Leveraging Administrative Data for Bias Audits: Assessing Disparate Coverage with Mobility Data for COVID-19 Policy

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ABSTRACT
Anonymized smartphone-based mobility data has been widely adopted in devising and evaluating COVID-19 response strategies such as the targeting of public health resources. Yet little attention has been paid to measurement validity and demographic bias, due in part to the lack of documentation about which users are represented as well as the challenge of obtaining ground truth data on unique visits and demographics. We illustrate how linking large-scale administrative data can enable auditing mobility data for bias in the absence of demographic information and ground truth labels. More precisely, we show that linking voter roll data—containing individual-level voter turnout for specific voting locations along with race and age—can facilitate the construction of rigorous bias and reliability tests. These tests illuminate a sampling bias that is particularly noteworthy in the pandemic context: older and non-white voters are less likely to be captured by mobility data. We show that allocating public health resources based on such mobility data could disproportionately harm high-risk elderly and minority groups.

1 INTRODUCTION
Mobility data has played a central role in the response to COVID-19. Describing the movement of millions of people, smartphone-based mobility data has been used to analyze the effectiveness of social distancing polices (non-pharmaceutical interventions), illustrate how movement impacts the transmission of COVID-19, and probe how different sectors of the economy have been affected by social distancing policies [1, 4, 7, 10, 20, 22, 33, 46]. Despite the high-stakes settings in which this data is deployed, there has been no independent assessment of the reliability of this data. In this paper we show how administrative data (i.e., data from government agencies kept for administrative purposes) can be used to perform such an assessment.

Data reliability should be a foremost concern in all policy-making and policy evaluation settings, and is especially important for mobility data due to the lack of transparency surrounding data provenance. Mobility data providers obtain their data from opt-in location-sharing mobile apps, such as navigation, weather, or social media apps, but do not disclose which specific apps feed into their data [31]. This opacity prevents data consumers such as policymakers and researchers from understanding who is represented in the data, a key question for enabling effective and equitable policies in high-stakes settings such as the COVID-19 pandemic. According to Grantz et al. [45], “a critical need to understand where and to what extent these biases may exist” in their discussion on the use of mobility data for COVID-19 response.

Of particular interest is potential sampling bias with respect to important demographic variables in the context of the pandemic: age and race. Older age has been established as an increased risk factor for COVID-19-related mortality [52]. African-American, Native-American and LatinX communities have seen disproportionately high case and death counts from COVID-19 [45] and the pandemic has reinforced existing health inequities that affect vulnerable communities [24]. If certain races or age groups are not well-represented in data used to inform policy-making, we risk enacting policies that fail to help those at greatest risk and serve to further exacerbate disparities.

In this paper we assess SafeGraph, a widely-used point-of-interest (POI)-based mobility dataset1 for disparate coverage by age and race. We define coverage with respect to a POI: coverage is the proportion of traffic at a POI that is recorded in the mobility data. For privacy reasons, many mobility datasets are aggregated up from the individual level to the physical point-of-interest (POI) level. Due to this aggregation, we lack the resolution to assess individual-level coverage quantities like the fraction of members of a demographic subgroup of interest who are represented in the data. Nonetheless, our POI-based notion of coverage is relevant for many COVID-19 policies that are made based on traffic to POIs, such as deciding to close certain business sectors or determining where to locate resources like pop-up testing sites. We use differences in the distributions of age and race across POIs to assess demographic disparities in coverage.

While we focus here on a specific dataset and implications for COVID-19 policy, the question of how one can assess disparate coverage is a more general one in algorithmic governance. Ground truth is often lacking, which is precisely why policymakers and academics have flocked toward big data, on the implicit assumption that scale can overcome more conventional questions of data reliability, sampling bias, and the like [2, 35]. Government agencies may not always have access to protected attributes, making fairness and bias assessments challenging [32].

The main contributions of our paper are as follows:

1 We show how administrative data can enable audits for bias and reliability (§ 5)

1POIs refer to anywhere people spend money or time, including schools, brick-and-mortar stores, parks, etc. https://www.safegraph.com/
(2) We characterize the measurement validity of a smartphone-based mobility dataset that is widely used for COVID-19 research, SafeGraph (§ 4.2, 6.1)
(3) We illuminate significant demographic disparities in the coverage of SafeGraph (§ 6.2)
(4) We illustrate how this disparate coverage may distort policy decisions to the detriment of vulnerable populations (§ 6.3)
(5) We perform robustness tests that evaluate our results against a possible type of confounding (§ 5.3, 6.2.1)

Our paper proceeds as follows. Section 2 and Section 3 discuss related work and background on the uses of mobility data in the pandemic. Section 4 provides an overview of our auditing framework and formalizes the assumptions to construct bias and reliability tests. Section 5 discusses the estimation approach using voter roll data. Section 6 presents results that while SafeGraph can be used to estimate voter turnout, the mobility data systematically undersamples older individuals and minorities. Section 7 discusses interpretation and limitations.

2 RELATED WORK

Our assessment of disparate coverage is related to several strands in the literature. First, the most closely related work to ours is SafeGraph’s own analysis of sampling bias discussed below. However, that analysis examines demographic bias only at the national aggregated level and does not address the question of demographic bias for POI-specific inferences. Ours is the first independent assessment of demographic bias to the extent we are aware.

