Big data, big problems: Responding to “Are we there yet?”

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Abstract

Bradley et al. (2021), as part of an analysis of the performance of large-but-biased surveys during the COVID-19 pandemic, argue that the data defect correlation provides a useful tool to quantify the effects of sampling bias on survey results. We examine their analyses of results from the COVID-19 Trends and Impact Survey (CTIS) and show that, despite their claims, CTIS in fact performs well for its intended goals. Our examination reveals several limitations in the data defect correlation framework, including that it is only applicable for a single goal (population point estimation) and that it does not admit the possibility of measurement error. Through examples, we show that these limitations seriously affect the applicability of the framework for analyzing CTIS results. Through our own alternative analyses, we arrive at different conclusions, and we argue for a more expansive view of survey quality that accounts for the intended uses of the data and all sources of error, in line with the Total Survey Error framework that have been widely studied and implemented by survey methodologists.

1 Introduction

Bradley et al. (2021), in their article “Are We There Yet? Big Surveys Significantly Overestimate COVID-19 Vaccination in the US”, use recent surveys of COVID-19 vaccination hesitancy and uptake to establish what they call a paradox: “the two ‘big surveys’ are far more confident, yet also far more biased, than the smaller, more traditional Axios–Ipsos poll.” Using the framework established by Meng (2013), they examine the possible sources of error in the surveys, such as sampling bias, sample size, and population heterogeneity. They conclude that for two of the surveys, sampling biases “can almost completely wipe out the statistical information” in the data, rendering suspect all point estimates and even correlations calculated using the survey data.

We are the designers and operators of one of the surveys discussed by Bradley et al. (2021): the COVID-19 Trends and Impact Survey (CTIS), operated in the United States by the Delphi

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1 We refer specifically to version 2 of their arXiv preprint, available at https://arxiv.org/abs/2106.05818v2. Because their work is under revision, future versions may differ.
Group at Carnegie Mellon University in collaboration with Facebook. (The survey is called “Delphi-Facebook” by Bradley et al. (2021).) This survey invites a random sample of participants from Facebook’s Active User Base each day, amounting to about 250,000 responses in the United States per week. The survey has operated continuously since April 6, 2020, collecting over 22 million responses during that time and allowing long-term tracking of COVID trends. Its sampling and weighting methodology is discussed in more detail by Kreuter et al. (2020), Barkay et al. (2020), and Salomon et al. (2021); further documentation and public aggregated datasets are available at https://cmu-delphi.github.io/delphi-epidata/symptom-survey/.

Bradley et al. (2021) correctly note that CTIS systematically overestimates COVID-19 vaccination rates in the United States, and we agree that users of any large dataset should be aware that size is no guarantee of accuracy. However, we disagree with their broader conclusions for several reasons:

1. As CTIS’s name suggests, its design goals are to facilitate rapid detection of trends—such as sudden increases in symptom rates that may indicate a COVID hotspot—and to do so at a fine geographic level; the large sample size is necessary for these goals. These goals can be met even with systematic biases, provided the biases do not change or only change slowly over time. Our results indicate the survey has been largely successful in this goal.

2. Bradley et al. (2021) correctly note that observed increases in the data defect correlation (ddc), the measure of “data quality” in the framework described by Meng (2018), do not necessarily imply a change in the sampling mechanism or an increase in sampling bias. But this point is worth greater emphasis, as it undermines the argument that the ddc can play a useful role in the evaluation of large surveys. In fact, analyses of CTIS survey data show that it predicts both COVID case rates and COVID vaccination rates much more accurately than a simple ddc analysis would show. For important problems such as geographic resource allocation, its effective sample size is thousands or tens of thousands of responses per week.

3. The data defect correlation framework is limited when applied to surveys because it does not account for sources of survey error beyond sampling biases. Self-reports from survey respondents can be biased for many reasons, and the ddc is no longer easily interpretable when any such biases are present. Though Bradley et al. (2021) argue the ddc is related to the design effect in this setting, we believe this is incorrect, and that it is unclear how to interpret the ddc or how to use it to improve survey quality. This problem may also affect the scenario analyses they present in Section 6.

4. More broadly, while we agree with Bradley et al. (2021) that big surveys are not a panacea, we believe that there is no one number that summarizes the suitability of any survey for any purpose. The data defect correlation can be useful in some cases, but it does not help judge the suitability of a survey for tracking trends or for other purposes. Anyone planning to use a dataset, large or small, must carefully consider its suitability for their specific research goals, which may differ from those explored in the framework established by Meng (2018).
So while Bradley et al. (2021) are correct to urge caution in the use of large but biased datasets, we believe it is important to provide additional context about our research goals, and to provide more actionable insights for researchers evaluating large survey datasets. We discuss these points in greater detail in the sections that follow.

2 Background

2.1 CTIS design goals

Since the value of a dataset depends on the research goals for which it is used, it may be helpful to discuss the original goals of the COVID-19 Trends and Impact Survey (CTIS). The Delphi Group at Carnegie Mellon University had prior experience with influenza forecasting, often using both standard public health surveillance data (such as reports of influenza-like illness from outpatient doctor’s visits) and unconventional signals, such as volumes of symptom-related Google search queries (Farrow, 2016, Chapter 4). This approach had been successful with influenza, and Delphi’s forecasts were among the most accurate in annual forecasting challenges conducted by the Centers for Disease Control (Lutz et al., 2019, Table 3).

