_1 measures the impact of graduating during the bust in the initial year for the major with no presence in the IT industry. The persistence of this impact for this type of major over the experience profile is estimated by β_2. *ITshare* represents the actual share of students in major *m* in the IT industry (see table 1) in 1997. Alternatively, I also provide results on the same specification using major fixed effects instead of *ITshare*. Additionally, I allow for separate experience profile with the inclusion of the interaction term *ITshare*exp*. Therefore, β_5 and β_6 represent the differential immediate and long-term impact, respectively, of graduating in the bust across the share of concentration in the IT industry for each major.

3.4 Results

In table 2, I present some summary statistics of the dependent variables to be studied in this paper. I first explore the effect of the bust on log yearly income (or earnings) in table 3. In

[52] Mother's education categories include less than high school, high school or equivalent, some college, Bachelor's degree, Post-graduate degree, and Not applicable/missing. The inclusion of the last category does not change any of the results. Minority includes non-White and non-Asians. The 10 regions correspond to the nine census regions plus one for being foreign-born.
[53] The advantage of using birth regions is that it is correlated with location of college and even current location of residence to a great extent, but it is not endogenous with respect to income like the later.

column 1, I report the results with a linear assumption of the experience profile. The results suggest that the bust period had an overall negative impact on all majors. The coefficient on the *Bust* variable suggests that there is an initial earnings disadvantage (the first year after graduation) of 12.5 percent for those graduating in the bust period. This initial disadvantage, however, reduces over the experience profile as suggested by the interaction term with experience.

Interestingly, the effect of the bust becomes particularly severe over the concentration of students in the IT industry. The immediate effect of the bust on changing the concentration of the major in the IT industry by a 100 percentage point (or simply going from zero to full share) increases the earnings gap by 16.3 percent. Furthermore, this disadvantage doesn't improve over the experience profile. In fact, the triple interaction term suggests that relative earnings decrease over the linear experience profile. The negative experience slope for the IT-related majors almost completely offsets any gain based on the positive slope on *Bust*exp.* Additionally, replacing the *ITshare* variable with major fixed effects has a trivial impact on the results (see column 2).

In columns 3 and 4, I use a simple dummy variable functional form to account for any non-linearity in the experience profile. I divide the ten years of potential experience by denoting high experience (HiExp) as having six or more years of experience.[54] The interpretation of the *Bust* variable is slightly different now as it measures not just the immediate impact, but the impact in the first five years of experience as well. Similar to the previous results, the severity of the bust on earnings gets stronger with an increase in concentration in the IT industry and this disproportionate gap in earnings persists in the later

[54] Note, the maximum number of experience years for the bust cohort is nine, while the maximum for the boom cohort is eleven.

part of the experience profile. There is an 11 percent increase in the earnings for bust graduates after five years, but this is not nearly enough to eliminate the earnings gap of 20 percent observed in the first five years, let alone the earnings gap for majors associated with a high concentration in the IT industry.

An overall message from the two different specifications seems to be that the bust had a severe impact on the majors with a higher concentration in the IT industry. It is clear from table 1 that the share of students in the IT industry is close to 10 percent or less for most majors. Computer Science and Electrical and Computer Engineering (ECE) clearly stand out in this table as outliers. Given this rather skewed distribution, we may expect that the results are primarily driven by these two majors with a high concentration in the IT industry. To explore this further, I replace the *ITshare* variable with a dummy variable called *IT*, which equals one if the major is either CS or ECE and zero otherwise.

In table 4, I report the results from the same specifications as before using the dummy variable for IT-related majors. The coefficient on the *Bust* variable is very close in magnitude to the same variable in table 3 suggesting an average negative effect on earnings of around 13 percent for those graduating in the bust period. However, this gap narrows over time for the average major and reaches parity with the boom cohort in 7 years with a slope of 2 in each year. This result is almost identical to the findings in AKS. Interestingly, the initial impact of the bust on IT majors is about 4 percent more. Furthermore, the differential impact on IT majors increases over the experience profile with a slope of negative 1.2 percent each year (see column 2).

The results using the dummy variable form for the experience measure illustrate the same story as with the linear experience measure (column 4 and 5). The IT majors graduating during the bust period have on average an 8.5 percent earnings disadvantage in addition to the 21 percent during the first five years. Even though this gap narrows on average by 11 percent in the second half of the experience profile, this gain is not nearly enough to offset the loss of around 30 percent in earnings for IT majors that graduated in the bust. The persistence of an earnings gap for almost ten years motivates a further analysis into the mechanisms that could explain the long-run differences.

4 Mechanisms

4.1 Theoretical Discussion on Possible Mechanisms

The traditional neo-classical model does not predict the effect of initial labor market experiences to persist over the long run. Alternatively, there are a few models and empirical studies that try to explain the "stickiness" of earnings and the slow catch up effect. The model in Jovanovic (1979) suggests that initial placements may lead to wrong human capital through firm-specific mismatch, which could have long-run impacts on labor market outcomes. This theory is supported by strong evidence from OWH (2012) and Oyer (2005). OWH find that students who graduate in recessions get jobs in lower quality firms with fewer employees, and over time, these students catch up to their luckier counterparts by constantly searching for better quality firms. Similarly, Oyer (2006) finds that students graduating with a PhD in economics in a bad economy get employed at lower ranked schools and this greatly decreases the quantity and quality of research over many years. In the case of IT workers, it would mean that students graduating in a bad economy would work at a start-up rather than a large established company. However, it is not clear whether this would necessarily lead to wrong human capital accumulation.

Even within a single firm, Baker, Gibbs, and Holmstrom (1994) find persistently worse labor market outcomes for those that started in a poor economy over a 20 period. Gibbons and Waldman (2004) justify this result in their task-specific human capital model that suggests that workers that start a job in a bad economy may actually start in lower ranked jobs with less important job tasks compared to their luckier counterparts. This, in turn, leads to persistent disadvantage over the long-run in terms of career growth. This could potentially account for much of the difference in earnings if, for example, the IT workers that graduated during the boom started as the main programmers, whilst those graduating in the bust started as junior programmers. The former could lead to a supervisory (higher earning) position much faster than the latter.

Lastly, it could be the case that students graduating in a bad economy would switch careers or take a job in another field temporarily just because of the lack of jobs available in their main field or industry. Neal (1999) suggests that in fact these initial career mismatch stick and that these divergent career paths could explain much of the difference in earnings over the long-run. All these models are generally supposed to be representative of all workers, but it may be the case that certain models explain the outcomes of the IT workers better than others.

4.2 Empirical Evidence

A persistent finding in all the specifications thus far is that in *both* cohorts, the boom and the bust cohort, the IT majors suffered a slower growth in their earnings over the experience profile compared to the rest of the majors.[55] This suggests that perhaps the downturn in the

[55] In all specifications, the coefficient on *IT*exp* is always negative and statistically significant. Though this coefficient does not completely offset the coefficient on *exp*, it does suggest a slower growth for IT majors than the rest of the majors.

IT industry was more of a correction that affected *all* IT workers and that boom time earnings were just temporary due to inflated wages. If we consider the starting median hourly wage (offered to recent graduates) as an indicator of this demand shift, then figure 5 clearly supports the idea of inflated wages.[56] The most noticeable trend is for CS, which shows a massive growth in starting wages during the boom years then a large correction after 2001. Results on ECE do not indicate such a drastic shift. This could be driven by the glut of new CS students entering the market even after the bust.

I estimate specification 1 for log hourly wages to gauge the immediate and long-term impact of the bust on the IT majors. In table 5, I report the estimates for the log hourly wage using the IT dummy as my treatment indicator instead of *ITshare*, however, the results are qualitatively similar using the continuous variable. The results suggest a crucial role of wage changes for the IT majors compared to the rest of the majors. Specifically, the IT majors that graduated in the bust started with 8.4 percent lower wages than the non-IT majors (see column 1 and 2). This wage gap persists over the linear experience profile. Alternatively, using a dummy variable functional form of experience, the estimates indicate a 13 percent wage gap for the bust graduates compared to the boom graduates in the first five years (see columns 3 and 4). The wage gap for IT majors that graduate in the bust is about 10 percent more severe. Furthermore, there does not seem to be any indication of narrowing in the wage gaps in the second half of the ten year experience profile.

Other mechanisms could also contribute to the earnings gap as mentioned in the previous section. I estimate specification 1 for six labor market outcomes: a dummy for employment, hours worked per week, a dummy for IT occupation, tenure (in months), firm

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[56] Hourly wage is calculated using the annual salary, weeks worked in the year and the usual number of hours worked per week (i.e. salary/weeks/hours).

size, and a dummy for supervisory occupation.[57] I define IT occupation if the individual's job code is defined as either a "computer or information scientists" or "electrical or computer hardware engineers."[58] Firm size is defined as a binary variable with one being assigned to individuals that claim to work for an employer with 5000 or more people.[59] In the case where the outcome variable is binary, the specification represents a simple linear probability model.

