The Link Between Survey Response Rates and Nonresponse Bias: Theory, Simulations, and Empirical Evidence From the Household Pulse Survey

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1.Introduction

Surveys are ubiquitous in social science research, including economics. Data on public health, unemployment, and inflation all rely on large-scale surveys. However, there is increasing concern that the quality of our surveys is declining due to nonresponse bias (e.g., Meyer, Mok and Sullivan (2015)). Nonresponse bias occurs when the response rates are different depending on the outcome variable. An example is when unemployed individuals are more likely to respond to a survey on employment than employed individuals. Extensive literature has been dedicated to increasing response rates, controlling for nonresponse bias, and imputing missing data: Groves et al. (2002) is a 29 chapter book covering the various aspects of nonresponse, and the entirety of volume 645 of *The Annals of the American Academy of Political and Social Science* is dedicated to nonresponse (Kreuter, 2013).

Of all of the possible causes of nonresponse bias, the most difficult one to address is when the variable of interest has a direct casual effect on the propensity to respond. This is also called "not missing at random" (Groves, 2006). My paper focuses on this type of bias, and I provide a statistical test to detect the presence and sign of nonresponse bias when data not missing at random.

I write down a model for nonresponse bias in the context of estimating the prevalence rate of a certain binary trait of interest D (e.g., depression) in a finite population. I define nonresponse bias as the difference between the probability that someone responds to the survey when they have that trait of interest and when they do not (i.e., $P(R = 1|D = 1) - P(R = 1|D = 0)$). The model is consistent with the widely known fact that increasing response rates do not necessarily result in lower nonresponse bias. My model leverages the empirical relationship between the observed prevalence rate of D in the survey and response rates. I argue that this relationship has different interpretations when we compare *within* a survey (i.e., different waves of the same survey) versus when we compare *across* surveys (e.g., two different surveys on depression in America). The first comparison likely holds the propensity to respond in different groups constant, and the second holds the

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underlying value $P(D = 1)$ constant. By examining the relationship between response rates and our key variable of interest *within* a survey, we can uncover the sign of nonresponse bias. If individuals are more likely to respond to surveys when they are unemployed, and unemployment exogenously increases, then we would observe higher response rates as people move into the unemployed group and the average propensity to respond goes up. I show theoretically that a linear regression of response rates on the aggregated prevalence rate of the trait of interest will have the same sign as the nonresponse bias and confirm this through a simulation.

Previous work measuring nonresponse bias usually compares survey estimates with some true aggregated value (e.g., surveyed vaccination rates v. CDC vaccination data, surveyed outcome in election v. actual election outcome). For example, Meng (2018) explored the effect of the correlation between responding to a survey and the outcome variable of interest—what he calls the data defect correlation (ddc)—and how it affects the bias of an estimated mean. One measure of how the ddc affects an estimate is effective sample sizes: a sample size for a simple random survey that will produce the same MSE as the survey in question. Meng applies his model to surveys used in the 2016 Presidential election. He finds that the effective sample size of all election surveys, which covered more than 3.2 million individuals, is a mere 400.

As Meng notes, the data defect correlation cannot be calculated using only the survey dat and requires knowledge of the true value of our estimand. The main contribution of this paper is that my methods allow me to estimate the sign (although not the magnitude) of nonresponse bias directly from survey data.

I apply my model to the Census Household Pulse Survey (HPS), using measured anxiety, depression, and vaccination rates as my outcome variables. Bradley et al. (2021) showed that the HPS overestimates vaccination rates in the early days of the vaccination campaign by comparing vaccination rates in the survey with vaccination rates reported by the CDC. Dobson et al. (2022) showed that HPS suggests that there is large increase the depression and anxiety rates between 2019 and 2020. However, this change in bad mental health is absent in two other nationally representative surveys—NHIS and BRFSS—which have much higher response rates. Both papers suggest that there may be nonresponse bias in HPS as it relates to these outcome variables. Under some assumptions, I find that having anxiety increases one's propensity to respond to surveys and that being vaccinated reduces one's propensity to respond.

2.An Illustrative Model

2.1 Setup

The surveyor wants to learn about the proportion of a population with a certain trait. For each individual in the population, I define two Bernoulli (i.e., binary) random variables D and R. D represents the key trait of interest (in the case of Dobson et al. (2022), the trait is depression), and R represents whether or not an individual will respond to the survey, where $P(R = 1)$ may vary based on D. This gives us four types of individuals ($D = 1 \cap R = 1, D = 1 \cap R = 0$, etc.). The surveyor surveys n individuals, of which $n \cdot P(R = 1)$ are expected to respond. I assume that these individuals are selected randomly from the population, is representative, and that there are no measurement errors. I define

$\mathbf{d} = d_1, d_2...d_n$ to indicate if individual i has depression and $\mathbf{r} = r_1, r_2...r_n$ to indicate whether individual i responds to the survey. I assume that (d_i, r_i) is i.i.d.. This is an extremely strong assumption, since it is equivalent to assuming that everyone has the same $P(D)$, $P(R = 1|D = 1)$ and $P(R = 1|D = 0)$. In other words, the causal effect on the propensity to respond when one moves from $D = 0$ to $D = 1$ is uniform across the population. This is an unrealistic assumption, but it allows us to solve the model cleanly and illustrate its key ideas.

The surveyor's estimand is $\theta = P(D = 1)$, which I will also refer to as the D-incidence rate in the population. I define $b = P(R = 1|D = 1)$, $a = P(R = 1|D = 0)$, and the observed response rate $\hat{R} = \frac{\sum_{i=1}^{n} r_i}{n}$, with $\mathrm{E}(\hat{R}) = P(R = 1) = b\theta + a(1 - \theta)$. The goal of the model is to find the sign of the nonresponse bias using only the survey data, which I will define as $b - a$, the difference in response rates between the group with the trait of interest and the group without.

The surveyor constructs the following estimator for θ

$$\hat{\theta} = \frac{\sum_{i=1}^{n} d_i r_i}{\sum_{i=1}^{n} r_i} \tag{1}$$

Which is the D-incidence rate among the people who responded. I also call this the "observed" D-incidence rate.