Second, our work relates to existing work on demographic bias in smartphone-based estimates [51]. A notable line of survey research has examined the distinct demographics of smartphone users [18, 36]. [49] and [50] document significant concerns about mobility-based estimates from mobile phone data, including particularly low coverage for elderly. The literature further finds that smartphone ownership in the United States varies significantly with demographic attributes [6]. In 2019 an estimated 81% of Americans owned smartphones with ownership rates of 96% for those aged 18-29 and ownership rates of 53% for those aged over 65 [42]. Racial disparities in smartphone ownership are less pronounced, with an ownership rate of 82%, 80%, and 79% for White, Latinx, and African-American individuals, respectively. Even conditional on mobile phone ownership, however, demographic disparities may still exist. App usage may differ by demographic group. According to one report, 69% of U.S. teenagers, for instance, use Snapchat, compared to 24% of U.S. adults. Of particular relevance to mobility datasets, the rate at which users opt in to location tracking may vary by demographic subgroup. Hoy and Milne, for instance, reported that college-aged women exhibit greater concerns with third party data usage. And even among users who who opt in to a specific app, usage behavior may differ according to demographics. Older users, for instance, may be more likely to use a smartphone as a “classic phone” [3].

Our work is in many ways a response to a recent call to characterize the biases in mobility data used for COVID-19 policies [23]. Grantz et al. highlight the potential for demographic bias, citing “clear sociodemographic and age biases of mobile phone ownership.” They note, “Identifying and quantifying these biases is particularly challenging, though, when there is no clear gold standard against which to validate mobile phone data.” We provide the first rigorous test for demographic bias using auxiliary estimates of ground truth.

Third, our work bears similarity to the literature on demographic bias in medical data and decision-making. A long line of research has demonstrated that medical research is disproportionately conducted on white males [17, 38, 41]. This literature has cataloged the harmful effects of making treatment decisions for subgroups that were underrepresented in the data [5, 47, 48]. In much the same vein, our work calls into question research conclusions based on SafeGraph data that may not be relevant for older or minority subgroups.

Last, our work relates more broadly to the sustained efforts within machine learning to understand sources of demographic bias in algorithmic decision making [12, 13, 21, 25, 34]. Important work has audited demographic bias of facial recognition technology [8], child welfare screening tools [11], criminal risk assessment scores [43], and health care allocation tools [2, 39]. Often the underlying data is identified as a major source of bias that propagates through the algorithm and leads to disparate impacts in the decision-making stage. Similarly, our study illustrates how disparate coverage in smartphone-based data can misallocate COVID-19 resources.

3 BACKGROUND ON SAFEGRAPH MOBILITY DATA

We now discuss the SafeGraph mobility dataset, illustrate how this data has been widely deployed to study and provide policy recommendations for the public health response to COVID-19, and discuss SafeGraph’s own assessment of sampling bias.

3.1 SafeGraph Mobility Data

SafeGraph contains mobility data from roughly 47M mobile devices in the United States. The company sources this data from mobile applications, such as navigation, weather, or social media apps, where users have opted in to location tracking. It aggregates this information by points-of-interest (POIs) such as schools, restaurants, parks, airports, and brick-and-mortar stores. Hourly visit counts are available for each of over 6M POIs in their database. Individual device pattern data is not distributed for researchers due to privacy concerns. Our analysis relies on SafeGraph’s ‘research release’ data which aggregates visits at the POI level.

3.2 Use of SafeGraph Data in COVID-19 Response

When the pandemic hit, SafeGraph released much of its data for free as part of the “COVID-19 Data Consortium” to enable researchers, non-profits, and governments to leverage insights from mobility data. As a result, SafeGraph’s mobility data has became the dataset de rigueur in pandemic research. The Centers for Disease Control and Prevention (CDC) employs SafeGraph data to examine the effectiveness of social distancing measures [37]. According to SafeGraph, the CDC also uses SafeGraph to identify healthcare sites that are reaching capacity limits and to tailor health communications. The California Governor’s Office, and the cities of Los Angeles [19],

2https://docs.safegraph.com/docs/places-summary-statistics
San Francisco, San Jose, San Antonio, Memphis, and Louisville, have each relied on SafeGraph data to formulate COVID-19 policy, including risk measurements of specific areas and facilities and enforcement of social distancing measures. Academics, too, have employed the data widely to understand the pandemic: [10] used SafeGraph data to examine how social distancing compliance varied by demographic group; [15, 16] used SafeGraph to infer the effect of “superspreader” events such as the Sturgis Motorcycle Rally and campaign events; [40] examined whether social distancing was more prevalent in high risk areas with high xenophobia; and [1] examined whether social distancing compliance was driven by political partisanship, to name a few. What is common across all of these works is that they assume that SafeGraph data is representative of the target population.

3.3 SafeGraph Analysis of Sampling Bias

SafeGraph has issued a public report about the representativeness of its data [44]. While SafeGraph does not have individual user attributes (e.g., age, race, education, income), it merged census data based on census block group (CBG), the smallest geographic unit for which the census publishes data, to assess bias along demographic characteristics. The racial breakdown of device holders, for instance, was allocated proportionally based on the racial breakdown of a CBG. SafeGraph then compared the total SafeGraph imputed demographics against census population demographics at the national, state, county, and CBG levels. According to SafeGraph, the results looked “quantitatively very close to the expected” at the state and county levels, but the sampling rates at the CBG level looked highly unrepresentative. SafeGraph warned that “local analyses examining only a few CBGs” should proceed with caution.

SafeGraph’s examination for sampling bias should be applauded. Companies may not always have the incentive to address these questions directly, and SafeGraph’s analysis is transparent, with data and replication code provided. As far as we are aware, it remains the only analysis of SafeGraph sampling bias.