In table 6A, I present the results for the six labor market outcomes using a linear functional form for experience. As expected, the probability of being employed is significantly less for those that graduated in the bust period compared to the boom cohort. However, IT graduates that graduated in the bust do slightly better in employment compared to the rest of the majors. It should be noted that employment for the SE majors is generally not an issue as pointed out by AKS. The average employment rate is well above 87 percent (table 2). On the other hand, having an IT-related job turns out to be a key factor here. The interaction term with experience suggests that IT majors that graduated during the bust generally start with IT jobs much like the boom cohort, but over time they tend to leave these jobs disproportionately.

A key factor in determining the earnings gap has been the number hours worked per week (see AKS). The results from table 6A don't seem to indicate any strong role of differences in underemployment. Alternatively, firm quality as measured by firm size has also been found to play a crucial role in determining earnings differences by OWH. In fact, OWH find that earnings gap closes over time through constant job search and job changes

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[57] A binary variable based on whether an individual claims to have a supervisory position is derived from the SUPWK variable.

[58] This definition is based on the SESTAT variable OCPR (NOCPR for surveys after 2001), where respondents are asked to mark the job code that best fits their current job. The two broad categories studied here are comprised of up to 13 specific job codes such as Database Administrator, Web Developer, and so on.

[59] This measure is derived from the variable EMSIZE in the SESTAT, which provides 5-6 categories of firm size. The 5000 or more category is chosen since it represents the largest employment size category consistent across all the survey years.

into better quality firms. My results suggest this may be the case for persistent gaps. IT students that graduated in the bust are 5.8 percent *more* likely to be in a large firm initially and are much more likely to leave large firms over the experience profile. This strange result does not explain the initial gaps in earnings, but could potentially explain some of the long-run gaps.

We would expect that constant job search would result in a lower tenure at any given job. Surprisingly, the results on tenure suggest that the bust cohort stays in a job about 5.3 months more than the boom cohort. However, the negative coefficient on *IT*Bust* almost completely offsets this estimate, resulting in an imprecise estimate for the IT majors. Over time, the IT majors stay in a job slightly longer than the IT majors that graduated in the boom time. The lack of job mobility combined with lower probability of being in a large firm could potentially explain why the IT graduates from the bust period do not catch up in earnings or wages to their luckier counterparts.

Alternatively, the dim prospects of a job in the IT industry may also explain the propensity of going to graduate school. A simple means comparison of IT graduates from the two cohorts suggests that by 2010, both cohorts have, statistically, the same probability (about 41 percent) of having a graduate degree. This result is in contrast to earlier results by AKS and Kahn (2010), where going to graduate school is often a means to delay joining the labor market or to gain further qualifications to counter the negative impact of a recession.

Lastly, I explore the role of supervisory position on the earnings and wage gaps. The results do indicate that the bust cohort, in general, is about 4.7 percent less likely to start in a supervisory position and this gap persists over the experience profile. In table 6B, I present the results using the dummy variable form of experience. As before with the linear form of experience, employment and weekly hours worked are not impacted by the bust for the IT majors. Alternatively, the IT majors graduating in the bust period are about 2.2 percent less likely to be in an IT occupation in the short-run and even more likely to leave in the long-run. They are also significantly less likely to be working in a large firm towards the later five years of the ten year experience profile. Results on tenure and probability of supervisory position are largely imprecise in this functional form.

5 Conclusion

In this paper, I document the short- and long-term impact of the Dot-Com Recession on college graduates and how that impact varies across majors with different levels of concentration in the IT industry. In my analysis, I show that the recession had a particularly strong impact on the IT sector's labor market. This, in turn, had a disproportionate impact on college majors with a high level of concentration in this sector, especially, computer science and electrical and computer engineering. My results suggest that students graduating with these IT-related majors in the bust period had around 4 percent lower earnings and more than 8 percent lower hourly wages immediately after graduating compared to other majors. Furthermore, these gaps persisted even up to ten years after graduation for the IT majors, while the gaps for other major dissipate.

These results support the findings in AKS that highly-concentrated majors are more sensitive to business cycle shocks. However, the particular nature of this recession driven by the bust in the Dot-Com bubble has long-term implications for the IT majors that are quite different from previous empirical studies. I find little or no difference in the number of hours worked per week for the boom and bust cohorts of IT majors. This result is in stark contrast to AKS, but it comes as no surprise considering that the sample of analysis in this paper is of high-skilled majors that wouldn't have trouble finding a fulltime job. However, finding a job in the IT industry seems to be a potential cause for prolonged earnings gap. The results suggest that IT workers graduating in the bust period are less likely to be in an IT occupation, especially, after five years of work experience.

The results on wages are perhaps the most revealing in terms of the overall labor market for the IT majors. One explanation is that the boom cohort experienced a rather inflated wage due to the bubble, but even after the bust, they were able to retain a substantial lead over the unlucky bust cohort. This seems to be true considering that none of the cohorts graduating after the bust all the way up to 2010 had a starting wage that matched the boom cohort.

Another possible explanation for long-term gaps is motivated by previous empirical work in the Canadian labor market by OWH, which suggests job mobility into better quality firms to be a key mechanism that narrows the gap in earnings over time. I find tenure, measured in months worked at a job, to be slightly more for the bust cohort suggesting a lack of job mobility. Additionally, the IT graduates of the bust period are significantly less likely to be working in a large firm. This result combined with the likelihood of being employed outside the IT occupation could be significant contributors to the differences in earnings.

Job immobility and working outside the IT field may not be a matter of choice for the bust graduates. The lack of job growth in the IT industry after the bust in the market combined with the continued supply of new graduates could have created a particularly loose market that persisted much after the recession. This, in turn, put downward pressure on wages that never quite reached the level that was observed at the height of the boom. The loose labor market combined with the low wages may have forced some of the bust IT graduates out of the IT field all-together.

The findings in this paper are of particular relevance to STEM majors. Since many of the STEM majors are geared towards specific occupations or industries, they may be prone to a different set of business cycle experiences than most majors. The Dot-Com recession has provided a natural case study for the IT-related majors in this regard. Future work should consider other markets and majors that could potentially shed more light on the dynamics of the labor market outcomes for college graduates.

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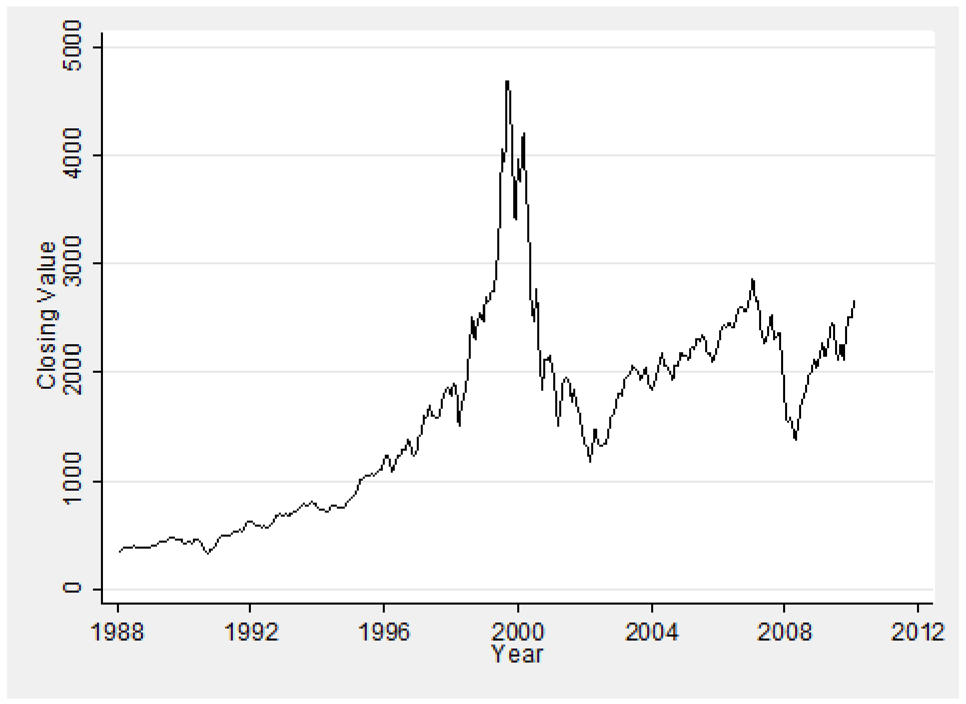
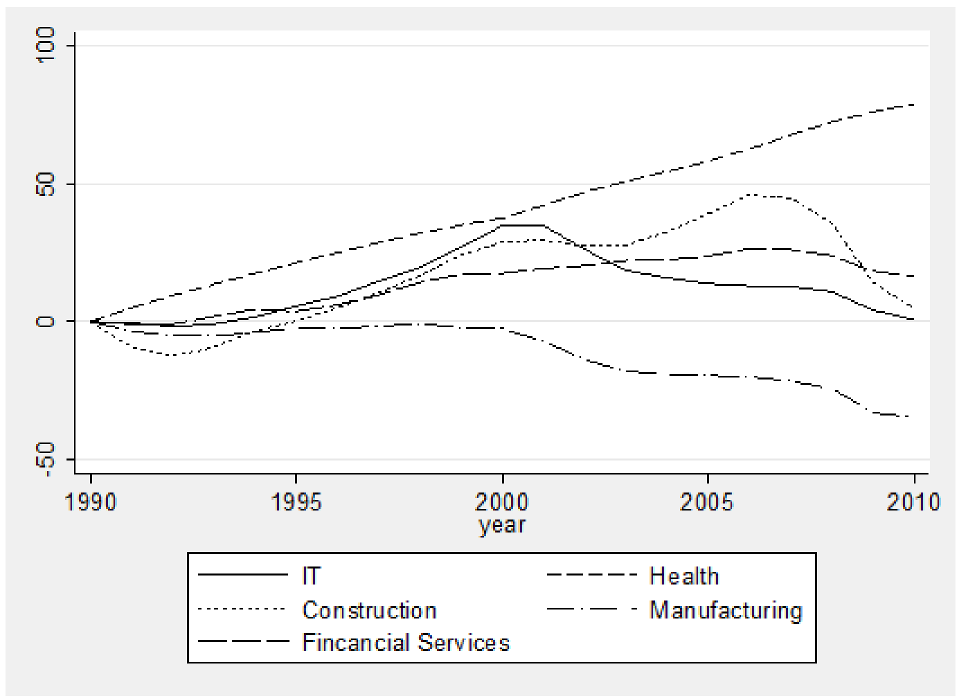
Figure 1: Nasdaq Composite Index Closing Values 1988-2010