Lemma 1. $\mathrm{E}[\hat{\theta}] = P(D = 1|R = 1) = \frac{b\theta}{b\theta + a(1-\theta)}$. In other words, $\hat{\theta}$ is an unbiased estimator for $P(D = 1|R = 1)$

Proof. See mathematical appendix. □

Lemma 2. $\hat{\theta}$ is an unbiased estimator for θ if and only if $b = a$, and that it is positively biased if $b > a$

Proof. See mathematical appendix. □

2.2 Results

To uncover nonresponse bias from the data, I examine the relationship between $\hat{R}$ and $\hat{\theta}$. There are two ways to decompose this change. The first is to consider two different surveys aiming to study the same phenomenon (e.g.,how many people have been vaccinated in a given state). In that case, the true underlying θ is the same across the two surveys, but b and a are different.

To have a more mathematical approach, I define an arbitrary parameter s. I think of s as the strength of the survey to elicit response, or one can think of s as the inverse of the cost of taking the survey. I then write a and b as functions of s with $\frac{\partial a}{\partial s} > 0 < \frac{\partial b}{\partial s}$ for all s.

Thus, when we compare across surveys, the intuition is that θ is constant while s changes. I take the partial derivative with respect to s to see what happens to the bias of our estimator as s and response rates increase.

Theorem 1. Without additional assumptions, it is unclear that increasing s reduces bias. $b > a$ and $b - a$ monotonically decreases as s increases (i.e. $\frac{\partial b}{\partial s} - \frac{\partial a}{\partial s} < 0$) form sufficient conditions for bias to decrease as s increases

Proof.

$$\begin{aligned}
\frac{\partial \mathrm{E}[\hat{\theta}-\theta]}{\partial s} &= \frac{\partial}{\partial s}\left(\frac{b(s)\theta}{b(s)\theta + a(s)(1-\theta)}\right) \\
&= \frac{\frac{\partial b}{\partial s}\theta[b\theta + a(1-\theta)] - b\theta[\frac{\partial b}{\partial s}\theta + \frac{\partial a}{\partial s}(1-\theta)]}{[b\theta + a(1-\theta)]^2} \\
&= \left(\frac{\partial b}{\partial s}\theta a(1-\theta) - \frac{\partial a}{\partial s}(1-\theta)b\theta\right) \times \frac{1}{[b\theta + a(1-\theta)]^2} \\
&= \left(\frac{\partial b}{\partial s}a - \frac{\partial a}{\partial s}b\right) \times \frac{\theta(1-\theta)}{[b\theta + a(1-\theta)]^2}
\end{aligned}$$

We can see that the second term is positive and that $\frac{\partial b}{\partial s}a - \frac{\partial a}{\partial s}b$ has unclear sign. Given additional assumptions, however, we have

$$\begin{aligned}
\frac{\partial b}{\partial s}a - \frac{\partial a}{\partial s}b &= \frac{\partial b}{\partial s}a - \frac{\partial b}{\partial s}b + \frac{\partial b}{\partial s}b - \frac{\partial a}{\partial s}b \\
&= \frac{\partial b}{\partial s}(a-b) + b\left(\frac{\partial b}{\partial s} - \frac{\partial a}{\partial s}\right)
\end{aligned}$$

Which is negative. In that case, we started with a positive bias ($b > a$), which decreased as response rates increased. The case when bias is negative is analogous. If $a > b$ and $\frac{\partial a}{\partial s} - \frac{\partial b}{\partial s} < 0$, then $\frac{\partial \mathrm{E}[\hat{\theta}-\theta]}{\partial s} > 0$. □

The intuition behind theorem 1 is that increasing survey response rates won't help if you're primarily increasing the response rate within a subgroup with a different D incidence rate than the population. For example, if $b > a$ and I increase response rates in the group with $D = 1$ way more than I do in the group with $D = 0$, then I would end up with more bias and not less. Our additional condition states that if the difference in subgroup response rates decreases as overall response rates increase, the bias would decrease. Wright (2015) gives an overview of empirical evidence on the (lack of) relationship between nonresponse rate and nonresponse bias.

Implicit in the parameterization of b and a as functions of s is that for any given level of s, there can be only one possible b and a value and thus one possible response rate. This implies that any two surveys with the same observed response rate must have the same b, a, and $\hat{\theta}$. This assumption is a simplification, and empirical evidence suggests that b and a vary based on survey protocols (Peytchev, Baxter and Carley-Baxter, 2009) without large changes in response rates. However, it does not detract from the central message of theorem 1 that higher response rates don't necessarily lead to less bias.

There is a perhaps less obvious relationship between observed prevalence and response rates. If we hold a and b constant, then as θ increases, $P(R = 1)$ will increase if $b > a$. In other words, as individuals move from $D = 0$ to $D = 1$, the unconditional probability that someone responds to the survey also increases.

Theorem 2. Holding b and a constant, the expected value of response rates will increase as the expected value of the observed D-incidence rate increases if and only if $b > a$.

Proof. We want to know the relationship between two observed variables, $\hat{R}$ and $\hat{\theta}$, as θ changes.

$$\frac{\partial \mathrm{E}[\hat{R}]}{\partial \theta} = \frac{\partial \mathrm{E}[\hat{R}]}{\partial \mathrm{E}[\hat{\theta}]} \frac{\partial \mathrm{E}[\hat{\theta}]}{\partial \theta}$$

$$\begin{aligned}
\frac{\partial \mathrm{E}[\hat{R}]}{\partial \mathrm{E}[\hat{\theta}]} &= \frac{\frac{\partial \mathrm{E}[\hat{R}]}{\partial \theta}}{\frac{\partial \mathrm{E}[\hat{\theta}]}{\partial \theta}} \\
&= \frac{b-a}{\frac{b(b\theta + a(1-\theta)) - b\theta(b-a)}{[b\theta + a(1-\theta)]^2}} \\
&= \frac{(b-a)[b\theta + a(1-\theta)]^2}{b^2\theta + ab - ab\theta - b^2\theta + ab\theta} \\
&= \frac{b-a}{ab} \times [b\theta + a(1-\theta)]^2
\end{aligned}$$

Which has the same sign as $b - a$. □

Our two theorems imply that, assuming $b > a$, $\frac{\partial b}{\partial s} - \frac{\partial a}{\partial s} < 0$, and that b and a are constant for any given survey, we will find that, comparing *across* surveys that measure the same θ, the observed prevalence rates decrease as response rates increases due to a decrease in bias. On the other hand, comparing *within* a survey that measures D over time as the true value θ varies, we will see prevalence rates increase as response rates increase because more individuals are in the "high response propensity" group, and thus overall response rates go up.