Nevertheless, their analysis suffers from several key limitations. First, because SafeGraph lacks demographic information about the users, the imputation of the CBG attributes imposes a strong homogeneity assumption. The mere fact that 52% of Atlanta’s population is African American does not mean that five out of ten SafeGraph devices in Atlanta belong to African-Americans. Second, by aggregating demographic analyses nationally for a single attribute at a time, the results may miss significant differences in the joint distribution of features. For instance, if coverage is better for younger populations and for whiter populations, but whiter populations are on average older than non-white populations, then evaluating coverage marginally against either race or age will underestimate disparities. Indeed we present evidence for such an effect in § 6. Third, the dramatic variation in SafeGraph coverage across CBGs is serious cause for concern because many of the COVID-19 analyses referenced above leverage SafeGraph data at finer geographic units than CBGs (e.g., POIs). This risks drawing conclusions from data at a level of resolution that SafeGraph has not established to be free from coverage disparities. Because SafeGraph’s analysis examines demographic bias only at census aggregated levels and does not address the question of demographic bias for POI-specific inferences, an independent coverage audit remains critical.

4 AUDITING FRAMEWORK

In this section we outline our proposed auditing methodology and state the conditions under which the proposed method allows us to detect demographic disparities in coverage. We motivate our approach by first describing the idealized audit we would perform if we had access to ground truth data. We then modify this framework to account for the limitations of available administrative data.

4.1 Notation

Let \( I = \{1, \ldots, n\} \) denote a set of SafeGraph POIs. Let \( S_i \in \mathbb{R}^n \) denote a vector of the SafeGraph traffic count (i.e, number of visits) for day \( j \in J \) where each element \( S_{ij} \) indicates the traffic to POI \( i \) on day \( j \). Similarly let \( T_j \) denote the ground truth traffic (visits) to POI \( i \) during day \( j \). When the context is clear, we omit the superscript \( j \) when referring to vectors \( S \in \mathbb{R}^n \) and \( T \in \mathbb{R}^n \). We use \( \odot \) to denote Hadamard division (the element-wise division of two matrices). With this, we define our coverage function \( C(S, T) \).

**Definition 1 (Coverage function).** Let \( C(S, T) : \mathbb{R}^n \times \mathbb{R}^n \to \mathbb{R}^n \) denote the following coverage function:

\[
C(S, T) = S \odot T
\]

The coverage function yields a vector where the \( ith \) element equals \( \frac{S_i}{T_i} \) and describes the coverage of POI \( i \).

Let \( D_i^j \) denote a numeric measure of the demographics of visitors to POI \( i \) on day \( j \); for instance \( D_i^j \) may be the percentage of visitors to a location on a specific day that are over the age of 65. Let \( \text{cor}(X, Y) = \frac{\text{cov}(X, Y)}{\sqrt{\text{var}(X) \text{var}(Y)}} \) denote the Pearson correlation between vectors \( X \) and \( Y \) and let \( r(X) \) be the rank function that returns the rank of vector \( X \). Our audit will consider the (Spearman) rank correlation \( \text{cor}(r(X), r(Y)) \), which provides a more flexible measure of association since it assumes only monotonicity (versus the linearity assumption in the Pearson correlation).

4.2 Idealized Audit

Our audit assesses how well SafeGraph measures ground truth visits and whether this coverage varies with demographics. We operationalize these two targets as follows:

**Definition 2 (Measurement signal and validity).** Define the strength of measurement signal as

\[
\text{cor}(r(S), r(T)).
\]

A positive signal indicates facial measurement validity, and a signal close to one indicates high measurement validity.

**Definition 3 (Disparate coverage).** We will say that disparate coverage exists when the rank correlation between coverage
and the demographic measure is statistically different from zero:
\[ \text{cor}(r(C(S,T), r(D)) \neq 0). \]

We are interested in identifying an association of any kind; we are not concerned with identifying a causal effect. Age might have a causal effect on smartphone usage, setting aside the question of manipulability [28], as depicted in the top panel (a) of Fig. 1. But as the bottom panel (b) depicts, age may not directly affect SafeGraph coverage but be directly correlated with a factor like urban/rural residence, which in turn does affect SafeGraph coverage. For either mechanism, the policy-relevant conclusion remains that SafeGraph is underrepresenting certain age groups.

### 4.3 Real-world audit

In reality, there is no ground truth source of information about foot traffic and the corresponding demographics for all 6 million POIs. Instead, we must make do with estimates of \(T\) and \(D\) based on auxiliary data sources about some subset of visits to a subset of POIs. In order to identify the relationship of interest (Def. 3) between coverage and demographics, we need the following to hold:

**Assumption 1 (No induced confounding).** The estimation procedure does not induce a confounding factor that affects both the estimate of demographics and the estimate of coverage (see Figure 2).

**Assumption 2 (No selection bias).** The selection is not based on an interaction between factors that affect coverage and demographics.

After introducing our auxiliary data and the estimation procedure, § 5.3 revisits the plausibility and testability of these assumptions. Appendix C discusses the analogous assumptions required to identify the target for measurement validity (Def. 2).

### 5 ESTIMATION VIA ADMINISTRATIVE DATA

It is quite challenging to identify data sources for ground truth visits to POIs with corresponding demographic information [23]. Consider for instance large sporting events where stadium attendance is closely tracked. Can we leverage differences in audience demographics based on the event (e.g., international soccer game between two countries) in order to assess disparate coverage? Two major impediments are lack of access to precise demographic estimates as well as confounding factors such as tailgating that may vary with demographics, thereby violating Assumption 1.