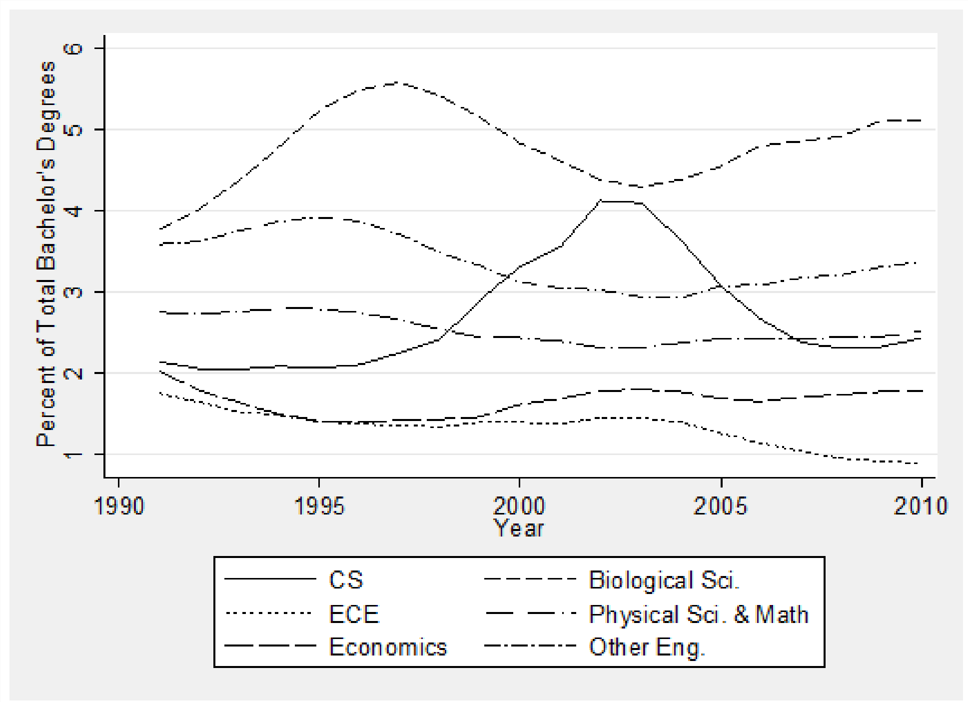
Notes: Monthly data on closing values is used from Yahoo! Finance.

Figure 2: Employment Trends in Five Large Sectors 1990-2010 (Indexed with 1990=0)



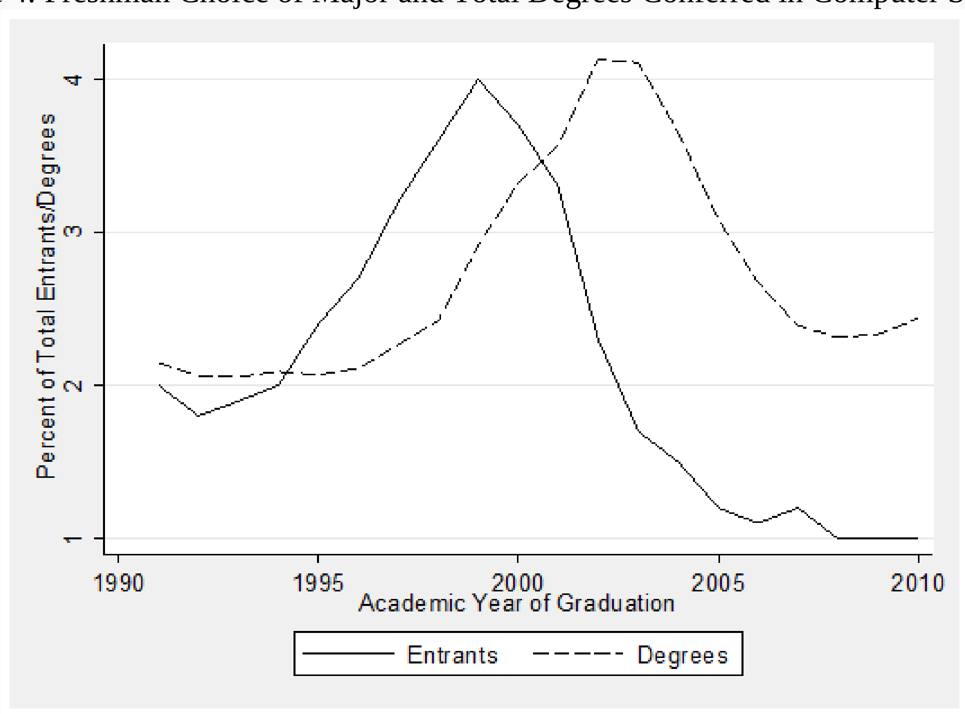
Notes: The sectors represented here are defined by the North American Industrial Classification System (NAICS). The employment data comes from Current Employment Statistics (CES-BLS).

Figure 3: Trends in Degrees Conferred in the U.S. 1991-2010



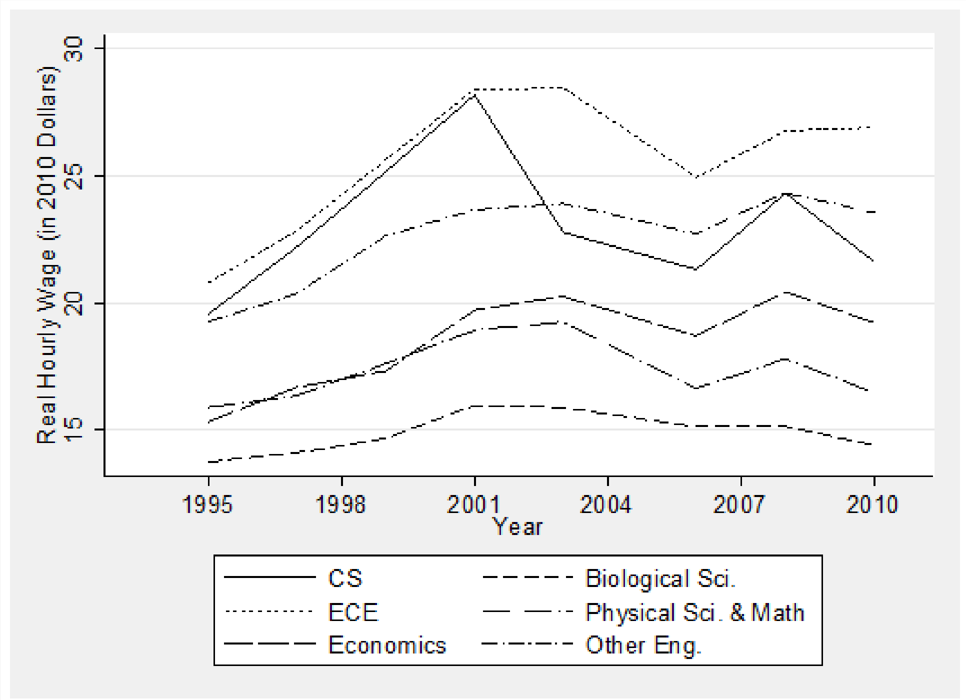
Notes: Yearly data on the number of bachelor's degrees conferred comes from the Integrated Postsecondary Education Data System (IPEDS).

Figure 4: Freshman Choice of Major and Total Degrees Conferred in Computer Science



Notes: Yearly data on the number of bachelor's degrees conferred come IPEDS, while the share of entrants is derived from survey The American Freshman: National Norms (1990-2010).

Figure 5: Starting Median Hourly Wage- by Major



Notes: Data from the National Survey of Recent College Graduates (1995, 1997, 1999, 2001, 2003, 2006, 2008, and 2010) is used to calculate the starting hourly wage. This survey constitutes a representative sample of individuals who have graduated within two years of the survey year. Starting hourly wage is calculated using the salary, divided by weeks worked on the job, divided by the usual hours worked per week. CPI from the BLS is used to convert the nominal amounts into real values.