The most substantial assumption of the model is that the individual data d_i, r_i is distributed i.d.d.. Suppose that we have a panel data and assume that all variation in expected response rate comes from changes in D. Then, the only variation we capture in the data would be from individuals that "switch" between $D = 1$ and $D = 0$. If the i.i.d. assumption does not hold, then the average value of $b - a$ among these switchers may be different from the population as a whole. This is analogous to the case where instrumental variable regressions capture only the casual effect on compliers.

In appendix B, I incorporate sampling bias into the model in a similar fashion and show that, as long as the probability that someone gets sampled remains constant throughout different waves of the survey (i.e., the sample size and population size remain the same), $\frac{\partial \mathrm{E}[\hat{R}]}{\partial \mathrm{E}[\hat{\theta}]}$ will still have the same sign as $b - a$. However, if there is both sampling bias and nonresponse bias, the sign of nonresponse bias might not be the same as the overall bias of $\hat{\theta}$.

3.Confirming Theory Through Simulations

In order to get variation in θ, we would want a survey that has regional level data, data across time, or, ideally, both. Then we aggregate at the geographical unit-time unit (GT) level to get observed response rates and the observed D-incidence rate. An example would be taking a survey that collects monthly data in the US with information on the state of the respondent. We can then aggregate on the month-state level.

Using GTs as our units of observation, I apply an ordinary least squares regression of response rates on observed incidence. Theorem 2 implies that, assuming that there is no omitted variable bias, the slope of the linear regression will be the same sign as $b-a$. Before diving into empirical applications, I first illustrate the validity of this sign estimator through a simulation.

I start with four possible values for b (0.1, 0.15, 0.2, 0.3) and a values that are identical to b, $b \pm 0.05, b \pm 0.1$, and $b \pm 0.2$, with the restriction that $a > 0$. The true D incidence rate of each GT (θ) is drawn from a uniform distribution, with five possible (lower bound,upper bound) combinations: (0.05,0.1), (0.1,0.12), (0.1,0.15), (0.1,0.2), and (0.2,0.4). This gives a range of possible variances for the D incidence rate. Drawing randomly from a uniform distribution also implies that the empirical distribution of the 500 GTs could vary. That is, we are likely to get some skewed distributions by sheer chance. In the end, I have 120 possible combinations for values of a, b, and range of θ. For each combination, I simulate 500 GTs with a sample size of 2,000 individuals in each GT 1,000 times. Then, I aggregated the response rates and observed D incidence rate at the GT-level and run a linear regression of the response rate on D incidence rate. I record various summary statistics, including Bootstrap standard errors obtained through 1,000 resampling of the 500 GTs. The raw code and full set of simulated outputs are available in appendix C, and I present some stylized findings here.

When the difference between b and a is exactly zero, the percent of slopes statistically significant at the 5% level is approximately 5% using OLS standard errors. This fits the definition of p-values. The proportion of slopes with the same sign as $b-a$ and the absolute value of the slope are positively correlated with the magnitude of $b-a$ and the variance in θ. In specifications with a large $|b-a|$ and a large variance in θ, the slope almost always has the same sign as $b-a$. All slope estimates are correct and statistically significant when the variation in θ is 0.2-0.4.

The bootstrap standard errors and OLS standard errors are essentially identical (the correlation between them is 0.99998). I used the 2.5 and 97.5 percentiles of the resampled regression slopes and defined a slope as "bootstrap-significant" if both percentiles are of the same sign. Taking the average across different parameters where $b \neq a$, the proportion that is bootstrap significant (0.650) is almost identical to the proportion that is significant based on conventional OLS standard errors (0.647). The proportion of slopes with the correct sign, conditional on being significant, is also almost identical between OLS (0.97447) and bootstrap (0.97432).

Overall, our simulation validates our theoretical model and suggests that when there is sufficient variation in θ and difference between b and a, the slope of the regression line of $\hat{R}$ on $\hat{\theta}$ will have the same sign as $b-a$.

4.Empirical Application to the Household Pulse Survey

4.1 Background and Methodology

I now apply this test to real-world data drawn from the Household Pulse Survey (HPS), an online-based survey aimed at measuring "various sectors impacted by COVID-19: employment status, consumer spending, food security, housing, education disruptions and dimensions of physical and mental wellness" (Callen, 2020). I will examine nonresponse bias as it relates to estimates for the proportion of individuals with depression, anxiety, and at least one shot of a COVID-19 vaccine in the HPS, variables that are likely plagued by nonresponse bias (Dobson et al., 2022) (Bradley et al., 2021).

I use Phases 1-3.4 of the HPS, the New York Times COVID-19 deaths data, and data on the percent of adults who have received their first shot of COVID-19 vaccine in each state from the CDC. HPS data collection is broken into waves (referred to as "weeks"). They are one week long in Phase 1 and two weeks long in the other phases. Using the provided weights, I calculate the rates of anxiety and depression (as measured by the GAD-2 and PHQ-2, respectively) at each state during each week using the provided weights. I also calculate the average recorded employment rate (from the question "In the last 7 days, did you do ANY work for either pay or profit?") at the week-state level. Finally, I obtain COVID-19 death data from the New York Times and vaccination data from the CDC in a given state for the middle day of each Pulse week, and merge it with the collapsed/averaged HPS dataset. More information on HPS data and the NYT COVID-19 data used can be found in Dobson et al. (2022) footnoteAs a coauthor on the article, I was responsible for drafting the data description section..