#### 5.1 Administrative data on voter turnout

We propose a solution using large-scale administrative data that records individual-level visits along with demographic information: voter turnout data. Such data has several unique advantages. First, because these stem from official certified records by election authorities, voter turnout information is of uniquely high fidelity. In an analysis of five voter file vendors, Pew Research, for instance, found that the vendors had 85% agreement about turnout in the 2018 election [30]. Second, numerous states break out voter turnout by *in person, election day* voting. This is critical, given the rise of absentee, mail, and early voting, enabling us to infer the exact count of individuals visiting a specific voting location on a specific day.

Third, voter registration forms typically include fields for date of birth, gender, and race. When race is not provided, data vendors estimate race. The Pew study found race to be 79% accurate across the five vendors, with accuracies varying from 67% for African-Americans to 72% for Hispanics to 93% for non-Hispanics. Fourth, voting (poll) locations provide many data samples across a wide geography with demographic variation, which is necessary given that SafeGraph POI visit information is necessarily aggregated. In short, this administrative data enables us to cleanly infer the demographics and number of voters to polling locations on election day.

We use individual voter records provided by L2\(^6\), a private voter file vendor which aggregates publicly available voter records from jurisdictions nationwide. Our analysis relies primarily on four fields from the voter files: age, race, precinct, and turnout. While this data is near ideal, it is missing one key piece of information: the poll location. We hence obtained a crosswalk of voting precinct to poll location from the North Carolina Secretary of State. This crosswalk enables us to map each voter via their voting precinct to a SafeGraph POI. We note that poll locations are often schools, community centers, religious institutions, and fire stations. These POIs may

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\(^6\)North Carolina, for instance requests both race and ethnicity (https://s3.amazonaws.com/dl.ncsbe.gov/Voter_Registration/NCVoterRegForm_06W.pdf).

\(^7\)The study did not name which voter file vendors were analyzed.

\(^8\)https://l2political.com/
hence also have non-voter traffic on election day. We address this possible source of confounding in § 5.2. Overall, our data includes 595K voters who turned out to vote at 549 voting locations that could be matched. Table 1 presents summary statistics on voters associated with polling locations that could be matched, showing that our data is highly representative of all voting locations. (Details on the data and preprocessing are provided in Appendices A and B.)

| Voters          | Matched Voters | All Voters   |
|-----------------|----------------|--------------|
| Mean Age        | 52.84          | 52.78        |
| Std Age         | 16.65          | 16.59        |
| Proportion over 65 | 0.26          | 0.26         |
| Proportion Hispanic | 0.04        | 0.04         |
| Proportion Black | 0.19           | 0.19         |
| Proportion White | 0.71           | 0.71         |

Table 1: Demographics of all voters in North Carolina’s 2018 general election compared to voters included in our analysis (“matched voters”). The matched voters are representative of the full voting population. Details of the matching procedure are given in Appendix B.

The disparate impact (Def. 3) question in this setting is does SafeGraph coverage of voters at different poll locations vary with voter demographics? We focus on two key demographic risk factors for COVID-19: age and race. We summarize the age distribution at a polling location \(i\) by computing the proportion of voters over age 65, which we denote by \(A_i\). Let \(R_i\) denote the proportion of voters whose ethnicity in L2 data is listed as Hispanic and Portuguese, Likely African-American, East and South Asian, or Other (all voters whose ethnicity was not listed as European). We will refer to \(R_i\) as the proportion of non-white voters.

We briefly discuss issues of representativeness. Because the voting population is not a random sample of the population, the magnitude of an association between coverage and age/race among voters is likely different than the magnitude among the whole population. Since the voting population is older and more white than the general population [30], the association among voters could very well underestimate the magnitude of the population association. However, our target measure of bias (Def. 3) does not depend on the magnitude of an association. Assuming Conditions 1 and 2 hold, evidence for any association on the voting population is indicative of an association (of perhaps different magnitude) on the full population.

### 5.2 Adjustment for non-voter traffic

Non-voter traffic may be incorporated into SafeGraph measures and may confound our analysis if the magnitude of that non-voter traffic varies with the demographic attributes of the voters. For instance, if younger voting populations are more likely to vote at community centers which have large non-voter traffic and older voting populations are more likely to vote at churches which have small non-voter traffic, then even if SafeGraph has no disparate coverage, we would observe a negative relationship between coverage and age.

We control for this confounding by estimating non-voter traffic using mean imputation. In Appendix D, we provide similar results using a local linear regression type imputation procedure.

#### 5.2.1 Additional notation

Letting \(j^*\) denote election day, we estimate the non-voter traffic at poll location \(i\) on election day, \(Z_i^{j^*}\), by averaging SafeGraph traffic to \(i\) on adjacent days:

\[
Z_i^{j^*} = \frac{S_i^{j^*-1} + S_i^{j^*+1}}{2}
\]

This adjustment enables us to compute the marginal traffic over the estimated baseline, which we term SafeGraph marginal traffic.

**Definition 4 (Marginal traffic).** SafeGraph marginal traffic denotes device counts above estimated baseline: \(S_i^j - Z_i^j\).

Let \(V_i^{j^*}\) denote the number of voters at poll location \(i\) as recorded by L2. With this, we refine our definition of coverage using the coverage function from Def. 1:

**Definition 5 (SafeGraph coverage).** SafeGraph coverage is \(C(S_i^j - Z_i^j, V_i^{j^*})\). Each element \(i\) of this vector refers to the ratio of marginal traffic at POI \(i\) to voter turnout at \(i\).