Table 1: Share of Majors that Claim to Work in IT Industry in 1997

Major Field	Share in IT
Computer and information sciences	0.412
Mathematics and statistics	0.164
Agricultural and food sciences	0.020
Biological sciences	0.021
Environmental life sciences	0.033
Chemistry, except biochemistry	0.042
Earth, atmospheric and ocean sciences	0.046
Physics and astronomy	0.164
Other physical sciences	0.054
Economics	0.063
Political and related sciences	0.037
Psychology	0.035
Sociology and anthropology	0.035
Other social sciences	0.046
Aerospace, aeronautical and astro. eng.	0.081
Chemical engineering	0.050
Civil and architectural engineering	0.027
Electrical and computer engineering	0.223
Industrial engineering	0.094
Mechanical engineering	0.071
Other engineering	0.080

Notes: Data from SESTAT 1997 is used with industry information derived from the EMBUS variable. Data on Major Field is derived from the variable BAMENG. Share in IT industry is calculated by collapsing the individual-level data at the major-level using the survey weights provided by SESTAT.

Table 2: Summary Statistics of Outcome Variables by Cohort

Outcome Variables	*Cohort*	N	Mean	Std. Dev.	Min	Max
Log Yearly Income:	All	34907	10.659	0.771	0.693	13.816
	Boom	17051	10.689	0.787	0.693	13.785
	Bust	17856	10.625	0.752	2.303	13.816
Log Hourly Wage	All	34907	3.061	0.652	-7.065	9.288
	Boom	17051	3.079	0.670	-7.065	7.226
	Bust	17856	3.040	0.630	-4.633	9.288
Employed:	All	39228	0.879	0.327	0	1
	Boom	19013	0.885	0.319	0	1
	Bust	20215	0.871	0.335	0	1
Hours Worked:	All	34907	42.895	11.775	1	96
	Boom	17051	43.107	11.529	1	96
	Bust	17856	42.658	12.040	1	96
IT Work:	All	34907	0.131	0.338	0	1
	Boom	17051	0.132	0.338	0	1
	Bust	17856	0.131	0.337	0	1
Large Firm:	All	34907	0.373	0.483	0	1
	Boom	17051	0.374	0.484	0	1
	Bust	17856	0.371	0.483	0	1
Tenure:	All	34907	37.563	39.965	0	465
	Boom	17051	39.092	41.539	0	461
	Bust	17856	35.855	38.060	0	465
Supervisory Work:	All	34907	0.349	0.477	0	1
	Boom	17051	0.369	0.482	0	1
	Bust	17856	0.328	0.469	0	1

Notes: All variables aside from Employed are restricted to the sample where a yearly income is observed. The variables Employed, IT work, Large Firm, and Supervisor are all 0-1 indicators. Survey weights are used in all cases.

Table 3: The Effect of Graduating in the Dot-Com Bust on Log Yearly Income

	(1)	(2)	(3)	(4)
Bust	-0.125***	-0.132***	-0.200***	-0.197***
	(0.030)	(0.028)	(0.027)	(0.029)
Bust*exp	0.020***	0.020***		
	(0.005)	(0.006)		
ITshare*Bust	-0.163***	-0.162***	-0.302***	-0.301***
	(0.040)	(0.039)	(0.024)	(0.031)
ITshare*exp	-0.083***	-0.079**		
	(0.028)	(0.030)		
ITshare*Bust*exp	-0.033*	-0.034*		
	(0.016)	(0.017)		
exp	0.065***	0.059***		
	(0.012)	(0.011)		
ITshare	1.401***		1.220***	
	(0.268)		(0.223)	
Bust*HiExp			0.109**	0.110**
			(0.039)	(0.040)
ITshare*HiExp			-0.548***	-0.514***
			(0.165)	(0.175)
Bust*ITshare*HiExp			-0.010	-0.024
			(0.111)	(0.122)
HiExp			-0.019	-0.026
			(0.058)	(0.059)
Major FE	No	Yes	No	Yes
N	34,907	34,907	34,907	34,907
R-squared	0.212	0.249	0.209	0.247

Notes: All columns include the background characteristics: mother's education, birth place, minority, and gender. Columns 1 and 2 use a linear variable of experience, while columns 3 and 4 use he dummy variable representation. HiExp is a dummy that represents six or more years of potential experience. Lastly, standard errors are clustered for the 21 majors included in the analysis. Significance of the estimates is designated as *10%, **5%, and ***1%.

Table 4: The Effect of Graduating in the Dot-Com Bust on Log Yearly Income: The Role of IT Majors

	(1)	(2)	(3)	(4)
Bust	-0.133***	-0.141***	-0.213***	-0.212***
	(0.030)	(0.028)	(0.026)	(0.027)
Bust*exp	0.019***	0.019***		
	(0.005)	(0.005)		
IT*Bust	-0.041**	-0.041*	-0.085***	-0.087***
	(0.019)	(0.020)	(0.026)	(0.027)
IT*exp	-0.024***	-0.023**		
	(0.008)	(0.008)		
IT*Bust*exp	-0.011**	-0.012**		
	(0.005)	(0.005)		
exp	0.061***	0.055***		
	(0.012)	(0.011)		
IT	0.446***		0.396***	
	(0.056)		(0.047)	
Bust*HiExp			0.109***	0.110***
			(0.035)	(0.035)
IT*HiExp			-0.159***	-0.150**
			(0.053)	(0.055)
Bust*IT*HiExp			-0.011	-0.018
			(0.032)	(0.036)
HiExp			-0.045	-0.049
			(0.054)	(0.054)
Major FE	No	Yes	No	Yes
N	34,907	34,907	34,907	34,907
R-squared	0.212	0.248	0.210	0.246

Notes: All columns include the background characteristics: mother's education, birth place, minority, and gender. Columns 1 and 2 use a linear variable of experience, while columns 3 and 4 use he dummy variable representation. HiExp is a dummy that represents six or more years of potential experience. Lastly, standard errors are clustered for the 21 majors included in the analysis. Significance of the estimates is designated as *10%, **5%, and ***1%.

Table 5: The Effect of Graduating in the Dot-Com Bust on Log Hourly Wage

	(1)	(2)	(3)	(4)
Bust	-0.042	-0.048	-0.130***	-0.130***
	(0.034)	(0.036)	(0.017)	(0.020)
Bust*exp	-0.001	-0.001		
	(0.008)	(0.008)		
IT*Bust	-0.084***	-0.083**	-0.103***	-0.103***
	(0.029)	(0.031)	(0.022)	(0.023)
IT*exp	-0.022**	-0.022**		
	(0.009)	(0.009)		
IT*Bust*exp	-0.003	-0.004		
	(0.007)	(0.008)		
exp	0.054**	0.050**		
	(0.019)	(0.019)		
IT	0.393***		0.346***	
	(0.061)		(0.044)	
Bust*HiExp			-0.016	-0.016
			(0.052)	(0.054)
IT*HiExp			-0.149**	-0.144**
			(0.054)	(0.056)
Bust*IT*HiExp			0.017	0.013
			(0.044)	(0.047)
HiExp			0.049	0.046
			(0.056)	(0.057)
Major FE	No	Yes	No	Yes
N	34,907	34,907	34,907	34,907
R-squared	0.212	0.248	0.210	0.246

Notes: All columns include the background characteristics: mother's education, birth place, minority, and gender. Columns 1 and 2 use a linear variable of experience, while columns 3 and 4 use he dummy variable representation. HiExp is a dummy that represents six or more years of potential experience. Lastly, standard errors are clustered for the 21 majors included in the analysis. Significance of the estimates is designated as *10%, **5%, and ***1%.

Table 6A: The Effect of Graduating in the Bust on Labor Market Outcomes- with Linear Experience Profile

	P(Employed)	Hours Worked	P(IT work)	P(Large Firm)	Tenure	P(Supervisor)
Bust	-0.063***	-1.552	0.009	-0.019	5.344***	-0.047**
	(0.013)	(1.023)	(0.011)	(0.017)	(1.755)	(0.018)
Bust*exp	0.006*	0.315*	-0.001	0.003	0.110	-0.001
	(0.003)	(0.173)	(0.001)	(0.005)	(0.307)	(0.003)
IT*Bust	0.035*	0.444	0.029	0.058**	-3.763	-0.015
	(0.018)	(0.702)	(0.022)	(0.027)	(2.213)	(0.017)
IT*exp	-0.000	-0.110	-0.010	0.003	0.590*	0.006
	(0.003)	(0.080)	(0.006)	(0.006)	(0.287)	(0.004)
IT*Bust*exp	-0.007	-0.114	-0.006***	-0.010*	0.861**	0.005
	(0.004)	(0.220)	(0.002)	(0.006)	(0.385)	(0.005)
exp	0.001	-0.019	0.015**	0.006	5.050***	-0.013*
	(0.008)	(0.333)	(0.007)	(0.010)	(1.080)	(0.007)
Major FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	39,228	34,956	34,956	34,956	34,956	34,956
R-squared	0.034	0.062	0.457	0.047	0.160	0.039

Notes: All columns include the background characteristics: mother's education, birth place, minority, and gender. All dependent variables aside from Hours Worked and Tenure are dummy variables. Lastly, standard errors are clustered for the 21 majors included in the analysis. Significance of the estimates is designated as *10%, **5%, and ***1%.