Guided by theorem 2, I regress the weighted response rates of a state-week on the observed proportion of individuals with anxiety, depression, or who have received at least one COVID-19 vaccination shot in that state-week in HPS. I also use the true proportion of individuals vaccinated from CDC data. I use a two-way fixed effects (TWFE) design and control for the employment rate and COVID-19 deaths in the area in an attempt to control for confounding variables. I also weight states based on their population size. This regression is unlikely to fulfill many of the assumptions of our model and the assumptions needed to draw a causal conclusion from a TWFE regression as outlined in Imai and Kim (2021). It's very unlikely that individuals have the same propensity to get anxiety/depression/vaccinated or that anxiety/depression/vaccination affects everyone's propensity to respond to the HPS in the same way, which is what the i.i.d. assumption in my model implies.

Ideally, we would want to use an instrumental variable approach and find exogenous variation in depression/anxiety/vaccination rates, but I was not able to find an instrument that would fulfill the exclusion criterion and have variation across states and time periods. Nonetheless, given that there currently does not exist any statistical method to estimate nonresponse bias as it relates to PHQ-4 anxiety and depression measures in the HPS, I believe that this paper presents a good starting point to begin tackling this difficult issue. Investigating vaccination rates and nonresponse is also useful in two ways. The first is that we already know the overall bias of our estimate since we know θ. The bias could be due to sampling, nonresponse, or a mix of both. This regression will allow us to isolate the nonresponse aspect of the bias. Secondly, since $\frac{\partial \mathrm{E}[\hat{\theta}]}{\partial \theta} = b - a$, we could obtain not just the sign but the magnitude of the nonresponse bias.

4.2 Results

The results from the anxiety and depression regressions are not very convincing, especially given the movement in our coefficients as we add controls. My preferred specification in both tables is column five. I conclude that we have some suggestive evidence that nonresponse bias inflated estimates for the anxiety rates in the HPS.

Table 1: Results from regressing state-week depression rates (as measured by PHQ-2) from the HPS on the weighted response rates.

	(1)	(2)	(3)	(4)	(5)
			Weighted Response Rate		
Obs. depression	-0.0683***	-0.0281***	-0.0660***	0.0093	0.0045
	(0.0159)	(0.0085)	(0.0156)	(0.0060)	(0.0059)
COVID deaths per 100k					0.0017***
					(0.0005)
Obs. employment					-0.0100*
					(0.0060)
Constant	0.0763***	0.0452***	0.0633***	0.0226***	0.0275***
	(0.0037)	(0.0021)	(0.0049)	(0.0023)	(0.0038)
Observations	2,295	2,295	2,295	2,295	2,295
R-squared	0.0127	0.7837	0.1763	0.9499	0.9504
State FEs	N	N	Y	Y	Y
Week FEs	N	Y	N	Y	Y

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

Table 2: Results from regressing state-week anxiety rates (as measured by GAD-2) from the HPS on the weighted response rates.

	(1)	(2)	(3)	(4)	(5)
			Weighted Response Rate		
Obs. anxiety	-0.0193	0.0106	-0.0232*	0.0157***	0.0118**
	(0.0140)	(0.0086)	(0.0126)	(0.0056)	(0.0056)
COVID deaths per 100k					0.0016***
					(0.0005)
Obs. employment					-0.0090
					(0.0060)
Constant	0.0654***	0.0353***	0.0529***	0.0201***	0.0246***
	(0.0040)	(0.0027)	(0.0046)	(0.0025)	(0.0040)
Observations	2,295	2,295	2,295	2,295	2,295
R-squared	0.0014	0.7827	0.1690	0.9500	0.9505
State FEs	N	N	Y	Y	Y
Week FEs	N	Y	N	Y	Y

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

Table 3 below shows us the results from vaccination rates, demonstrating how that higher vaccination rates are associated with lower response rates. Since the coefficients are more stable, the signs of the slopes are always negative, and we have the true vaccination rates, I am more confident that we have identified a causal relationship between vaccination rates and response rates. My preferred specifications in the table are columns five and six.

Table 3: Results from regressing state-week vaccination rates as measured in the HPS (Obs. Vax rates) and reported from the CDC (Actl. Vax rates) on the weighted response rates. I use data from weeks 25-39 (inclusive). Data on vaccination status is not available before week 25. After week 39 (the end of Phase 3.2 of HPS), vaccination rates in many states are equal to the CDC cap of 95%, so I dropped them due to a lack of variance.

	(1)	(2)	(3)	(4)	(5)	(6)
			Weighted Response Rate			
Obs. vax rates	-0.0136***		-0.0078		-0.0097*	
	(0.0033)		(0.0053)		(0.0054)	
Actl. vax rates		-0.0130***		-0.0163***		-0.0163***
		(0.0030)		(0.0042)		(0.0047)
COVID deaths per 100k					0.0051***	0.0051***
					(0.0008)	(0.0008)
Obs. employment					-0.0068	-0.0065
					(0.0074)	(0.0072)
Constant	0.0755***	0.0734***	0.0584***	0.0577***	0.0581***	0.0569***
	(0.0024)	(0.0018)	(0.0014)	(0.0010)	(0.0045)	(0.0042)
Observations	765	765	765	765	765	765
R-squared	0.0386	0.0394	0.9353	0.9366	0.9413	0.9424
State FEs	N	N	Y	Y	Y	Y
Week FEs	N	N	Y	Y	Y	Y

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

My findings demonstrate how nonresponse cause the HPS to underestimate the true proportion of individual vaccinated: If we believe in all of the model's assumptions, then column six tells us that $P(R = 1|D = 1) - P(R = 1|D = 0) = -0.0163$. That is, individuals who are vaccinated are 1.63 percentage points less likely to respond to the survey than unvaccinated individuals. In reality, however, HPS *overestimating* vaccination rates. How do we reconcile these two findings? I note that they can co-exist when there is also sampling bias in the survey (see appendix B). The nonresponse bias pushes our estimate in the negative direction, but the sampling bias pushes our estimate in the positive direction such that we end up with a positive bias overall. We can see this in equation 4, where $b - a < 0$ does not imply a certain sign on the bias of $\tilde{\theta}$.