We focus on two measures of demographics: \(A\), the proportion of voters over the age of 65, and \(R\), the proportion of voters who are an ethnic group besides white.

We control for this confounding by estimating non-voter traffic using mean imputation. In Appendix D, we provide similar results using a local linear regression type imputation procedure.

### 5.3 Testing disparate coverage among voters

Following our proposed audit in Def. 3, we test whether there is a rank correlation between \(C(S_i^j - Z_i^j, V_i^{j^*})\) and demographic measure \(D\). If so, and if we believe Assumptions 1-2 hold, then we may conclude SafeGraph has disparate coverage. We next discuss how we can partially test Assumption 1 and how we can reason about the remaining assumptions.

We can relax Assumption 1 that the estimation procedure does not induce confounding by decomposing confounding into time-invariant and time-varying confounding. We can test for time-invariant confounding, enabling us to make the weaker and more reasonable assumption of no time-varying confounding. A time-invariant confounder is a confounder that affects our demographic estimate and traffic on election day and on non-election days. This contrasts to a time-varying confounder that affects our demographic estimate and traffic on election day but does not affect traffic on non-election days. Examples of time-invariant and time-varying confounding are given in Figure 3. The assumption of no time-varying confounding is testable but it is reasonable to believe this holds in our setting. Most voting places,

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7 Non-voter traffic may be affected by device attribution errors, in which device GPS locations are incorrectly assigned to one of two adjacent POIs. SafeGraph reports in its user documentation that “[i]t is more difficult to measure visits to a midtown Manhattan Starbucks than a visit to a suburban standalone Starbucks.” If younger voting populations are more likely to vote in dense urban polling locations, then even if there isn’t large non-voter traffic in the same facility, large traffic in an adjacent facility could still be incorrectly attributed to the polling location with greater likelihood than to a suburban polling location.

8 The adjustment resulted in negative estimates of voter traffic for poll locations at schools. Because of the difficulties in estimating the baseline for schools, we have removed schools from our analysis.

9 In what follows we use the generic variable \(D\) to indicate either measure of demographics.
we conduct a placebo test that repeats the disparate coverage audit (Def. 3) comparing marginal traffic on placebo (non-election) days (e.g., parking lot). We do not believe this is likely because voting vote; and (ii) Older (or non-white) voters leave their smartphones 10 on treatment effects [27].

Examples of violations of this assumption might include: (i) The older (or non-white) population who does vote is more likely to use smartphones than the older (or non-white) population who doesn’t vote is more likely to vote; and (ii) Older (or non-white) voters leave their smartphones at home when they go vote but always carry their smartphones otherwise whereas younger (or white) voters bring their smartphones to the polls and elsewhere. We believe such mechanisms are unlikely. Testing this assumption would require the use of an additional auxiliary dataset which is outside the scope of this paper.

## 6 RESULTS

### 6.1 Measurement Validity

Election day brings a dramatic increase in traffic to polling locations relative to non-election days, and any valid measure of visits should detect this outlier. Figure 4 shows the daily aggregate traffic across polling locations for October and November of 2018, and as expected, we see a significant increase in both total traffic (top panel) and marginal traffic (bottom panel) on election day. To assess the strength of this signal using the framework described above (Def. 2), we present the correlation between marginal SafeGraph traffic on election day and actual voter turnout. The rank correlation test yields a positive correlation: \( \text{cor}(r(S^0 - Z^0), r(V^*)) = 0.445 \) with p-value < 0.001.\(^{11}\) Figure 5 displays this relationship by comparing the marginal election traffic \( S^0 - Z^0 \) on the x-axis against actual voter counts \( T_f^\bullet \) on the y-axis for each polling location.

This corroborates that SafeGraph data is able to detect broad patterns in movement and visits. That said, the estimates at the individual polling place location level are quite noisy: root mean-squared error is 899 voters with a standard error of 45. For instance, amongst polling places that registered 20 marginal devices, roughly 250 to 1600 actual voters turned out. This significant noise is likely due to a combination of factors. First, SafeGraph may incorrectly attribute voters to nearby POIs because of incorrect building geometries. Second, we are not able to perfectly adjust for non-election traffic. Third, SafeGraph may have disparate coverage of voters by demographic attributes. This last factor is the focus of our analysis.

### 6.2 Demographic Bias

We assess whether the demographic composition of voters who actually turned out to vote in person is correlated with coverage. First, we find a statistically significant negative correlation between

\[^{10}\] This placebo test is similar to methods of randomization inference in the literature on treatment effects [27].

\[^{11}\] The correlation is similar but slightly lower for unadjusted SafeGraph traffic: \( \text{cor}(r(S^0), r(V^*)) = 0.409 \) with p-value < 0.001.
Figure 4: SafeGraph traffic by weekday over October and November 2018 for all polling locations in North Carolina. The top panel shows all SafeGraph traffic and the bottom panel the marginal traffic computed using the method in § 5.2. In both total and marginal traffic, the election day (dotted) line shows a significant boost in traffic.

Figure 5: Election day traffic as observed by SafeGraph (x-axis) and actual voter turnout across polling locations (y-axis). Each dot represents a polling location in North Carolina in the 2018 general election.

Figure 6: Estimated SafeGraph coverage rates against age and race for North Carolina 2018 general election for percentiles of poll location by age (top) and race (bottom).