Table 6B: The Effect of Graduating in the Bust on Labor Market Outcomes- with Non-Linear Experience Profile

	P(Employed)	Hours Worked	P(IT work)	P(Large Firm)	Tenure	P(Supervisor)
Bust	-0.061***	-1.352**	-0.018*	-0.031**	-3.201*	-0.027
	(0.011)	(0.516)	(0.009)	(0.012)	(1.639)	(0.027)
Bust*HiExp	0.010	0.129	0.026	0.037**	-1.096	-0.006
	(0.017)	(0.304)	(0.035)	(0.015)	(1.880)	(0.015)
IT*Bust	0.043*	2.206*	-0.004	0.025	-0.234	-0.005
	(0.022)	(1.229)	(0.007)	(0.031)	(2.035)	(0.017)
IT*HiExp	0.000	-0.679	-0.055	0.021	3.715**	0.044*
	(0.019)	(0.510)	(0.048)	(0.029)	(1.574)	(0.024)
IT*Bust*HiExp	-0.025	-0.453	-0.043***	-0.061**	2.962	0.029
	(0.021)	(1.235)	(0.015)	(0.024)	(2.349)	(0.023)
HiExp	-0.024	-1.808**	0.016**	-0.016	0.049	-0.008
	(0.014)	(0.771)	(0.007)	(0.015)	(1.610)	(0.023)
Major FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	39,228	34,956	34,956	34,956	34,956	34,956
R-squared	0.034	0.062	0.457	0.047	0.160	0.039

Notes: All columns include the background characteristics: mother's education, birth place, minority, and gender. All dependent variables aside from Hours Worked and Tenure are dummy variables. Lastly, standard errors are clustered for the 21 majors included in the analysis. Significance of the estimates is designated as *10%, **5%, and ***1%.

Last-Place Aversion Revisited

Anand J. Shukla

1 Introduction

Research in social identity and redistributive preferences has grown tremendously in the field of economics in recent years.[60] Though social identity has traditionally fallen under the psychology and sociology literature, it has become an increasingly important input in understanding the economic phenomena of redistributive preferences (eg. Meltzer and Richard, 1981; and Shayo, 2009).[61] A recent study in this regard has proposed that low-income individuals might oppose redistribution because it could differentially help the group just beneath them (Kuziemko et al., 2014, herein KBRN). More generally, the Last-Place Aversion hypothesis suggests that ordinal rank plays a differential role for individuals based on their position in the distribution.

However, distinctions in ranks aren't always clear in the real world, and whether individuals actually identify themselves with a certain rank can be a key factor in influencing behavior. For instance, McGuire's (1984) distinctiveness argument postulates that being "distinctive" within a group makes gender more salient and activates gender-associated behaviors. Similarly Benjamin et al. (2010) find that making ethnicity salient in a laboratory setting elicits certain behaviors on intertemporal and risk choices. In this paper, I study the observed behavior associated with last-place aversion (LPA) in KBRN and its relation to the saliency of rank. Specifically, I conduct a similar laboratory experiment to the one in KBRN

[60] See Costa-Font & Cowell (2015) for a recent survey of this literature.
[61] The literature in economics has largely focused on the importance of relative position in wealth or consumption in a reference group and its relation to utility and economic behavior (see Engelmann & Strobel, 2004 and the papers reviewed within).

with a slight modification on the saliency of rank to see how the results change. Additionally, I analyze redistributive preferences outside the lab, using data on voting behavior in the U.S. that is indicative of LPA, and measure the relationship between this behavior and the salience of rank in an income distribution.

KBRN conduct two experiments in the laboratory to test the LPA hypothesis. In the first experiment, participants are asked to either receive a payment with probability one, or play a two-outcome lottery with equivalent expected value. The authors find that individuals randomly placed in the last place are significantly more likely to pick the lottery compared to any other rank. In the second experiment, individuals are randomly assigned to a group where they receive a unique dollar amount and then are asked to give $2 (bonus amount) to the person ranked either below them or above them in the group.[62] They find the second-to-last group disproportionately does not give to the individual ranked below them because it would put them in last place. In both experiments, the authors make salient the rank order of each member of the experimental group in addition to the initial endowment. That is, they frame the question in the experiments by explicitly pointing out the rank of each individual in the group, even going as far as calling the person with the lowest endowment- “last place.” Framing it in such a way could induce the participant to think that the experimenter wants the rank to be considered, and thereby elicit the behavior that suggests last-place aversion.

I explore whether a slightly modified dictator game to the one in KBRN- without explicit rank information, and only information on each member’s endowments- would generate the same pattern of behavior. Giving the option of donating $2 just to the person ranked above or below could also potentially induce rank concerns among the participants.

[62] Individuals in the dictator game are allotted a unique dollar amount in the set {1, 2, 3, 4, 5, 6}. Giving the bonus of $2 to a person ranked below would drop the rank of the individual as the difference between each rank is only one dollar.

Therefore, I allow participants to choose *any* other member to receive the $2. Furthermore, I limit the groups to five individuals and give small and simple amounts to each member such that rank can be easily inferred.

I find no pattern of behavior consistent with last-place aversion in the experiment. Overwhelmingly, those not at the bottom of the distribution chose to give the $2 to the member at the bottom of the distribution. This result is statistically the same across the first to fourth ranked individuals. The bottom (fifth) ranked individuals almost always gave the $2 to the second-to-last individual. These results are most consistent with the distributional preference model of Charness and Rabin (2002).

In addition to the lab experiments, KBRN analyze whether the LPA behavior exists outside the lab. They conduct their own survey and use data from the General Social Survey (GSS) from the U.S. to study patterns associated with an individual's place in the income distribution and their redistributive preferences. In their own survey, they find that individuals making just above the minimum wage are significantly more likely to oppose minimum wage increases. Similarly, they find evidence from the GSS data that those in the 6^{th}-8^{th} decile of the income strata are more likely to vote against redistributive policies, compared to the individuals in the other deciles, controlling for a linear trend in income deciles.

Similar to the lab experiments, we may expect the salience of these income categories to play a role in influencing rank-specific behavior. In this regard, I use various inequality indicators as a measure of the salience of rank and study whether the results indicating LPA in KBRN vary across states with higher level of inequality. Klor and Shayo (2010) find that identity within a group with a hierarchy of categories (such as education or occupation) is heavily influenced by the distance from, or similarity with, other group members. We then may expect that rank distinctions are easier to identify when the difference or distance in the average income between different classes is large, such as higher inequality in the income distribution.

Furthermore, research suggests that a localized rank or standing may serve as better reference point for rank-specific behavior compared to one based on the entire US (Luttmer, 2005). In addition to the income deciles based on the entire US (as in KBRN), I also use state-specific income deciles for a more localized measure of rank.

Using data from the Cooperative Congressional Election Survey (CCES), I find qualitatively similar results that indicate an aversion to last place for the 6th-8th decile of income strata as in KBRN.[63] The CCES has a clear advantage over the GSS because of its large sample size that allows for a within-state analysis. I find the results are more prominent when using income deciles within a state rather than the ones computed for the entire country.

Interestingly, I find strong evidence that higher inequality is associated with a stronger aversion for last place. The results show that a standard deviation increase in the inequality measure (defined as the income ratio 90/10) of a state is associated with a 1.5 percentage point decrease in the propensity of the 6th-8th income decile individuals to vote *for* an increase in the federal minimum wage. This explains about 36 percent of the standard deviation of the propensity to vote in favor of an increase.

Aside from contributing to the theory of last-place aversion, the results from this paper compliment some of the previous research in preferences for redistribution. For example, Luttmer (2001) finds a negative relationship between diversity and preferences for

[63] The 6th-8th income decile group is a close approximation for those above the minimum wage category. This is explained further in section 4.

redistribution. Specifically, the author shows evidence of a “negative exposure effect” whereby people decrease their support for welfare programs as the recipiency rate of welfare in the community increases. Similarly, Alesina and Glaesar (2004) argue that the support for the welfare state weakens with increasing social heterogeneity. They find a negative correlation between “racial fractionalization” and the level of social spending. Clearly, the salience of social categories, whether through inequality, or diversity, or other heterogeneity, plays crucial role on redistributive preferences and economic behavior.

The paper is organized as follows. In section 2, I go over the experimental methodology that I use to test whether framing can induce the results observed in KBRN. Section 3 discusses the results from this experiment. In section 4, I analyze the preference for an increase the federal minimum wage from a nationwide election survey. Section 5 provides a discussion on the findings and suggests potential ways forward.

2 Experimental Methodology

The experiment is designed to study last-place aversion in a neutral environment. The experiment differs from KBRN, in two distinct ways. I do not explicitly provide the ranks of each participant in a group. Instead, I simply provide the amount each member of the group has. Second, I allow the individuals to pick any individual to give the bonus amount to.[64] In comparison, KBRN permit the bonus to be awarded only to the person ranked either directly above or directly below.