This also highlights the limitation of theorem 2, where we are only able to obtain the sign of the *overall* bias of our estimator when there is no measurement error or sampling bias, and the only bias comes from nonresponse.

Finally, I provide some intuition on the findings. Because the HPS is administered online, I hypothesize that when individuals spend more time online, they are more likely to respond to the survey. Overall, it's possible that being anxious increased the time spent online, and that being vaccinated decreased time spent online (perhaps people went out more).

5.Conclusion

In this paper, I write down and apply a new model of nonresponse bias that focuses on signing nonresponse bias on a binary variable of interest D. I find that, given some strict assumptions, the slope of a regression of response rates on observed prevalence of D in our survey is exactly the sign of nonresponse bias, defined as $P(R = 1|D = 1) - P(R = 1|D = 0)$. I confirmed these findings through a simulation and applied the model to the Household Pulse Survey. I find suggestive evidence that anxious individuals and non-vaccinated individuals are more likely to respond to the HPS conditional on being sampled and that the difference between vaccination rates in the HPS and reality is likely driven by sampling bias.

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Appendix A: Mathematical Appendix

Proof of lemma 1

$$\begin{aligned}
\mathrm{E}[\hat{\theta}] &= \mathrm{E}\left[\frac{\sum_{i=1}^{n} D_i R_i}{\sum_{i=1}^{n} R_i}\right] \\
&= \mathrm{E}\left[\mathrm{E}\left[\frac{\sum_{i=1}^{n} D_i R_i}{\sum_{i=1}^{n} R_i}\middle|\mathbf{R}=1\right]\right] \\
&= \mathrm{E}\left[\frac{1}{n}\mathrm{E}\left[\sum_{i=1}^{n} D_i R_i\middle|\mathbf{R}=1\right]\right] \\
&= \mathrm{E}\left[\frac{1}{n}\sum_{i=1}^{n}\mathrm{E}\left[D_i\middle|\mathbf{R}=1\right]\right] \\
&= \mathrm{E}\left[\frac{n}{n}\mathrm{E}\left[D_1\middle|R_1=1\right]\right] && \text{This follows when } d_i, r_i \text{ are distributed i.i.d.} \\
&= \mathrm{E}[D_1|R_1=1] = P(D=1|R=1)
\end{aligned}$$

Proof of lemma 2

$$\begin{aligned}
\mathrm{E}[\hat{\theta}] = P(D=1|R=1) &= \frac{P(R=1|D=1)P(D=1)}{P(R=1)} \\
&= \frac{b\theta}{b\theta + a(1-\theta)}
\end{aligned}$$

It follows immediately that $\mathrm{E}[\hat{\theta}] = \theta$ when $a = b$.

To see the second point, rewrite $b = a+\alpha$. Thus we can interpret α as the degree of bias and take the partial w.r.t. α and show that $\frac{\partial \mathrm{E}[\hat{\theta}]}{\partial \alpha}$ is always positive. That is, as α increases, $\mathrm{E}[\hat{\theta}]$ increases. Since we know that $\mathrm{E}[\hat{\theta}]$ has no bias when $\alpha = 0$, this implies that when $b - a = \alpha > 0$, bias is positive.

$$\begin{aligned}
\mathrm{sign}\frac{\partial}{\partial \alpha}\left(\frac{(a+\alpha)\theta}{(a+\alpha)\theta + a(1-\theta)}\right) &= \mathrm{sign}\left(\theta \cdot [(a+\alpha)\theta + a(1-\theta)] - (a+\alpha)\theta^2\right) \\
&= \mathrm{sign}\left(a\theta(1-\theta)\right)
\end{aligned}$$

which is always positive.

Appendix B: Extension to Sampling Bias

Previously, I assumed that there is no sampling bias. This is not always the case, and Bradley et al. (2021) suggests that oversampling of Democrats in the HPS could be driving the positive bias in vaccination rates.

I still work within a finite population framework. Suppose the surveyor sample n individuals without replacement from a population of N people. Let S be the Bernoulli random variable indicating if someone gets sampled. Bias from sampling occurs when $P(D = 1|S = 1) \neq P(D = 1|S = 0)$. Analogous to section 2.1, I define $\beta = P(S = 1|D = 1)$ and $\alpha = P(S = 1|D = 0)$. Once again, I assume that the data is independent and identically distributed

In a world with sampling bias, we have a new estimator

$$\tilde{\theta} = \frac{\sum_{i=1}^{N} d_i r_i s_i}{\sum_{i=1}^{N} r_i s_i}$$

where $s_1, s_2 ... s_N$ describes whether or not an individual has been sampled. Notice that we are now iterating from 1 to N instead of 1 to n. We also have $P(R = 1|S = 1) = b\theta + a(1-\theta)$ as before, and $\tilde{R} = \frac{\sum_{i=1}^{N} r_i s_i}{\sum_{i=1}^{N} s_i}$ with $E[\tilde{R}] = P(R = 1|S = 1)$

Lemma 3. $\mathrm{E}[\tilde{\theta}] = P(D = 1|S = 1 \cap R = 1)$

Proof. Analogous to proof of lemma 1. □

Lemma 4. Theorem 1 applies to sampling bias. Suppose we parameterize β and α as some function of s, which is the total number of individuals sampled. Then, increasing the sample size will reduce sampling bias if $\frac{\partial \beta}{\partial s} - \frac{\partial \alpha}{\partial s} < 0$ and $\beta - \alpha > 0$, but it may not if that condition does not hold.

Proof. Analogous to proof in theorem 1. Note that

$$\begin{aligned}\mathrm{E}[\tilde{\theta}] &= \frac{P(S = 1 \cap R = 1|D = 1)P(D = 1)}{P(S = 1 \cap R = 1)} \\ &= \frac{b}{b\theta + a(1-\theta)} \cdot \frac{\beta\theta}{\beta\theta + \alpha(1-\theta)}\end{aligned}$$

Thus the first term becomes a constant when we take the derivative, and the remaining logic is identical to theorem 1 □

To simplify notation, I will use $P(X)$ to indicate the probability that a given Bernoulli random variable is equal to one, $P(Y|X)$ to indicate $P(Y = 1|X = 1)$, and $P(X, Y)$ to indicate $P(X \cap Y)$.