SafeGraph coverage and the primary risk factor of COVID-19: age. The top panel of Figure 6 shows how SafeGraph coverage \(C(S - Z, V)\) varies with \(A\), the proportion of voters over age 65. The rank correlation test yields \(\text{cor}(r(C(S - Z, V), r(A)) = -0.18\) with p-value < 0.001. We also find that coverage weakly decreases as the proportion of non-white voters increases (bottom panel). The rank correlation of race and coverage is \(\text{cor}(r(C(S - Z, V), r(R)) = -0.076\) with p-value = 0.073.

We now examine the interaction between age and race, which is particularly important for two reasons. First, there are widespread concerns that disparate impact can be more pronounced at the “intersection” of protected groups [8, 9, 14]. Second, as we show in Appendix A, age and race are highly correlated in our sample. Polling locations with younger voters are also more likely to have
higher proportions of minority voters. We hence fit simple linear regressions to model coverage as a function of the percentage over 65, percentage white, and the interaction between these demographic attributes. Appendix E presents fuller regression results, which demonstrate that once we control for age, the effect for race is more robust. Because the demographics have to be interpreted jointly, the top panel of Figure 7 presents a heat map of coverage with age bins (quartiles) on the x-axis and race bins (quartiles) on the y-axis. This bottom left cell, for instance, shows that precincts that are the most white and young have highest coverage rates. Conditional on a young precinct, a greater minority population decreases coverage. The lowest coverage is for older minority precincts. The lower panel of Figure 7 similarly plots race on the x-axis against coverage on the y-axis, separating older precincts (yellow) and younger precincts (blue). As can be seen from the regression lines, there is both an average difference between older and younger precincts and coverage declines as the minority population increases.

In short, we have provided evidence that there is disparate SafeGraph coverage by two protected attributes that are risk factors for COVID-19: age and race. We bolster this claim of disparate coverage by next showing that we pass the placebo test for time-invariant confounding.

6.2.1 Placebo tests of Assumption 1. In this section we use the placebo test framework described in §1 to support the assumption of no time-invariant confounding. We consider \( \rho(r(C(S^j - Z^j, V^{j*})), r(D^{j*})) \), the rank correlation between voter demographics and the ratio of SafeGraph marginal traffic on non-election days to voter turnout. Evidence of a non-zero correlation may suggest time-invariant confounding induced by our estimation procedure. Figure 8 shows that the election-day rank correlation is significantly outside the placebo distribution (empirical one-sided \( p \)-values are 0 and 0.03 for age and race, respectively). Our second robustness check computes the coefficients for the linear regression of \( C(S^j - Z^j, V^{j*}) \sim A^{j*}, R^{j*} \) on all weekdays \( j \) in October and November 2018. The coefficients for age and race are statistically outside the placebo distribution (empirical one-sided \( p \)-values are 0 and 0.02 for age and race respectively). In Appendix C, we present results that show that placebo tests for measurement validity also pass.

6.3 Policy implications

We now examine the policy implications of disparate coverage in light of the widespread adoption of SafeGraph data in COVID-19 response. In particular, we show how disparate coverage may lead to under-allocation of important health resources to vulnerable populations. For instance, suppose the policy decision at hand is where to locate mobile pop-up COVID-19 testing sites, and suppose the aim is to place these sites in the most trafficked areas to encourage asymptomatic individuals to get tested. One approach would use SafeGraph traffic estimates to rank order POIs. How would this ordering compare to the optimal ordering by ground truth traffic? Using voter turnout as an approximation to ground truth traffic, we perform linear regression of the rank of voter turnout against rank according to SafeGraph traffic as well as age and race: \( r(V) \sim r(S - Z) + A + R. \) Table 2 presents results of this rank regression (where rank is in descending order), confirming that the SafeGraph rank is significantly correlated with ground truth rank. But the large coefficient on age indicates that each percentage point increase in voters over 65 is associated with a 4 point drop in rank relative to the optimal ranking. Similarly, the coefficient on race indicates that each point increase in percent non-white is
Table 2: To evaluate a potential rank-based policy allocation, we compare the rank of voter turnout against rank by SafeGraph traffic, controlling for age and race in a linear regression. Although SafeGraph rank is correlated with the optimal rank by voter turnout, the coefficients on age and race indicate that each demographic percentage point increase is associated with a 4-point and 1-point drop in rank for age and race, respectively. This indicates that significant adjustments based on demographic composition should be made to a SafeGraph ranking. Failure to do so may direct resources away from older and more minority populations.

We also consider the implications of using SafeGraph to inform resource allocation decisions, such as provision of health care resources such as masks, decisions to open or close categories of businesses in public health orders, and whether to allocate investigations in failures to comply with social distancing. We compare two approaches to such resource allocation decisions as follows: We bin polling locations into terciles based on their age and race and calculate what the allocation would be under ground truth (from voter turnout data) and under the SafeGraph data. Table 3 presents results. Each cell presents the proportion of resources that would be allocated to that age-race tercile, demonstrating that strict reliance on SafeGraph would under-allocate resources by 35% to the oldest/most non-white category (p-value < 0.05) and over-allocate resources by 30% to the youngest/whitest category (p-value < 0.05).
We have provided the first independent audit of demographic bias we are not able to test coverage at the individual level. To avoid white populations. Our results suggest that policies made without policies not abiding by social distancing), it is unlikely to capture the kind of demographic interaction effects we document here. Much more work should be done to study disparate coverage and ideally provide e.g. a weighing correction to the normalization factors that properly accounts for the demographic disparities documented in this audit.