The CTIS effort began in March 2020, as efforts to track the COVID pandemic accelerated. Delphi approached Facebook about the possibility of using its platform to host symptom surveys that could identify rises in symptoms that may precede increases in confirmed cases, providing early warning of COVID case increases. Facebook agreed, and the effort quickly expanded to include an international version of the survey, hosted by the University of Maryland. The instrument also gained additional items on social distancing, mental health, comorbidities, and other topics of public health interest. It has operated continuously since its launch, and aggregate data is released publicly every day, typically within 1–3 days of survey responses being received.

The survey’s emphasis on tracking and hotspot detection informed its design. To detect COVID hotspots, it would need to sample responses continuously so changes in symptom rates could be detected within days. To localize these hotspots to cities or counties, and thus facilitate public health responses, it would need large sample sizes to ensure power to track trends separately for each major city or county. Hence the sampling design ensured a response volume of tens of thousands of responses per day.

This response volume could not help the survey accurately estimate the population prevalence of COVID-19, but that was never the intention. In the spring of 2020, the rate of asymptomatic cases of COVID-19 was unknown; even if it was known, COVID-19 can present with many different symptoms, most of which could easily be caused by other illnesses or even seasonal allergies. A symptom survey would suffer from many sources of measurement error. But if symptom rates suddenly increased, that could be a sign that public health officials should be prepared for a case increase—even if no point estimate of prevalence could be made.

This hypothesis was borne out by experience. Using the survey, we estimated rates of COVID-like illness (CLI), as well as the percentage of respondents who reported knowing someone in their local community who was currently sick (called “CLI-in-community”). In Figure 1 we see the correlation
Figure 1: Correlation between the rate of CTIS respondents who know someone who is currently sick (“CLI-in-community”) and officially reported rates of COVID-19 cases (both as 7-day averages). Left: On each date, correlation between CTIS estimates of CLI-in-community and state reports of new cases. Right: For each state, correlation between the time series of CLI-in-community and of reported cases.
between CLI-in-community and official state reports of confirmed COVID cases. For much of the pandemic, this correlation was quite strong, only dipping during the winter of 2020 before recovering to again be quite high. In fact, Reinhart et al. (2021, Figs. 2 and 3) showed that, for much of the pandemic, these CLI-in-community estimates correlated more strongly with reported COVID cases than several other indicators, including some based directly on medical claims from doctor’s offices. McDonald et al. (2021) further demonstrated that CLI estimates from the survey can be useful in near-term COVID case forecasting, suggesting they contain information beyond what is already available in public case reporting and hotspot detection. Survey data is used to inform COVID-19 case and death forecasts submitted by several teams to the CDC-sponsored COVID-19 Forecast Hub (Cramer et al., 2021), including forecasts by Delphi and by Rodríguez et al. (2021).

In short, the sample size of CTIS was chosen to ensure it could meet its goals, not to somehow compensate for potential weaknesses of its Facebook-based sampling mechanism. Experience has shown that CTIS is useful for these goals. As the pandemic progressed, data from CTIS also began to be used for numerous other goals. For example, Bilinski et al. (2021) studied the public reaction to increasing infection rates; Sudre et al. (2021) explored the strong association between self-reported anosmia and test positivity (while showing consistency between CTIS results and data collected through other means); and Lessler et al. (2021) examined associations between in-person schooling, mitigation measures, and COVID-19 test positivity. Nonetheless, we agree that analysts using the data must be aware that it is best used for specific goals; as Kreuter et al. (2020) wrote when introducing the survey,

In order to minimize the impact of various sources of error, we recommend analysts focus on temporal variation. Although estimates for a single point in time may be affected by many error sources (stemming from both sample selection and measurement procedures) these errors are likely to remain constant over relatively short periods of time, thereby producing unbiased estimates of change over time (Kohler, Kreuter, & Stuart, 2019).

Our focus on changes over time and on spatial comparisons, as in Figure 1, would be valid if the survey’s sampling bias indeed remains relatively constant over time and space. We will examine this in Sections 3 and 4 after first reviewing the data defect framework used by Bradley et al. (2021).

2.2 The data defect correlation framework

It will be helpful to review the error decomposition originally introduced by Meng (2018) and used by Bradley et al. (2021) to analyze the errors in each survey’s estimates.