I now write down the bias of our new estimator:

$$\mathrm{E}[\tilde{\theta} - \theta] = P(D|S, R) - P(D) = \frac{P(S, R|D)P(D)}{P(S, R)} - \frac{P(S, R)P(D)}{P(S, R)} \tag{2}$$

$$= \frac{P(D)}{P(R|S)P(S)} \left[P(S, R|D) - P(S, R)\right] \tag{3}$$

$$= \frac{\theta}{[b\theta + a(1-\theta)][\beta\theta + \alpha(1-\theta)]} \times \{b\beta\theta - [b\theta + a(1-\theta)][\beta\theta + \alpha(1-\theta)]\} \tag{4}$$

I now assume that $P(S)$ remains constant. In other words, the number of individuals sampled throughout the survey does not change. In the case of the HPS, this assumption holds starting from Phase 2. We now investigate the relationship between response rates and $\tilde{\theta}$.

Theorem 3. Even if there is sampling bias in the survey, as long as the assumptions in theorem 2 hold and $P(S = 1)$ remains constant, the same linear regression of response rates on $\tilde{\theta}$ should have the same sign as $b - a$.

Proof.

$$\begin{aligned}
\frac{\partial \mathrm{E}[\tilde{\theta}]}{\partial \theta} &= \frac{P(S,R|D)P(S,R) - P(S,R|D)P(D)\frac{\partial P(S,R)}{\partial P(D)}}{P(S,R)^2} \\
&= \frac{P(S,R|D)}{P(S,R)^2}\left[P(R|S)P(S) - P(D)\frac{\partial P(R|S)P(S)}{\partial \theta}\right] \\
&= \frac{P(S)P(S,R|D)}{P(S,R)^2}\left[P(R|S) - P(D)\frac{\partial P(R|S)}{\partial \theta}\right] \\
&= \frac{P(S)P(S,R|D)}{P(S,R)^2}\left[b\theta + a(1-\theta) - \theta(b-a)\right] \\
&= \frac{P(S)P(S,R|D)}{P(S,R)^2} \cdot a
\end{aligned}$$

which is always positive. Thus

$$\frac{\partial \mathrm{E}[\tilde{R}]}{\partial \mathrm{E}[\tilde{\theta}]} = \frac{\frac{\partial P(R=1|S=1)}{\partial \theta}}{\frac{\partial \mathrm{E}[\tilde{\theta}]}{\partial \theta}} = \frac{b-a}{\frac{\partial \mathrm{E}[\tilde{\theta}]}{\partial \theta}}$$

which retains the sign of $b - a$. □

The reader can check that the expression $\frac{\partial \mathrm{E}[\tilde{\theta}]}{\partial \theta}$ is equivalent to the one is theorem 2 if S and R are independent, conditionally independent given D, and that S and D are independent.

However, it is important to note that the nonresponse bias does not necessarily have the same sign as the *overall* bias of our estimator. Looking at equation 4 for the bias of $\tilde{\theta}$, we see that $b - a > 0$ does not imply that $\mathrm{E}[\tilde{\theta} - \theta] > 0$.