The experiment procedure is as follows:

[64] Instructions given during the experiment are provided in the Appendix 1 for reference.

The computer program randomly picks a group of 5 participants and assigns each group member an initial endowment from {1.50, 3.25, 5.00, 6.75, and 8.50} with a difference of $1.75 between each rank. The distribution of endowments here is different from the one in KBRN, where the distribution is simply a dollar value for each integer from 1 to 6. Giving integer values from 1 to 6 makes rank salient. Therefore, I choose to give slightly odd values, where rank can be easily inferred, but it is not made explicit.

The participants are asked to choose some other member of the group to give a bonus of $2.

This process is repeated over ten rounds per group.[65] At the end of these rounds, one round is selected at random to be the paying round. The participants receive their endowment from that round. In addition, one of the five member's bonus decision is chosen at random to implement. Whoever the randomly chosen group member assigned the bonus to in the paying round receives the $2 bonus.

The experiment consists of two sessions, each with ten rounds, with a total of 30 participants. This gives us a total of 60 game observations.[66]

Hypothesis: Those individuals who received an initial value of $3.25 (second-to-last-place) are less likely to give the money to those individuals who received the lowest value of $1 compared to those receiving a higher initial endowment.

[65] The groups stay the same across the ten rounds.

[66] In one session, there are three groups playing in each of the ten rounds adding up to 30 games. Combine this with the games played in the second session and that sums up to 60 games.

3 Results

The results from the experiment reveal no indication of last-place aversion. Figure 1 illustrates the proportion of times the two-dollar bonus was given to the lowest ranked group member. The lowest ranked individual received the bonus by all other members majority of the rounds. In contrast to the results in KBRN, the second-to-last ranked group member, on average, chose for the bonus to be allocated to the lowest member more than 80 percent of the time. This is statistically no different than the estimates of the higher ranked individuals at the 5 percent confidence.

Figure 2, shows the entire distribution of choices for each rank. The most visible dark-shaded bars represent the proportion of time the two-dollar bonus was allocated to the person with $1.50, which is exactly the result from figure 1.

The lowest ranked (with $1.50) individuals allocated the bonus to the second lowest ranked person (with $3.25) almost 70 percent of the time. Overall, the second-to-last ranked was allocated the bonus more than any other rank except the lowest. Surprisingly, the highest ranked with a value of $8.50, received the bonus the third most-often.

These results do not fully address or disprove the LPA theory prescribed in KBRN. One clear difference in my experiment is that a person is only 25 cents behind the person ranked below them if they chose to give the bonus to that individual, whereas in KBRN, the person is put one dollar behind the person ranked below them with this decision. If the difference between one dollar and 25 cents potentially explains the results, then certainly the LPA theory requires some caveats. Alternatively, it may be the case that making rank salient among the participants plays a crucial in their preferences.

4 Last-Place Aversion and Support for Minimum Wage Increases

An aversion to last place has also been inferred from real-world situations with two such examples given in KBRN; one, using the General Social Survey (GSS) in the U.S. and another with an online survey. Both analyze the preferences for an increase in the minimum wage. The results from the GSS data suggest that those with incomes in the 6th-8th decile range have a significantly lower probability to vote for redistribution policies compared to the general negative trend of voting for redistributive policies as income increases. Furthermore, the online survey in KBRN specifically asks for hourly wage, and finds similar results- those just above the minimum wage disproportionately do not support increases in minimum wage.

This begs the question: does the saliency of economic class distinctions in the real-world exaggerate the correlations observed in KBRN? In this section, I explore this further by analyzing the favorability of the same crucial redistributive policy- an increase in the federal minimum wage- and how this favorability varies across the U.S. states with different levels of economic class distinctions.[67] Income inequality by its very nature exaggerates class distinction. Therefore, I use various measures of income inequality-as a proxy for salience of rank- and see how the favorability of increasing the minimum wage laws varies across these measures.

4.1 Data and Empirical Framework

I analyze data from the 2006 and 2008 Cooperative Congressional Election Survey (CCES), a national stratified sample survey administered by YouGov/Polimetrix

[67] There are four states that have a higher minimum wage than the proposed federal minimum wage, and therefore would not be affected. I show the results excluding these states as well.

(Ansolabehere, 2006 and 2008).[68] This extensive survey on how Americans view the Congress and their policies is the largest survey in the U.S. in this context. The questionnaire consists of a wide-range of policy-related queries ranging from gay marriage to the Israel-Palestine conflict, and of course, an increase in the minimum wage. A clear advantage of this data is its large sample size that is big enough to do a within-state analysis, unlike the GSS data.[69]

Similar to the GSS data, family income is the only measure of income reported in the CCES. The income measure in KBRN is adjusted by dividing the family income by the square root of the total number of people in the household. However, CCES does not contain the information regarding the number of household members, and so cohabitation with a spouse/partner is used as an approximation. Therefore, in the case of cohabitation, the family income is divided by the square root of 2.

Like KBRN, I categorize the sample into income deciles. However, the question remains whether the 6th-8th deciles truly represents the second-to-last rank. KBRN do not give any supporting argument aside from the fact that it is a rough approximation. It is difficult to gauge given the lack of information on hourly wage. An alternative way to approximate the group of interest is to use the American Community Survey (ACS) to calculate the income percentile of those just above $7.25 hourly wage, the proposed minimum wage, and compare it to the income percentile measure from CCES. It turns out that this group is approximately in the 12th percentile of the income distribution and across

[68] This data and its sampling framework is available at the following website: http://projects.iq.harvard.edu/cces/home

[69] My sample consists of all individuals that report an income, which reduces the sample by 10 percent. Additionally, I require region and state indicators, which reduces the sample by a further 2 percent, to a total number of 58,246 observations.

the 50 states it is largely in the second-to-last decile.[70] If the correlation between the adjusted family income from CCES and the hourly wage from ACS is strong, then the range of 6^{th}-8^{th} is not a bad assumption to capture the group just above the minimum wage group and below the median.

Following the methodology in KBRN, I regress the individual's opinion about a minimum wage increase (equal to one if yes) on the individual's income decile and also an indicator of whether the person is in the 6^{th}-8^{th} decile. The year and geographic region fixed effects are also included. Results from this specification should be directly comparable to the results found in KBRN.

A localized rank or standing may play a bigger role in inducing LPA-type behavior as is suggested in some literature (eg. Luttmer, 2005). Specifically, the literature states that individuals often compare their relative standing according to their reference point, which is often local rather than national. In the second set of regressions, I modify the individual's income decile to reflect the decile in their respective state, rather than the decile in the U.S.

Lastly, I study how the LPA results from the above mentioned regressions vary with the level of inequality across the 50 states and D.C. I create three measures of income inequality- the ratio of 90^{th} and 50^{th} percentile, the ratio of the 90^{th} and 10^{th} percentile, and the ratio of the 50^{th} and 10^{th} percentile- using yearly income from the ACS data.[71] This variable is added to the regression along with an interaction term of the LPA indicator (6^{th}-8^{th} decile) and the inequality measure. This interaction term would describe how the LPA results vary across the inequality measure.

[70] Appendix Table 2 shows the income percentile that corresponds to $7.25 for the U.S. and by each state, using ACS 2006 and 2008. Hourly wage is calculated using the method established in Welsh-Loveman et al. (2014).
[71] The ACS data is preferred over the CCES data when deriving exact percentiles for each state because of its larger sample size that gives a more precise measure of inequality per state.

4.2 Results

The average support for an increase in the federal minimum wage is 76 percent across the U.S. In Table 1 I report the regression results for the two methods of calculating an individual's economic status. The first measure is an income decile derived from all of the U.S. akin to the KBRN study (column 1), while the second measure is a within-state income decile (column 4). The results from column 4 are almost twice as big as those in column 1. People in the second-to-last group (defined as 6th-8th decile) are .6 percentage point less likely to vote for a hike compared to the same group define within each state, are 1.2 percentage point less likely to vote for a hike in the minimum wage.[72] In column 2 and 4, I control for regional effects that may influence a preference for an increase in minimum wage given some of the political distinctions across regions.[73] Lastly, in column 3 and 6, I exclude states that already have a higher minimum wage than the proposed increase.[74] This is done to analyze only those states where an increase in the federal minimum wage would actually matter. The results remain largely similar across the different specifications.

In Table 3, I explore the relation how the LPA behavior varies across the inequality in each state. In this specification, I limit the analysis to the within-state decile measure for income status since the earlier results suggest that local rankings hold greater importance. I report the summary statistics for this specification in Table 2.