Consider a population of individuals $i = 1, \ldots, N$. Suppose there is some variable of interest $Y_i$ that could be observed for each member of the population; its population average is denoted $\bar{Y}_N$. Now suppose we take a random sample of size $n$ from this population, and use the indicator

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2We speculate that this is at least partly due to the decline in reported cases in January 2021, resulting in there being less signal and more noise—i.e., there was less heterogeneity between states. We will return to this point in Section 3.
variable $R_i \in \{0, 1\}$ to denote whether person $i$ was included in the random sample. Once we take the sample, we calculate the sample mean, which can be written as

$$\bar{Y}_n = \frac{\sum_{i=1}^{N} R_i Y_i}{\sum_{i=1}^{N} R_i}.$$ 

Now let $\hat{\rho}_{R,Y} = \text{corr}(R_i, Y_i)$ be the correlation between sampling and the random variable in the finite population, let $f = n/N$, and let $\sigma_Y$ be the population standard deviation of $Y_i$. Then Meng (2018) showed that one can decompose the estimation error of the sample mean as follows:

$$\bar{Y}_n - \bar{Y}_N = \hat{\rho}_{R,Y} \times \sqrt{\frac{1 - f}{f}} \times \frac{\sigma_Y}{f}.$$ 

(1)

The data quality term is the data defect correlation; ideally this correlation is nearly 0. By rearranging eq. (1), we obtain

$$\hat{\rho}_{R,Y} = \frac{\bar{Y}_n - \bar{Y}_N}{\sqrt{\frac{\sigma_Y^2 (1 - f)}{f}}}.$$ 

(2)

If the population quantity $\bar{Y}_N$ is known, this can be used to estimate the data defect correlation. The ddc results presented by Bradley et al. (2021) use Centers for Disease Control data on COVID-19 vaccine uptake as the ground truth, and plug in estimates $\bar{Y}_n$ from various surveys.

3 CTIS estimates across states

Bradley et al. (2021) question whether a large but biased survey can facilitate accurate geographic comparisons, even at the state level. Their Figure 1 shows a weak correlation between CTIS estimates of vaccine uptake on March 27, 2021 and official CDC data on state-level vaccine uptake, and this serves as supporting evidence for their conclusion that (page 21):

Selection bias tells us that respondents are not exchangeable with non-respondents, and hence it may impact all studies of that dataset to varying degrees. This includes study of associations – both Delphi-Facebook and Census Household Pulse significantly overestimate the slope of vaccine uptake over time relative to that of the CDC benchmark (Fig. 2) – as well as ranking – the Census Household Pulse and Delphi-Facebook rankings are more correlated with each other ([Kendall] rank correlation: 0.49), than either ranking is with that of the CDC (0.21 and 0.26, respectively), as indicated in Fig. 1.

In other words, because of the survey biases that presumably vary across space, even studies of associations, such as state-level associations with factors that might explain vaccine uptake, are suspect.
Figure 2: CDC-reported vaccination rates compared to CTIS estimates for each state. March 27th corresponds to Figure 1 of Bradley et al. (2021), while the later dates show the rapid uptake of vaccines—and the increasing correlation between the survey and CDC data.

But these correlation results are based on vaccine uptake data as of March 27, 2021, several weeks before all adults in the United States became eligible to receive vaccines. Because only seniors, healthcare workers, and certain other high-risk and essential groups were eligible to receive vaccines, true vaccination rates were quite low. In early April, all adults became eligible to receive a vaccination, and vaccination rates rose rapidly over the next six weeks. In Figure 2, we see that the relationship between CTIS estimates and CDC data firmed up substantially over this time.

Figure 2 hence raises two questions:

1. How did CTIS perform over time? The plots suggest it began to correlate well with CDC data as states began to diverge, but can we quantify this?

2. Is the lack of correlation in March due to spatial bias or due to the difficulty of the problem?

We will examine each in turn.

### 3.1 Comparison with CDC data over time

We can make comparison in Figure 2 more concrete by considering the Kendall correlation between CTIS estimates and CDC data over time, as the vaccination campaign progressed. (We chose Kendall correlation to match the analyses performed by Bradley et al. (2021).) The left panel of
Figure 3: **Left:** On each date, correlation between CTIS state-level estimates of vaccination rate and CDC data. March 27 is indicated with a red line. **Right:** For each state, correlation between CDC and CTIS vaccination rate time series. Each time series shows high correlation, most over 0.8, indicating that CTIS tracks trends well. Wyoming shows the lowest Kendall correlation, 0.66.

Figure 3 shows this correlation, and shows a dramatic increase in the correlation once vaccinations became available to the entire adult population. By early May, the correlation reached 0.8, a much stronger relationship than just a month earlier. In the right panel, we see that the correlation between each state’s vaccine uptake time series and its time series of survey estimates is also quite high, highlighting the survey’s ability to track trends.

There are several interesting points to take away from this. First, the utility of CTIS data for comparisons between states evidently increased over this time period—despite Figure 3 of Bradley et al. (2021) showing the ddc increasing over the same time period, implying decreasing data quality. Though the ddc is intended to serve as an “index of data quality,” this suggests that a different framework would be needed to assess the utility of surveys for tasks other than point estimation.

Second, though the ddc decomposition includes a term for “problem difficulty,” that term does not quantify the difficulty of this problem. The error decomposition shown in eq. (1) is for the error in estimating a population mean, and the problem difficulty term is \( \sigma_Y \), the standard deviation of the quantity in the population. But in this problem, the error \( \bar{Y}_n - \bar{Y}_N \) is irrelevant; the goal is to estimate the ranking of states to determine which states have the lowest vaccination rates. (We could imagine using this, or a similar ranking for vaccine hesitancy, as part of decisions for prioritizing government resources to encourage vaccination.) The difficulty of the ranking problem depends on the heterogeneity of the states; if all states have similar vaccination rates, ranking them well requires
very tight margins of error.