Appendix C: Simulation Code and Results

Code:

```
#Tim Hua 2022
#Note: Even if you parallelize, this simulation can take a long time to run.
if (!require(tidyverse)) install.packages("tidyverse"); library(tidyverse)
if (!require(tictoc)) install.packages("tictoc"); library(tictoc)
if (!require(magrittr)) install.packages("magrittr"); library(magrittr)
if (!require(doParallel)) install.packages("doParallel"); library(doParallel)
#setwd("T:\\Middlebury\\OneDrive - Middlebury College\\Research\\Nonresponse")
set.seed(20220928)
n <- 2000 #Number sampled
gtUnits <- 500 #Number of geographical-time units.
b <- c(0.1,0.15,0.2,0.3)
```

```
abgap <- c(-0.2,-0.1,-0.05,0,0.05,0.1,0.2)
thetalower <- c(0.05,0.1,0.1,0.1,0.2)
thetaupper <- c(0.1,0.12,0.15,0.2,0.4)

#Generate parameters for parallel calculations later
parlist = list()
for (i in b){
  #for each possible b
  for (j in abgap){
    #for each possible difference
    a <- i + j
    for(k in 1:length(thetalower)){
      if(a > 0.01){
        parlist[[length(parlist) + 1]] <- c(i,a,thetalower[k],
                                            thetaupper[k],
                                            round(runif(1,0,1)*100000))
        #The last term is for reproducibility and generates
        #a fixed seed in each %dopar% enviornment
      }
    }
  }
}
cores <- detectCores()
cl <- makeCluster(cores)
registerDoParallel(cl)


#TIME FOR PARALLELIZING
#I love dopar so much
oneparam = parlist[[90]]
tic()
output_w_boot <- foreach(oneparam = parlist,
                         .combine = 'rbind') %dopar% {
  #Faster version of as.data.frame
  quickdf <- function(l) {
    class(l) <- "data.frame"
    attr(l, "row.names") <- .set_row_names(length(l[[1]]))
    l
  }
  set.seed(oneparam[5])
  NSIMULS <- 1000
  slope_vec <- rep(NA,NSIMULS)
  sig_vec <- rep(NA,NSIMULS)
  slopecorrect_vec<-rep(NA,NSIMULS)
  pval_vec <- rep(NA,NSIMULS)
```

```
sd_vec <- rep(NA,NSIMULS)
boot_sd_vec <- rep(NA,NSIMULS)
boot_ptil025 <- rep(NA,NSIMULS)
boot_ptil975 <- rep(NA,NSIMULS)
for(i in 1:NSIMULS){
  #A new simulation
  thetahat_vec <- rep(NA,gtUnits)
  Rhat_vec <- rep(NA,gtUnits)
  for (l in 1:gtUnits){
    #in each geographical-time area.
    theta <- runif(1,oneparam[3],oneparam[4])
    ndepInSample <- rbinom(1,n,theta)
    ndepRespond <- rbinom(1,ndepInSample,oneparam[1])
    nNotdepRespond <- rbinom(1,n-ndepInSample,oneparam[2])
    thetahat_vec[l] <- ndepRespond/(ndepRespond + nNotdepRespond)
    Rhat_vec[l] <- (ndepRespond + nNotdepRespond)/n
  }
  results <- summary(lm(Rhat_vec ~ thetahat_vec))
  slope_vec[i] <- results$coefficients[[2,1]]
  sig_vec[i] <- results$coefficients[[2,4]] < 0.05
  slopecorrect_vec[i] <- sign(results$coefficients[[2,1]]
                             ) == sign(oneparam[1]-oneparam[2])
  pval_vec[i] <- results$coefficients[[2,4]]
  sd_vec[i] <- results$coefficients[[2,2]]
  #1000 times bootstrap resampling.
  bootslopes <- rep(NA,1000)
  gendf <- quickdf(list(Rhat = Rhat_vec, thetahat = thetahat_vec))
  #quickdf saves like 58 microseconds
  for (j in 1:1000){
    bootdf <- gendf[sample.int(nrow(gendf), size = gtUnits, replace = T),]
    #Using sample.int instead of slice_sample saves like 300ish microseconds
    results <- summary(lm(Rhat ~ thetahat, data = bootdf))
    bootslopes[j] <- results$coefficients[[2,1]]
  }
  boot_sd_vec[i] <- sd(bootslopes)
  boot_ptil025[i] <- quantile(bootslopes,0.025)
  boot_ptil975[i] <- quantile(bootslopes,0.975)

}

#Outputting summary stats:
tibble::tibble(b = oneparam[1], a = oneparam[2],
       theta_range = paste0(as.character(oneparam[3]),"-",
                            as.character(oneparam[4])),
       prop_correct = mean(slopecorrect_vec),
```

```
          prop_sig = mean(sig_vec),
          prop_sig_correct = mean(slopecorrect_vec[sig_vec]),
          avg_slope = mean(slope_vec),
          n_sims = length(slope_vec),
          avg_pval = mean(pval_vec),
          avg_sigpval = mean(pval_vec[sig_vec]),
          avg_sd = mean(sd_vec),
          avg_boot_sd = mean(boot_sd_vec),
          p_boot_lbp = mean(boot_ptil025 > 0),
          #percent of lower bootstrap bound positive
          p_boot_ubn = mean(boot_ptil975 < 0),
          #percent of upper bootstrap bound negative
          p_boot_sig_cor = ifelse(oneparam[1] == oneparam[2],0,
                                  ifelse(oneparam[1] > oneparam[2],
                                         p_boot_lbp/(p_boot_lbp + p_boot_ubn),
                                  p_boot_ubn/(p_boot_lbp + p_boot_ubn)))
          #percent bounds that are correct out of the significant ones
          )
    #I can afford to use a tibble here (runtime wise)
    #since it only gets run 120 times
  }
  toc()
  output_w_boot %<>% mutate(diff = b - a)
  View(output_w_boot)
  write_csv(output_w_boot,"sim_output_boot1.2.csv")

  #bench::mark(
  #  as.data.frame = as.data.frame(list(Rhat = Rhat_vec,
  #                                     thetahat = thetahat_vec)),
  #  quick_df      = quickdf(list(Rhat = Rhat_vec, thetahat = thetahat_vec))
  #)[c("expression", "min", "median", "itr/sec", "n_gc")]
  #bench::mark(
  #  bootdf = gendf[sample.int(nrow(gendf), size = gtUnits, replace = T),]
  #)[c("expression", "min", "median", "itr/sec", "n_gc")]

  #bench::mark(
  #  bootdf2 = slice_sample(gendf,n = gtUnits, replace = T)
  #)[c("expression", "min", "median", "itr/sec", "n_gc")]
```

The actual results have 16 columns. I pick a eight notable ones to report here. All code and data are available upon request.

b	a	theta_range	prop_correct	prop_sig	prop_sig_correct	avg_sd	avg_boot_sd
0.1	0.05	0.05-0.1	0.977	0.475	1	0.005453	0.005463

0.1	0.05	0.1-0.12	0.608	0.066	0.833333	0.005882	0.005893
0.1	0.05	0.1-0.15	0.931	0.331	1	0.005122	0.005122
0.1	0.05	0.1-0.2	1	0.994	1	0.003971	0.003957
0.1	0.05	0.2-0.4	1	1	1	0.003163	0.00313
0.1	0.1	0.05-0.1	0	0.03	0	0.012733	0.012727
0.1	0.1	0.1-0.12	0	0.053	0	0.013103	0.013118
0.1	0.1	0.1-0.15	0	0.057	0	0.01096	0.01097
0.1	0.1	0.1-0.2	0	0.054	0	0.007847	0.00783
0.1	0.1	0.2-0.4	0	0.052	0	0.004549	0.004538
0.1	0.15	0.05-0.1	0.908	0.24	1	0.021621	0.021605
0.1	0.15	0.1-0.12	0.606	0.064	0.734375	0.021817	0.021783
0.1	0.15	0.1-0.15	0.853	0.198	1	0.017912	0.017947
0.1	0.15	0.1-0.2	0.998	0.879	1	0.012584	0.012525
0.1	0.15	0.2-0.4	1	1	1	0.006452	0.006413
0.1	0.2	0.05-0.1	0.982	0.575	1	0.031678	0.031525
0.1	0.2	0.1-0.12	0.629	0.07	0.885714	0.031455	0.031439
0.1	0.2	0.1-0.15	0.97	0.489	1	0.025833	0.025741
0.1	0.2	0.1-0.2	1	0.999	1	0.018034	0.017883
0.1	0.2	0.2-0.4	1	1	1	0.008739	0.008652
0.1	0.3	0.05-0.1	1	0.966	1	0.053979	0.053389
0.1	0.3	0.1-0.12	0.762	0.119	0.966387	0.052635	0.052443
0.1	0.3	0.1-0.15	0.999	0.937	1	0.043606	0.043224
0.1	0.3	0.1-0.2	1	1	1	0.030608	0.03002
0.1	0.3	0.2-0.4	1	1	1	0.014385	0.014091
0.15	0.05	0.05-0.1	1	0.978	1	0.004837	0.004817
0.15	0.05	0.1-0.12	0.745	0.098	0.979592	0.005737	0.005744
0.15	0.05	0.1-0.15	1	0.91	1	0.005009	0.004981
0.15	0.05	0.1-0.2	1	1	1	0.003927	0.003869
0.15	0.05	0.2-0.4	1	1	1	0.003817	0.003716
0.15	0.1	0.05-0.1	0.947	0.38	1	0.010387	0.010376
0.15	0.1	0.1-0.12	0.619	0.055	0.745455	0.011772	0.011781
0.15	0.1	0.1-0.15	0.923	0.313	1	0.00956	0.009538
0.15	0.1	0.1-0.2	1	0.962	1	0.006708	0.006683
0.15	0.1	0.2-0.4	1	1	1	0.004447	0.004432
0.15	0.15	0.05-0.1	0	0.074	0	0.017062	0.017016
0.15	0.15	0.1-0.12	0	0.063	0	0.018862	0.018916
0.15	0.15	0.1-0.15	0	0.051	0	0.014956	0.014955
0.15	0.15	0.1-0.2	0	0.061	0	0.01009	0.010085
0.15	0.15	0.2-0.4	0	0.058	0	0.005637	0.005622
0.15	0.2	0.05-0.1	0.876	0.23	0.995652	0.024452	0.024396
0.15	0.2	0.1-0.12	0.576	0.052	0.826923	0.026652	0.026674
0.15	0.2	0.1-0.15	0.865	0.188	0.994681	0.020944	0.02093
0.15	0.2	0.1-0.2	0.999	0.847	1	0.0139	0.013851
0.15	0.2	0.2-0.4	1	1	1	0.007107	0.007079