Another possible solution is increased transparency. Researchers do not know details about the source of SafeGraph’s mobility data, namely which mobile apps feed into the SafeGraph ecosystem. Access to such information may make the bias correction approach more tractable. If for instance researchers could identify that a data point emanates from Snapchat, then they could use what is known about the Snapchat user base to make adjustments. Given its increasing importance for policy, SafeGraph should consider disclosing more details about which apps feed into their ecosystem.

| SafeGraph allocation | Optimal voter allocation |
|----------------------|--------------------------|
| 1                    | 0.35 ± 0.03              | 0.27 ± 0.01              |
| 2                    | 0.32 ± 0.03              | 0.28 ± 0.02              |
| 3                    | 0.18 ± 0.02              | 0.21 ± 0.01              |
| 4                    | 0.15 ± 0.02              | 0.23 ± 0.02              |

Table 3: Allocation of resources for age-race tertiles by SafeGraph versus by true voter counts (with standard errors). The SafeGraph allocation redirects over 30% of the optimal allocation from the oldest/most non-white tertile (3) to the youngest/whitest tertile (1) (p-value < 0.05).
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APPENDIX

A DATA

A.1 Mobility Data

Our mobility data comes from SafeGraph via its COVID-19 Data Consortium. Specifically, we rely on the SafeGraph Patterns data, which provides daily foot traffic estimates to individual POIs, and the Core Places data, which contains basic location information for POIs.

A.2 Election Data

Our election data comes from certified turnout results of the 2018 North Carolina general election, as collected by L2. For each registered voter, L2 provides demographic data, such as name, age, ethnicity, and voting district/precinct, as well as their voter history.

We provide some additional descriptive information about the data here. First, Figure 10 shows the correlation between age and race across polling locations. This illustrates the importance of jointly interpreting how coverage varies by age and race.

Figure 10: Non-white voters are more likely to be young

Second, the top panel of Figure 11 illustrates the density of locations by age quartile on the x-axis and race quartile on the y-axis. The two modal polling locations are for locations with white elderly populations and non-white young populations. The bottom panel displays average number of voters in these cells, showing that young, high-minority cells represent a particularly large number of voters.

A.3 Poll Location Data

Our polling location and precinct data for North Carolina for Election Day 2018 was acquired from the North Carolina Secretary of State. This dataset contains the street address for each polling place, including location name, county, house number, street name, city, state, and zip code, as well as the precinct associated with the polling location.

B DETAILS ON DATA CLEANING AND MERGING

This study required that we merge the points-of-interest (POIs) as defined by SafeGraph with the polling locations in North Carolina in 2018. To do so, we used SafeGraph’s Match Service, which takes in a POI dataset and using an undisclosed algorithm, matches it with its list of all POIs, appending the unique SafeGraph ID for all

12https://docs.safegraph.com/docs/matching-service-overview
matched POIs. The service utilizes a variety of basic information\textsuperscript{13} to determine matches; of these, we provided the location name (or polling place name), street address, city, state, and postal code for all polling locations in North Carolina in 2018. The match rate, i.e. the percentage of input polling locations SafeGraph could match with one of its POIs, was 77.6%.

The polling location dataset, now having SafeGraph IDs for each matched location, was then joined with the SafeGraph Places dataset, which contains basic information like location name and address for the POI, for comparison between the matched POI and the polling location. The SafeGraph matching algorithm was at times too lenient, matching locations near each other but with different names or matching locations with different street addresses. To remedy this, we ran the dataset through a postprocessing script which removed matches where the addresses differed by three or more words to account for false positives. This resulted in a match rate of 47.7%. We then filtered out POIs where SafeGraph returned multiple candidate matches since we could not be confident the first match was the correct match. This resulted in a match rate of 42.2%.

Finally, we mapped voters from the L2 voter file to the appropriate polling location with SafeGraph ID. The L2 voter file contains the precinct for each voter, and the polling location data associates each precinct with a polling location, so by mapping voter to precinct and precinct to polling location (and SafeGraph ID) we could fetch the polling location for each voter for which there was a match with a SafeGraph POI. We observed differences in how the polling data and L2 named the same precinct. For instance, one source may using preceding zeros "0003" whereas another may not "3" or one source may use "WASHINGTON WARD 1" whereas the second uses "WASHINGTON 1".

**B.1 Challenges for scalability**

While a key virtue of our approach is bringing in auxiliary ground truth data, the drawback is that this approach is not conducive to iterative audits over time (or geography) because of scalability challenges. Voter locations change with every election and there is no national database that collects voter location information over time. Creating the crosswalks between (a) SafeGraph POIs and voter locations and (b) voter locations and precincts in voter turnout files is a heavily manual process that differs for each jurisdiction, given the decentralized nature of election administration.

**C ASSUMPTIONS REQUIRED FOR MEASUREMENT VALIDITY**

In this section, we provide the analogue of § 4.3 and 6.2.1 for measurement validity. The assumptions required for our measurement validity analysis are much weaker than those discussed and evaluated in the main paper, but we provide the results here for completeness. To identify the relationship between ground truth visits and SafeGraph traffic, we need the following to hold:

**Assumption 3 (No induced confounding (measurement validity)).** The estimation procedure does not induce a confounding factor that affects both the estimates of ground truth visits \(T\) and the estimated marginal SafeGraph traffic \(S - Z\).

**Assumption 4 (No selection bias (measurement validity)).** The selection is not based on an interaction between factors that affect ground truth visits \(T\) and the estimated marginal SafeGraph traffic \(S - Z\).