All three measures of inequality give qualitatively similar results- higher inequality is associated with a stronger aversion for last-place. I explore the specification with the inequality measures for each state (columns 1, 3, and 5), and also excluding the inequality

[72] I report the point estimates for each decile for the first specification in the Appendix 3.
[73] Regions are defined as in the US Census: Northeast, Midwest, South, and West.
[74] The states with a minimum wage higher than the proposed hike in the federal minimum wage include: Connecticut, Oregon, Vermont, and Washington. A complete list of state minimum wages is provided in Appendix 2, Table 1.

measure and instead including a state dummy (columns 2, 4, and 6). On average, a standard deviation increase in the 90/10 measure translates to a 1.5 percentage point decrease in the probability of favoring an increasing in minimum wage. This is about 36 percent of the standard deviation of the dependent variable. Similarly, a standard deviation increase, in the case of the 90/50 measures, is associated with a 1.2 percentage point decrease in the probability of favoring an increasing in minimum wage. This is about 29 percent of the standard deviation of the dependent variable. Both measures have a statistically significant effect on the level of last-place aversion. Though the 50/10 inequality measure gives a similar negative relationship, the estimate on the interaction term is quite imprecise.

One possible reason for the differences in the precision could be because people consider the top of the income strata (ie. 90th percentile) as a reference point and the further away their group is from this reference point, the more insecure they feel, resulting in a LPA-type behavior. A 50/10 ratio is ambiguous in this regard because a large ratio does not necessarily mean a greater distance between the second-to-last place group from the reference point. Whereas the other two measures (90/10 and 90/50) necessarily measure the distance between the second-to-last group to the reference point.

5 Discussion

In this paper, I try to dissect whether the last-place aversion (LPA), observed in Kuziemko et al. (2014, KBRN), is associated with the saliency of rank distinctions. I conduct a modified dictator game without explicitly displaying ranks, but allowing the participants to infer rank on their own. I find no such pattern in the results that suggest an aversion to last place. The contrasting results of this paper compared to KBRN could be due to the differences in payouts in both of the experiments, where giving two dollars to the person ranked below would put the second-to-last person a dollar behind in KBRN, while it would only put the person 25 cents behind in my experiment. The difference between one dollar and 25 cents could potentially explain the results, but it clearly suggests that the theory of last-place aversion requires some caveats.

Additionally, I analyze the preferences for an increase in the minimum wage and its relationship with the saliency of income ranks across the US, which contributes to the deeper understanding of this relationship. I find the preference for an increase in the minimum wage decreases as inequality increases among the group of individuals categorized just above the minimum wage. Insofar as an increase in the inequality measure makes rank distinctions salient, this analysis gives further credence to the idea that rank-based behavior is more present when rank distinctions are salient. However, this could be an endogenous result. It could be that a particular set of behaviors-such as voting against minimum wage increases-are means by which social groups create status for their members, and thereby increase inequality (McAdams, 1995).[75]

The analysis in this paper is by no means comprehensive or complete, but it does provide some empirical support for earlier work done on social identity and redistributive preferences. There are several ways in which this work can be extended. One obvious area to explore is the mechanisms through which rank is made salient in the real world, and how it can be incorporated into economic and empirical models. Additionally, it may be interesting to have a comparison over various other factors that influence redistributive preferences such as social heterogeneity as defined by the distribution of race (Alesina and Glaesar, 2004) or the share of welfare recipients (Luttmer, 2001).

[75] McAdams argues that discrimination is a means by which social groups create status for their members.

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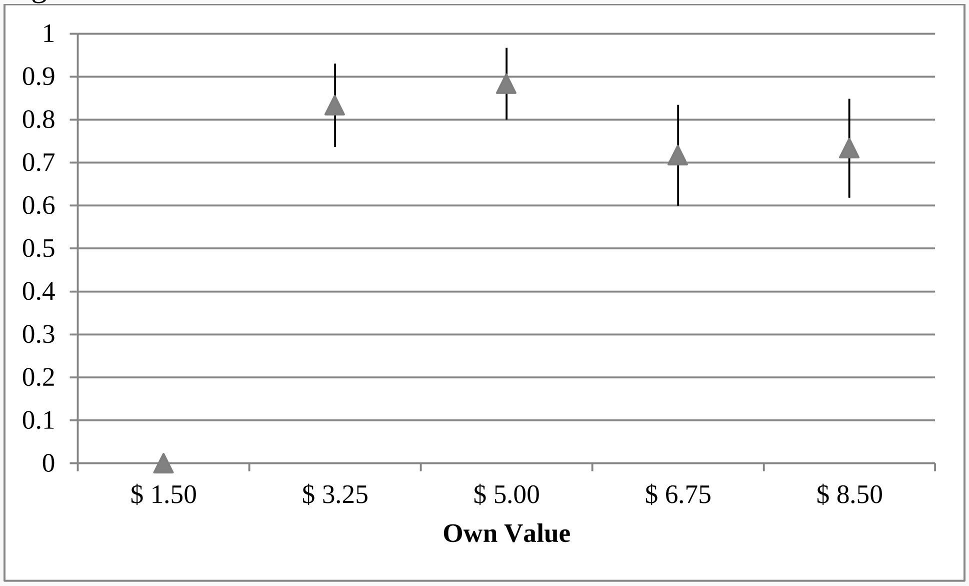
Figure 1: The Share of Donations to the Lowest Ranked Individual



Note: The triangles represent the average, while the black bands correspond to the 95 percent confidence intervals.

Figure 2: Distribution of Decisions by the Endowment Level

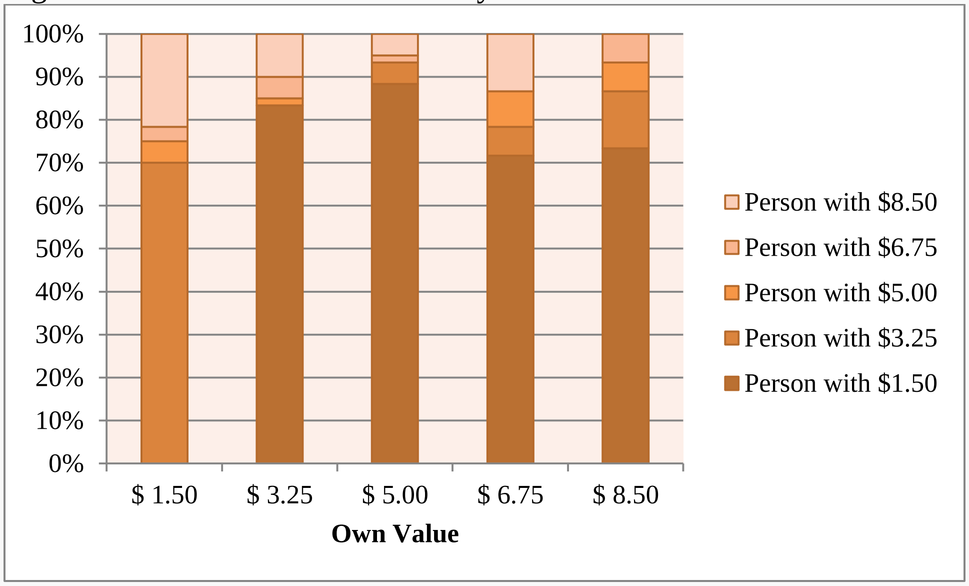


Table 1: Support for Minimum Wage Increase By Income Decile

	Income decile across US			Income decile within each state		
	(1)	(2)	(3)	(4)	(5)	(6)
LPA group (6th-8th US wide)	-0.00688*	-0.00707*	-0.00835**			
	(0.00395)	(0.00395)	(0.00401)			
Income decile (US wide)	-0.0215***	-0.0217***	-0.0216***			
	(0.000655)	(0.000657)	(0.000666)			
LPA group (6th-8th by state)				-0.0122***	-0.0124***	-0.0133***
				(0.00404)	(0.00403)	(0.00410)
Income decile (state wide)				-0.0216***	-0.0217***	-0.0217***
				(0.000651)	(0.000650)	(0.000659)
Region FE	No	Yes	Yes	No	Yes	Yes
High minimum wage state	Yes	Yes	No	Yes	Yes	No
Observations	58,248	58,246	56,331	58,248	58,246	56,331
R-squared	0.027	0.028	0.029	0.027	0.029	0.029

Note: Robust standard errors are reported in parentheses. The drop in observations in columns 2 and 5 reflect the availability of data, while columns 3 and 6 represent the observations without the states with minimum wages above the proposed Federal minimum wage.

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

Table 2: Summary Statistics of the State-Level Variables

	Observations	Mean	Std. Dev.	Minimum	Maximum
Minimum Wage Increase	51	0.766	0.041	0.692	0.919
90/10 Ratio	51	16.411	2.053	10.714	24.444
90/50 Ratio	51	2.628	0.159	2.2	3
50/10 Ratio	51	6.241	0.636	4.286	8.444

Note: Inequality measures derived from American Community Survey 2006 & 2008. Propensity to vote for an increase in the minimum wage derived from Cooperative Congressional Election Survey 2006 & 2008.