Let’s examine the problem difficulty in more detail, since it will illuminate the value and weaknesses of the CTIS results.

3.2 Ranking difficulty and CTIS’s biases

One way to quantify the difficulty of a survey estimation problem is to ask: What size of random sample would be required to achieve our desired result, on average? This is the goal of the “effective sample size” $n_{\text{eff}}$ calculated by Bradley et al. (2021), which is “the size of a simple random sample that we would expect to exhibit the same level of error as what was actually observed in a given study” (page 9). If the level of error observed is known, for example if we can compare the survey results to a gold standard, the $ddc$ decomposition allows $n_{\text{eff}}$ to be estimated. Bradley et al. (2021) calculated that in the case of CTIS, “a biased national sample of 250,000 contains no more usable information than a simple random sample of size 20” (page 22).

But the estimation problem here is not point estimation. If we are interested in prioritizing resources between states, the appropriate question is “What size of simple random sample would we expect to exhibit the same rank correlation with the truth as what was actually observed in a given survey?” We can already see that $n_{\text{eff}} = 20$ is not the answer to this question, because a sample of size 20 from the United States population cannot provide a ranking of 50 states, let alone one that correlates strongly with the true ranking.

We conducted a simulation study to explore this point further. In each simulation, we drew a random sample from the United States population, drawing a simple random sample from each state with size in proportion to its proportion of the total population. Within each state, we simulated a survey estimate of the state vaccination rate; this estimate was centered at the CDC-estimated vaccination rate and drawn from a sampling distribution based on the state sample size. The resulting estimates were then correlated with the CDC ground truth data, and this procedure was repeated 1,000 times to yield an average correlation.

Figure 4 shows the results of this simulation over a range of national sample sizes. We can see that on March 27—again, according to Figure 3—the date where CTIS has nearly its worst correlation—a sample of nearly 4,000 respondents would be required to match CTIS. And even a sample of $n = 20000$ respondents would, on average, only barely reach a correlation of 0.5 with the truth, because states had similar vaccination rates and were difficult to rank.

We would expect this to vary over time, since the problem difficulty changed as state vaccination rates became more heterogeneous. Indeed, Figure 5 shows how the sample size needed to match CTIS’s correlation changed each week, showing that early on—when all states had very low vaccination rates—a random sample of size over 70,000 would be required to rank the states as accurately as CTIS did, while by summer, a sample of size 15–20,000 would be required instead.

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3 A complete analysis of the data’s utility for resource allocation would be more complicated: for example, if states have very similar vaccination rates, an error in prioritization would be less harmful than an error when some states have dramatically lower rates than others.
Figure 4: On March 27, 2021, the expected Kendall correlation between state-level survey estimates and CDC data, if the state estimates were obtained from a random sample of the United States population. CTIS’s correlation on that date is shown in red.

Figure 5: For each week, the random sample size needed to match CTIS’s Kendall correlation with state-level CDC data.
So when Bradley et al. (2021) note that large surveys correlate poorly with ground-truth data, we must first ask “How hard is this problem?” We find that the ddc decomposition does not answer this question correctly, and that the ranking problem was actually quite hard, requiring even an unbiased random sample to be quite large to perform well. Only with this answer in hand can we evaluate survey performance. We conclude that while CTIS’s sample of size 250,000 does not perform as well as a random sample of the same size would, it performs much better than one would expect from the analysis presented by Bradley et al. (2021). This implies the spatial variation in sampling bias is not as severe as claimed, and the claim that “a biased national sample of 250,000 contains no more usable information than a simple random sample of size 20” (page 22) does not apply to the ranking task. We believe this claim only applies to the narrower task of unbiased population point estimation.

Returning to our main argument, we see that Figure 2 hints at the reason for these problems. The ddc is defined in terms of the correlation of two random variables, but the correlation depends on both the relationship between the variables and their marginal distributions. Vaccination was rapidly increasing over this time period, and so was the difference between states: in March, the range in true vaccination rates was limited—between 20 and 40%—but that range expanded dramatically over six weeks. This shift in marginal distribution caused both an increase in the Kendall correlation, and hence the utility of the survey for making comparisons and allocating resources, and an increase in the ddc. We will explore this problem in more detail in the next section when we explore biases over time.

4 Is the bias increasing over time?

Besides making comparisons between states, we have also advocated for CTIS’s use in tracking trends over time. Though we have shown this strategy to be quite successful for tracking COVID case rates, Figure 3 of Bradley et al. (2021) suggests that both the Census Household Pulse and CTIS surveys experience decreasing data quality, as measured by the data defect correlation (ddc), over time. Page 12 notes that:

This decomposition suggests that the increasing error in estimates of vaccine uptake in Delphi–Facebook and Census Household Pulse is primarily driven by increasing ddc, which captures the overall impact of the bias in coverage, selection, and response.

They reach this conclusion by comparing CTIS and Household Pulse estimates of COVID-19 vaccine uptake to CDC data on vaccine distribution, which is reasonably comprehensive and reliable. Both surveys overestimate vaccination rates, and the size of the difference increases over time. But this does not imply that the sampling bias is increasing, as they note:

However, this does not necessarily imply a change in the response mechanism, because an identical response mechanism can result in a different ddc as the correlation between that mechanism and the outcome changes, e.g., an individual’s vaccination status $Y$ changes over time.
We think it is useful to explore the consequences of this caveat in more detail, since it limits how well the \textit{ddc} can be used as an “index of data quality.”