As we do above for disparate coverage, we can partially test Assumption 3 using placebo inference (see next section). While we can test for time-invariant confounding, we cannot test for time-varying confounding. Nonetheless it is difficult to postulate a reasonable mechanism for time-varying confounding in our measurement analysis. Assumption 4 would be violated if SafeGraph coverage is better for polling location POIs versus non-polling location POIs.

**C.1 Robustness test for time-invariant confounding**

To test for time-invariant confounding in our estimation of the correlation between ground truth visits and SafeGraph visits, we consider the rank correlation between voter turnout and SafeGraph marginal traffic on non-election days. We would not expect to find a non-zero such correlation, and indeed Figure 12 shows that the positive correlation on election day is significantly outside the distribution for placebo days (empirical one-sided \(p\)-value = 0).

**D IMPUTATION OF NON-VOTER TRAFFIC**

To estimate voter traffic on election day, we estimate the amount of traffic on non-election day Tuesdays using SafeGraph Monthly Places Patterns data from January 2018 to April 2020 and subtract that estimate from the total recorded traffic on election days. In particular, to estimate the number of visits on a given non-election day Tuesday, we use the number of visits on adjacent weekdays to calculate that estimate. We took two approaches to making that estimation procedure does not induce a confounding factor that affects both the estimates of ground truth visits \(T\) and the estimated marginal SafeGraph traffic \(S - Z\).
calculation and selecting the optimal number of adjacent weekdays to base the estimate off.

In the first approach, we look at the $X$ adjacent weekdays before and after a given Tuesday and use the average of the traffic on all those weekdays to estimate the traffic on Tuesday. That is, for $X = 2$, we calculate the estimate as the average of the traffic on the Friday and Monday before and Wednesday and Thursday after a given Tuesday. We performed this calculation for all North Carolina polling locations and all Tuesdays, excluding Election Days and the first and last Tuesdays from January 2018 to April 2020, with traffic data available from the SafeGraph Patterns data, which gave us 147,613 data points. We tested $X \in [1, 4]$ as this considers all weekdays up to the next or previous Tuesday. The following are the evaluation metrics for this approach:

| $X$ | RMSE  | $R^2$ | MAE  |
|-----|-------|-------|------|
| 1   | 10.85 | 0.870 | 4.05 |
| 2   | 10.37 | 0.881 | 3.91 |
| 3   | 10.61 | 0.875 | 3.95 |
| 4   | 10.75 | 0.874 | 4.00 |

Averaging the traffic on the two weekdays before and after a given Tuesday performs best by all three evaluation metrics.

In the second approach, we used the traffic on adjacent weekdays as features for a linear regression model, to account for the possibility that traffic on certain weekdays may be more impactful in calculating an accurate estimate. With the same dataset as the one described for the first approach we used 10-fold cross validation with 3 repeats, with the following results:

| $X$ | RMSE  | $R^2$ | MAE  |
|-----|-------|-------|------|
| 1   | 10.65 | 0.872 | 4.16 |
| 2   | 9.98  | 0.888 | 3.89 |
| 3   | 9.88  | 0.890 | 3.83 |
| 4   | 9.76  | 0.893 | 3.76 |

The linear model using traffic from the four adjacent weekdays before and after the given Tuesday performed the best across both approaches, so we used this model to estimate non-voter traffic on Election Day.

We used the model to predict the number of non-voter visits to each of 806 polling location POIs on Election Day (November 6th, 2018). 10 of the POIs did not have Patterns visit count data for 4 weekdays before Election Day (October 31st, 2018), so we imputed the traffic to be the traffic 3 weekdays before (November 1st, 2018) for those POIs.

The model was also used to impute the traffic at poll location POIs on the 48 weekdays between October 1st, 2018 and November 30th, 2018. These predictions were then used to repeat the data analyses on SafeGraph coverage as an additional robustness check, producing similar results to original analysis that relied on mean imputation to adjust for non-voter traffic. Figures 13 illustrates comparable results as before.

**E ADDITIONAL RESULTS**

Table 4 presents the full regression results for Section 6.2. The first column shows that the percent of the voting population over 65 is negatively associated with coverage. The second column shows that controlling for age, an increase in the percentage of the population that is white is associated with an increase in the coverage rate. The third column fits interaction terms, for which we present the substantive interpretation in Section 6.2.
### Table 4

|                  | (1)        | (2)        | (3)        |
|------------------|------------|------------|------------|
| % over 65        | −0.037***  | −0.044***  | −0.075**   |
|                  | (0.009)    | (0.010)    | (0.033)    |
| % white          | 0.010***   | −0.0003    |            |
|                  | (0.004)    | (0.012)    |            |
| % white × % over 65 | 0.042      |            |            |
|                  |            | (0.043)    |            |
| Constant         | 0.031***   | 0.025***   | 0.033***   |
|                  | (0.003)    | (0.003)    | (0.009)    |

|                  | Observations | R²       | Adjusted R² | Residual Std. Error | F Statistic |
|------------------|--------------|----------|-------------|---------------------|-------------|
|                  | 452          | 0.034    | 0.049       | 0.018 (df = 450)    | 15.668***   |
|                  |              |          |             |                     | (df = 1; 450) |
|                  |              |          |             | 0.018 (df = 449)    | 11.639***   |
|                  |              |          |             |                     | (df = 2; 449) |
|                  |              |          |             | 0.018 (df = 448)    | 8.073***    |
|                  |              |          |             |                     | (df = 3; 448) |

Note: *p<0.1; **p<0.05; ***p<0.01

### Table 5: Linear regression models of coverage rate by demographic attributes of polling locations.