Table 3: Support for Minimum Wage Increase By Income Decile and Inequality

	(1)	(2)	(3)	(4)	(5)	(6)
LPA group (6th-8th)	0.115***	0.112***	0.180***	0.187***	0.0353	0.0422
	(0.0379)	(0.0378)	(0.0652)	(0.0651)	(0.0346)	(0.0392)
Income decile	-0.0217***	-0.0218***	-0.0217***	-0.0218***	-0.0217***	-0.0218***
	(0.000650)	(0.000649)	(0.000650)	(0.000649)	(0.000650)	(0.000649)
LPA*Inequality (90/10)	-0.00764***	-0.00751***				
	(0.00226)	(0.00226)				
Inequality (90/10)	0.00404***					
	(0.00139)					
LPA*Inequality (90/50)			-0.0711***	-0.0739***		
			(0.0242)	(0.0241)		
Inequality (90/50)			0.0497***			
			(0.0144)			
LPA*Inequality (50/10)					-0.00767	-0.00892
					(0.00554)	(0.00630)
Inequality (50/10)					0.0284**	
					(0.0125)	
Region FE	Yes	No	Yes	No	Yes	No
State FE	No	Yes	No	Yes	No	Yes
Observations	58,246	58,248	58,246	58,248	58,246	58,248
R-squared	0.029	0.033	0.029	0.032	0.029	0.032

Note: Robust standard errors in parentheses.

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

Appendix 1: Experiment

INSTRUCTIONS

During the game, you will participate in several rounds, and at the beginning of each round, the computer will randomly hold a lottery, and give you and the other players in your group different amounts of money.

During each round, you will be presented with a choice about who should get more money. This additional money is drawn from a separate pool and does not take away from the amount of money you have. The choices you make are private, and will not be shown to anyone else at any time.

Once everyone in your group has made a choice, the computer will randomly select one group member's choice, and award the additional money as that person decided. Then, a new lottery will be held and the next round will automatically begin.

At the end of the session, the computer will automatically select one round to be the paying round. Your earnings for the experiment will be determined by what happened in the paying round only. Since you will not know which round will be chosen as the paying round, it is best to treat each round as if it is the paying round.

Appendix 2: Wage Information

Appendix Table 1: Minimum Wage in 2006 and 2008

	2006	**2008**
Federal (FLSA)	5.15	**5.85**
Alabama	...	...
Alaska	7.15	7.15
Arizona	...	**6.9**
Arkansas	5.15 [c]	6.25[c]
California	6.75	**8**
Colorado	5.15	**7.02**
Connecticut	**7.4**	7.65
Delaware	6.15	**7.15**
Florida	**6.4**	**6.79**
Georgia	5.15(d)	5.15(d)
Hawaii	**6.75**	7.25
Idaho	5.15	**5.85**
Illinois	**6.50[c]**	**7.50[c]**
Indiana	5.15(e)	**5.85(e)**
Iowa	5.15	**7.25**
Kansas	2.65	2.65
Kentucky	5.15	**5.85**
Louisiana	...	...
Maine	**6.5**	**7**
Maryland	5.15	6.15
Massachusetts	6.75	**8**
Michigan	5.15(e)	**7.15(e)**
Minnesota	**5.25 - 6.15(g)**	5.25-6.15(g)
Mississippi	...	...
Missouri	5.15	**6.65**
Montana	4.00 - 5.15	**4.00-6.25(g)**
Nebraska	5.15[c]	**5.85[c]**
Nevada	5.15	**6.33**
New Hampshire	5.15	**6.5**
New Jersey	**6.15**	7.15
New Mexico	5.15	**6.5**
New York	**6.75**	7.15
North Carolina	5.15	6.15
North Dakota	5.15	**5.85**
Ohio	2.80 - 4.25(g)	**7**
Oklahoma	2.00 - 5.15	**2.00-5.85(g)**
Oregon	**7.5**	**7.95**
Pennsylvania	5.15	**7.15**

Rhode Island	6.75	7.4
South Carolina	...	...
South Dakota	5.15	**5.85**
Tennessee	...	...
Texas	5.15	**5.85**
Utah	5.15	**5.85**
Vermont	**7.25**	**7.68(e)**
Virginia	5.15[c]	**5.85[c]**
Washington	**7.63**	**8.07**
West Virginia	5.15(d)	**6.55**
Wisconsin	**5.7**	6.5
Wyoming	5.15	5.15
District of Columbia	**7**	7

Source: U.S. Department of Labor, Wage and Hour Division

Notes:

... - not applicable

[c] - Rates applicable to employers of four or more.
(d) - Rates applicable to employers of six or more. In West Virginia, applicable to employers of six or more in one location.

(e) - Rates applicable to employers of two or more.
(g) - Minnesota sets a lower rate for enterprises with annual receipts of less than $500,000 ($4.90, January 1, 1998-January 1, 2005). The dollar amount prior to September 1, 1997 was $362,500 ($4.00 - January 1, 1991-January 1, 1997); Montana sets a lower rate for businesses with gross annual sales of $110,000 or less ($4.00 - January 1, 1992-January 1, 2005); Ohio sets a lower rate for employers with gross annual sales from $150,000 to $500,000 ($3.35 - January 1, 1991-January 1, 2005) and for employers with gross annual sales under $150,000 ($2.50 - January 1, 1991-January 1, 2005); Oklahoma sets a lower rate for employers of fewer than 10 full-time employees at any one location and for those with annual gross sales of less than $100,000 ($2.00, January 1, 1991-January 1, 2005)

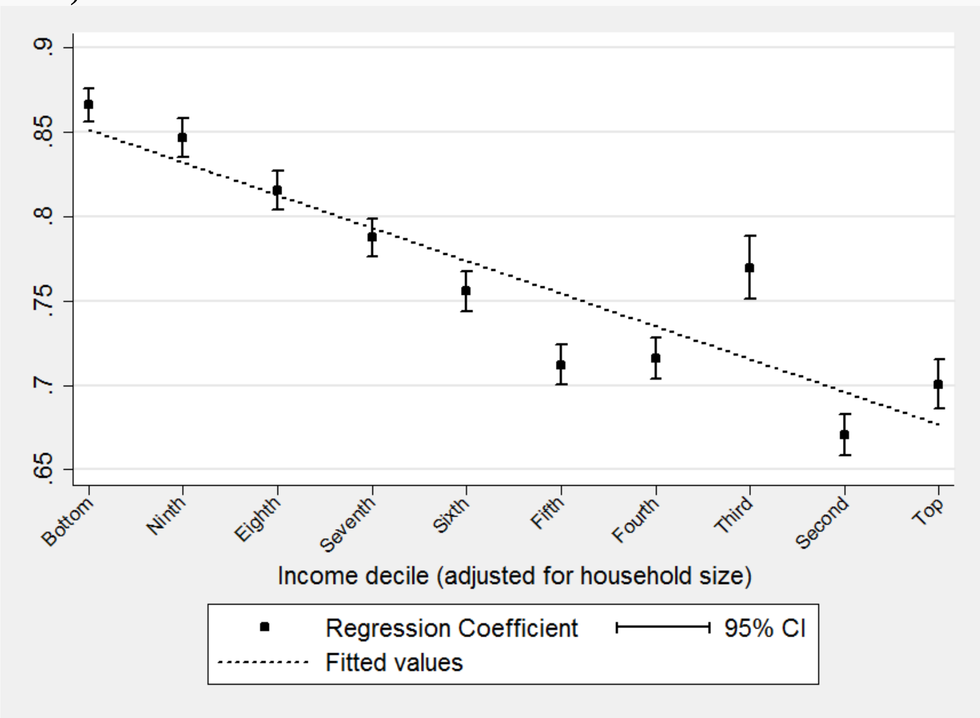
Appendix Table 2: Percentile of those earning $7.25 as an hourly wage	
Region	*Percentile at $7.25*
All of US	12.88
Alabama	16.45556
Alaska	11.66698
Arizona	12.40609
Arkansas	17.3664
California	11.84255
Colorado	12.00122
Connecticut	7.64063
Delaware	10.76401
District of Columbia	8.880015
Florida	12.63986
Georgia	13.66008
Hawaii	10.92264
Idaho	16.86697
Illinois	12.02847
Indiana	13.77444
Iowa	14.24542
Kansas	15.01536
Kentucky	14.90406
Louisiana	17.80921
Maine	12.02194
Maryland	8.281553
Massachusetts	8.227299
Michigan	13.84739
Minnesota	11.15433
Mississippi	18.48244
Missouri	14.78837
Montana	17.19782
Nebraska	15.36291
Nevada	9.202
New Hampshire	9.068355
New Jersey	8.36542
New Mexico	17.05123
New York	11.37
North Carolina	14.46551
North Dakota	15.9292
Ohio	13.58315
Oklahoma	17.72738
Oregon	11.81168

Pennsylvania	13.12073
Rhode Island	10.39789
South Carolina	15.15353
South Dakota	16.48961
Tennessee	14.94125
Texas	15.84258
Utah	14.74256
Vermont	10.58262
Virginia	11.23226
Washington	9.854938
West Virginia	18.95328
Wisconsin	11.61821
Wyoming	13.87283

Source: American Community Survey (ACS) 2006 and 2008.
Note: Hourly wage calculations are done using the method proscribed in Welsh-Loveman et al. (2014). All income data has been CPI adjusted to account for differences in years.

Appendix 3: Minimum Wage Increase By Decile

Appendix Figure 1: Propensity to vote for a hike in the Minimum Wage- by Decile (US-Wide)



Appendix Figure 2: Propensity to vote for a hike in the Minimum Wage- by Decile (by each state)