Why could \textit{ddc} be increasing for these surveys? Crucially, the underlying population quantity—the percentage of American adults who are vaccinated—is monotonically increasing. For a fixed sampling bias, in a situation where respondents who are willing to be vaccinated are more likely to respond to a survey, the estimation bias monotonically increases even if the probability of responding is held fixed for every member of the population. Hence the \textit{ddc} increases even if the data’s quality is fixed. A simple model will help us demonstrate this point.

4.1 Demonstration

Consider a simplified example. Let $I$ be the indicator function, and let $V = \mathbb{I}(\text{person is vaccinated})$. Let $R = \mathbb{I}(\text{responds to survey})$. Let the population be composed of two groups of people, denoted by $G \in \{1, 2\}$. These groups have both different probabilities of responding and different probabilities of being vaccinated:

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\begin{align*}
\Pr(G = 1) &= \eta \\
\Pr(G = 2) &= 1 - \eta \\
\Pr(V = 1 | G = 1) &= \rho(t) \\
\Pr(V = 1 | G = 2) &= \frac{\rho(t)}{b} \\
\Pr(R = 1 | G = 1) &= 0.02 \\
\Pr(R = 1 | G = 2) &= 0.02 / \gamma,
\end{align*}
\]

where

- $b \geq 1$ is the differential vaccination rate: if $b = 2$, members of group 2 are half as likely to be vaccinated as members of group 1

- $\gamma \geq 1$ is the differential response rate: if $\gamma = 2$, members of group 2 are half as likely to respond to the survey as members of group 1

In line with typical response rates for online surveys on the Facebook platform, 2% of group 1 responds. Crucially, the vaccination rate of group 1 is $\rho(t)$, which depends on time $t$ as more vaccines become available. Assume that $R \perp V | G$, i.e. $G$ is the only determinant of vaccination or response probability.

If we conduct a survey of this simplified population, what would be the bias in the survey’s estimated vaccination rate, as a function of the true rate? A survey of this population would estimate the quantity $\Pr(V | R = 1)$, and the estimation bias is $\Pr(V | R = 1) - \Pr(V)$. We can see that when $\rho(t) = 0$, this bias must be zero, since all respondents would say they are not vaccinated. (We are ignoring measurement error in this example.)

We can use Bayes’ rule and algebra to work out this bias as a function of $\rho(t)$ (see Appendix A for details). Assuming a population of size 250,000,000 and sample of size 30,000 (roughly matching the United States adult population and a typical daily sample size for CTIS in 2021, respectively), Figure 6 illustrates how the \textit{ddc} varies with $\rho(t)$ when $b = 2$, $\gamma = 4$, and $\eta = 0.5$. (These values were chosen because one might expect about half the adult population to harbor views skeptical of vaccines.) This demonstrates that, with no change in response probabilities, the \textit{ddc} can increase for a survey where vaccine uptake is associated with response, even if the mechanism for that association
Figure 6: The data defect correlation ($ddc$) as a function of $\rho(t)$, for the simple example in Section 4.1. Here $b = 2$ and $\eta = 0.5$.

is fixed. Notably, the scale of $ddc$ values shown in Figure 6 is similar to the scale of those in Figure 3 of Bradley et al. (2021), illustrating that a moderate but fixed sampling bias is consistent with the changing $ddc$ presented there.

4.2 Analysis in the observed data

The model in section 4.1 is simplified: it does not account for any form of measurement error in the survey questions (which we will discuss in Section 5). But it still provides an interesting point of comparison. The model predicts a linear relationship, with slope greater than 1, between the vaccination rate and the survey estimates (see Appendix A), and we can look at this relationship in the observed data easily. Figure 7 shows the results.

For the first several months of the vaccination campaign, the relationship appears quite linear, with different slopes for each state. This matches what we could expect in the simplified model if the response mechanism was fixed in time but varied between states: the slope is a function of $\gamma$, $\eta$, and $b$, which we might reasonably expect to vary by state. Hence this does not indicate the presence of biases that change over time.

Only later, in April and May, do the slopes begin to decrease, reducing the gap between survey estimates and CDC data. This could reflect shifts in the response mechanism that reduce the bias; for example, perhaps some hesitant respondents became more willing to both become vaccinated and respond to the survey. This may also reflect COVID vaccines being made available to children 12 years and older beginning in early May; CTIS only samples respondents 18 and older and hence
Figure 7: Official CDC estimates of population vaccination rates at each date and CTIS estimates of adult vaccination rate on the same date. Each line represents one US state or territory over time.

cannot track vaccination in this group, though CDC data includes all vaccinations. The slope change appears to correspond to the time period for which Bradley et al. [2021] Fig. 3) showed CTIS’s ddc decreasing.

4.3 Implications

First, these results mean that the ddc should not be interpreted as representing the survey bias or data quality per se. We can see that by remembering that the ddc is a correlation between a random variable $V$ and each population member’s survey response indicator $R$:

$$\text{ddc} = \rho_{V,R} = \text{corr}(V, R).$$

Just like any correlation, if $V$ is concentrated around one value, it will be smaller than if $V$ has a larger range; that is to say, the ddc depends on both the sampling bias and the marginal distribution of $V$, and comparisons of ddc between time points or populations with different distributions of $V$ are comparisons of both sampling bias and $V$.

This suggests that one cannot compare surveys in different locations or time periods, or measuring different quantities, by comparing their ddc values; or at least that such comparisons would not reveal which survey is more methodologically sound. It also suggests that there are other explanations for the ddc’s changes over time than the one presented on page 49:
Delphi-Facebook’s ddc is higher overall, and shows a stark divergence between the two age groups after March 2021. The ddc for seniors flattens and starts to decrease after an early March peak, whereas the error rate for younger adults continues to increase through the month of March 2021, and peaks in mid-April, around the time at which all US adults became eligible.

This is consistent with the hypothesis that barriers to vaccine and online survey access may be driving some of the observed selection bias in Delphi-Facebook. Early in the year, vaccine demand far exceeded supply, and there were considerable barriers to access even for eligible adults, e.g., complicated online sign-up processes, ID requirements, and confusion about cost.

This is also consistent with the observation that vaccination rates among seniors increased rapidly through March, but that the increase slowed in March as most willing seniors were already vaccinated, causing the ddc to stop rising. Meanwhile, vaccination rates among younger adults rose quickly for a month or two longer as eligibility expanded. (The drops in ddc at the end of the time period are not well-explained by either hypothesis.) It is not clear to us how we could distinguish between these two hypotheses using the survey data alone.

Finally, the relatively consistent bias shown in Figure 7 suggests that the survey bias changed only slowly over time. This matches the survey’s design goal of tracking trends over time and detecting sudden changes. The bias does differ by state (or, more likely and more precisely, by local demographic factors), explaining why the state-level survey estimates do not perfectly correlate with CDC data.

5 Limitations of the data defect framework for surveys

Above, we have argued that the data defect correlation measures only one aspect of a dataset’s suitability for purpose, that it is difficult to interpret when population quantities are changing, and that the COVID-19 Trends and Impact Survey has proven suitable for several important purposes not considered by Bradley et al. (2021). These are important points to consider, but there lurks a more fundamental problem: the error decomposition given by Meng (2018) does not directly apply to surveys subject to measurement error.

5.1 Interpretation of the ddc with measurement error

Bradley et al. (2021) estimated the ddc using eq. (2) by substituting CDC data, assumed to be reliable, for $\tilde{Y}_N$ and substituting survey estimates for $\tilde{Y}_n$. But a survey does not obtain $Y_i$; it asks real humans for $Y_i$, and real humans may interpret the question differently than intended, misremember events, feel pressured to give particular answers, or deliberately lie (Tourangeau et al., 2000). Let $Y_i^*$ denote the response of subject $i$ to a survey question asking them about $Y_i$. The relationship of $Y_i^*$ to $Y_i$ may be complex and depend on individual-level features, and the survey estimator is the sample mean $\bar{Y}_n^*$, not $\tilde{Y}_n$.
As Meng (2018, page 691) noted, the error decomposition does not apply to the error $\bar{Y}_n - \bar{Y}_N$ if there is measurement error:

Statistically, [the decomposition] is applicable whenever the recorded values of $[Y]$ can be trusted; for example, if a response is to vote for Clinton, it means that the respondent is sufficiently inclined to vote for Clinton at the time of response, not anything else. Otherwise we will be dealing with a much harder problem of response bias, which would require strong substantive knowledge and model assumptions [see, e.g., Liu et al. (2013)]. See Shirani-Mehr et al. (2018) for a discussion of other types of response bias that contribute to the so-called Total Error of survey estimates.

Bradley et al. (2021, page 10) acknowledge this point, but state that in this setting, the data defect correlation “becomes a more general index of data quality directly related to classical design effects,” justifying its use to evaluate the surveys. However, we do not believe this is true when systematic measurement error is present. We must examine the derivation in their Supplementary Material B.1 to spot the limitation.

Suppose we use the sample mean of respondent reports $Y_i^*$ to estimate $\bar{Y}_n$; denote this sample mean by $\bar{Y}_n$. The total survey error is hence $\bar{Y}_n - \bar{Y}_N$. If we plug this into eq. (2) and rearrange, we obtain

$$\hat{\rho}_{R,Y} \sqrt{N} = \frac{\bar{Y}_n^* - \bar{Y}_N}{\sqrt{(1-f)\sigma_Y^2/n}}. \tag{3}$$

We denote the quantity in eq. (3) as $Z$; Bradley et al. (2021) argue that “the expectation of $Z^2$ with respect to $R$ (if it is random) is simply the well-known design effect.” The design effect is simply the variance of an estimator (under whatever sampling strategy is used) divided by the variance of a simple random sample estimator applied to the same population:

$$\mathbb{E}[Z^2] = \mathbb{E}\left[\frac{(\bar{Y}_n^* - \bar{Y}_N)^2}{(1-f)\sigma_Y^2/n}\right] = \frac{n}{(1-f)\sigma_Y^2} \mathbb{E}\left[(\bar{Y}_n^* - \bar{Y}_N)^2\right].$$

The first term is the reciprocal of the simple random sample variance when sampling from a finite population; the second term is the variance of $\bar{Y}_n$, if and only if $\mathbb{E}[\bar{Y}_n^*] = \bar{Y}_N$. Hence $Z^2$ is related to the design effect if and only if our estimator suffers no systematic measurement error. In a real survey instrument, this is unlikely.

One can also see this problem through two trivial examples:

1. Suppose 50% of the population is vaccinated, a simple random sample is taken, and all sampled individuals respond via in-person visits from an interviewer. But the interviewer is extremely intimidating, making 100% of respondents report that they are vaccinated. The design effect will be zero (because, regardless of the sample, the sample mean will always be 100%), $\text{corr}(Y, R)$ will also be nearly zero (because it is a simple random sample without nonresponse bias), but $\hat{\rho}_{R,Y}$ will be large when estimated by eq. (2).
2. Suppose 50% of the population is vaccinated and the survey is voluntary, but it is only completed by vaccinated individuals, and the question text is confusing and causes half of respondents to interpret it backwards. The sample estimate will hence be 50% on average. The design effect will be nearly one (since we are really estimating what proportion of vaccinated people misread the question, and the population mean of that quantity is the same as the population vaccination rate) and $\hat{\rho}_{R,Y}$ will be nearly zero, even though $\text{corr}(Y, R)$ is large.

As a result, if we plug the total error from a real survey into eq. (2) and obtain $\hat{\rho}_{R,Y}$, the result is neither $\text{corr}(R, Y)$ nor is it connected to the survey design effect. Bradley et al. (2021) conducted scenario analyses to study the effects of survey biases on estimates of vaccine hesitancy (their Section 6), but these scenario analyses relied on the assumption that $\hat{\rho}_{R,Y} \approx \text{corr}(R, Y)$ (see their Appendix E.1). Because of the limitations above, this assumption need not be true, though it is unclear to us how this will affect the results of their scenario analysis; further analytical work and simulations may be needed to determine if the scenario analysis approach is viable.

In sum, the problem we discussed in Section 4 is magnified: increasing ddc does not imply increasing sample bias, nor does it necessarily imply increasing correlation between the sampling mechanism and the population quantity. It only implies increasing total error, which could come from several sources, and a small total error could still mask serious sampling or measurement problems.

5.2 The total survey error framework

We have seen that researchers considering using survey data to answer their research questions must carefully consider whether the data is appropriate, and that the ddc neither answers that question directly (except for the very specific goal of population point estimation) nor helps researchers identify the types of error in a survey that might affect their work. Fortunately, survey methodologists have developed more complete frameworks for describing sources of errors in surveys, along with methods for evaluating how each might affect a survey.

Researchers using survey data can apply the Total Survey Error (TSE) framework (Biemer, 2010; Biemer et al., 2003) in order to understand potential sources of error and how they could affect the research questions at hand. The TSE framework breaks down possible sources of error into representation errors (including sampling errors) and measurement errors. Measurement errors include problems of validity (when the question text measures a different concept than was intended by the researcher), measurement error (when errors occur in data collection due to problems such as inaccurate translations or misunderstanding by respondents), and processing error (such as data cleaning and coding errors).

In the case of COVID-19 vaccination rates, processing error could affect both survey estimates and benchmark estimates from official sources, due to differences across jurisdictions in reporting structures and timelines. But measurement error in particular could play an important role in the differences observed in CTIS and Census Household Pulse on the one hand and Axios-Ipsos on the other.
For example, consider the question text used by each instrument to ascertain the respondent’s vaccination status:

**Axios-Ipsos** Do you personally know anyone who has already received the COVID-19 vaccine?

- Yes, I have received the vaccine
- Yes, a member of my immediate family
- Yes, someone else
- No

**CTIS** Have you had a COVID-19 vaccination?

- Yes
- No
- I don’t know

**Census Household Pulse** Have you received a COVID-19 vaccine? 

- Yes
- No

Notice that CTIS and Census Household Pulse have very similar items—the CTIS item was designed to be similar to Household Pulse so it could be interpreted alongside other Household Pulse data—while the Axios-Ipsos item is somewhat different. The key question is whether these questions are interchangeable for the purpose of estimating vaccine uptake, or whether some respondents might answer these questions differently. Unfortunately that is a question that can only be answered empirically, not simply from mathematical theory, and we do not have a direct comparison of the different versions on survey instruments given to the same population.

One could speculate, however, that social desirability bias is mitigated by the Axios-Ipsos version of the question. Social desirability bias can occur whenever respondents feel pressure to give socially acceptable answers, and often occurs on surveys of sensitive topics. The amount of social desirability bias can be affected by small changes in wording that make respondents feel more or less comfortable answering (Krumpal, 2013). For example, perhaps respondents feel uncomfortable admitting they are unvaccinated, but are more comfortable if they can say they at least know someone else who is vaccinated. Another possible explanation is related to the phenomenon of acquiescence, in which survey respondents preferentially give positive or agreeable answers (Krosnick, 1999); the Axios-Ipsos wording, by giving respondents the opportunity to say “Yes” without saying they have personally been vaccinated, would reduce the effect of acquiescence on estimates of vaccination rate.

We do not mean to suggest these are the right explanations for the difference between survey estimates; they are merely two explanations of many possible explanations. These hypotheses are at

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4Beginning in Phase 3.2, on July 21, 2021, this text changed to “Have you received at least one dose of a COVID-19 vaccine?”
least consistent with the observed data, which shows CTIS and Census Household Pulse’s estimates matching each other more closely than they match Axios-Ipsos, but there could be many other explanations, including in their sampling frames, their use of incentives (Singer et al., 2013), their nonresponse biases, the position of the questions relative to other questions, or their weighting procedures (Groves et al., 2010). The data defect correlation, oblivious as it is to sources of measurement and specification error, would not help us assess these possibilities. Additional detailed empirical study is required, possibly including experiments using different question text or different sampling strategies.

6 Discussion

A natural conclusion of the argument made by Bradley et al. (2021) might be that users of large-scale survey data subject to sampling biases should carefully examine its suitability for their research goals, for example by applying the total survey error framework (Biemer et al., 2003, chapter 2), rather than blindly assuming that a large sample size guarantees suitability. We would endorse this conclusion, which is why it is surprising that Bradley et al. (2021) do not reach it. In fact, they cite a prominent example of this caution, noting that “Delphi–Facebook is a widely-scrutinized survey that, to date, has been used in 10 peer-reviewed publications, including on important topics of public policy such as mitigation strategies within schools” (page 22). This is a reference to Lessler et al. (2021), who used CTIS data to track the proportion of children attending in-person schooling over time, and the mitigation measures applied by each school district. But they did not use the data unaware of its limitations, as one might assume based on the brief description given; instead, in preceding work, Lupton-Smith et al. (2021) carefully compared schooling data from CTIS to two other schooling datasets that relied on direct data collection from individual school districts, and hence provided benchmarks. Their caution was rewarded: they found good consistency between the datasets and carefully examined limitations in the survey data, strengthening their subsequent research.

We believe that large and nontraditional datasets, such as administrative records and massive surveys, have an increasing role to play in modern science—but that this role must be balanced with a keen awareness of their limitations. While we believe that Bradley et al. (2021) have made important points deserving of consideration by anyone using a large dataset, we also believe they have not gone far enough. Because of the limitations of the data defect correlation framework, it does not provide researchers adequate tools to evaluate large survey datasets in the context of their specific research goals. General comments about big data’s failings do not help researchers fix them, and in the face of such warnings, those with an important question and relevant big data will still be tempted to use the data. We must provide them tools such as the data defect correlation, but also other tools to estimate measurement error, measure sampling bias, improve weighting, estimate the effects of potential biases, and so on. As we have shown here, a single metric (like the \( ddc \)) cannot do this alone.

We welcome further work on tools and frameworks to study and characterize large datasets, and we encourage the broader statistics and survey science communities to work together to address
these challenges. There is a need for additional tools to understand all components of total survey error (Groves et al., 2010). We believe that a key lesson of ongoing debates in the statistics community—over reproducibility, hypothesis testing, \( p \) values, and so on—is that scientific practice is improved by engaging constructively with scientists to equip them with appropriate tools, rather than through post-hoc criticism of their results.

A Derivation of bias example

In this appendix, we derive some of the results underlying the simple model in Section 4.1. In that model, we have that the true population vaccination rate is:

\[
Pr(V) = Pr(V \mid G = 1) Pr(G = 1) + Pr(V \mid G = 2) Pr(G = 2)
\]

\[
= \eta \rho(t) + (1 - \eta)\rho(t)/b
\]

\[
= \frac{\rho(t) + (b - 1)\eta \rho(t)}{b}.
\]

We now work out the conditional probability of vaccination among those who respond to the survey, which is what the sample proportion among survey respondents would estimate:

\[
Pr(V \mid R = 1) = \sum_{g \in \{1, 2\}} Pr(V \mid R = 1, G = g) Pr(G = g \mid R = 1)
\]

\[
= \sum_{g \in \{1, 2\}} \frac{Pr(V \mid G = g) Pr(R = 1 \mid G = g) Pr(G = g)}{Pr(R = 1)}
\]

\[
= \frac{\rho(t) + (b\gamma - 1)\eta \rho(t)}{b + (\gamma - 1)b\eta}.
\]

The error in the survey estimate is the difference between the vaccination rate among survey respondents and the vaccination rate in the entire population:

\[
Pr(V \mid R = 1) - Pr(V) = \frac{(b - 1)(\gamma - 1)(\eta - 1)\eta}{b + (\gamma - 1)b\eta} \rho(t).
\]

The bias \( Pr(V \mid R = 1) - Pr(V) \) is hence an increasing function of \( \rho(t) \) whose slope depends on \( b \) (and goes to zero when \( b = 1 \)).

If we obtain a particular sample of \( n \) respondents from the population and let \( Y_i \in \{0, 1\} \) indicate whether each person is vaccinated, the sample mean \( \hat{Y}_n \) is an estimator of \( Pr(V \mid R = 1) \). Figure 6 was produced by plugging eq. (4) into eq. (2), the \textit{ddc} estimator, in place of the error \( \hat{Y}_n - \hat{Y}_N \).
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