0.15	0.25	0.05-0.1	0.992	0.609	1	0.032533	0.032352
0.15	0.25	0.1-0.12	0.654	0.07	0.942857	0.035091	0.035068
0.15	0.25	0.1-0.15	0.981	0.572	1	0.027363	0.027297
0.15	0.25	0.1-0.2	1	1	1	0.01803	0.017909
0.15	0.25	0.2-0.4	1	1	1	0.008747	0.008667
0.15	0.35	0.05-0.1	1	0.983	1	0.049712	0.049358
0.15	0.35	0.1-0.12	0.775	0.11	0.954545	0.052579	0.052581
0.15	0.35	0.1-0.15	1	0.957	1	0.041309	0.040977
0.15	0.35	0.1-0.2	1	1	1	0.027276	0.026907
0.15	0.35	0.2-0.4	1	1	1	0.012665	0.012508
0.2	0.1	0.05-0.1	1	0.919	1	0.009121	0.009097
0.2	0.1	0.1-0.12	0.729	0.094	0.978723	0.011112	0.0111
0.2	0.1	0.1-0.15	0.998	0.823	1	0.008928	0.00891
0.2	0.1	0.1-0.2	1	1	1	0.006252	0.006226
0.2	0.1	0.2-0.4	1	1	1	0.004691	0.004632
0.2	0.15	0.05-0.1	0.913	0.303	1	0.014503	0.014459
0.2	0.15	0.1-0.12	0.626	0.058	0.672414	0.017267	0.017274
0.2	0.15	0.1-0.15	0.894	0.258	1	0.013338	0.013309
0.2	0.15	0.1-0.2	1	0.921	1	0.00887	0.008828
0.2	0.15	0.2-0.4	1	1	1	0.005446	0.005441
0.2	0.2	0.05-0.1	0	0.062	0	0.020459	0.020442
0.2	0.2	0.1-0.12	0	0.042	0	0.024029	0.023979
0.2	0.2	0.1-0.15	0	0.057	0	0.018315	0.018285
0.2	0.2	0.1-0.2	0	0.054	0	0.011796	0.011799
0.2	0.2	0.2-0.4	0	0.05	0	0.006441	0.006426
0.2	0.25	0.05-0.1	0.893	0.245	1	0.026913	0.026811
0.2	0.25	0.1-0.12	0.568	0.063	0.793651	0.031121	0.031163
0.2	0.25	0.1-0.15	0.865	0.203	1	0.023518	0.023498
0.2	0.25	0.1-0.2	0.998	0.806	1	0.014991	0.014972
0.2	0.25	0.2-0.4	1	1	1	0.007636	0.007604
0.2	0.3	0.05-0.1	0.99	0.643	1	0.033626	0.033525
0.2	0.3	0.1-0.12	0.674	0.066	0.954545	0.03865	0.038565
0.2	0.3	0.1-0.15	0.982	0.568	1	0.028963	0.028841
0.2	0.3	0.1-0.2	1	1	1	0.018355	0.0183
0.2	0.3	0.2-0.4	1	1	1	0.008932	0.008894
0.2	0.4	0.05-0.1	1	0.989	1	0.047348	0.047107
0.2	0.4	0.1-0.12	0.793	0.133	0.984962	0.053654	0.053592
0.2	0.4	0.1-0.15	1	0.97	1	0.040252	0.039956
0.2	0.4	0.1-0.2	1	1	1	0.025553	0.025354
0.2	0.4	0.2-0.4	1	1	1	0.011861	0.011757
0.3	0.1	0.05-0.1	1	1	1	0.007876	0.007828
0.3	0.1	0.1-0.12	0.885	0.277	1	0.010765	0.010745
0.3	0.1	0.1-0.15	1	1	1	0.008599	0.008503
0.3	0.1	0.1-0.2	1	1	1	0.006076	0.00598

0.3	0.1	0.2-0.4	1	1	1		0.005646	0.005534
0.3	0.2	0.05-0.1	0.997	0.812	1		0.016152	0.016109
0.3	0.2	0.1-0.12	0.72	0.092	0.945652		0.021202	0.021142
0.3	0.2	0.1-0.15	0.993	0.7	1		0.015458	0.015435
0.3	0.2	0.1-0.2	1	1	1		0.009836	0.009821
0.3	0.2	0.2-0.4	1	1	1		0.00621	0.006184
0.3	0.25	0.05-0.1	0.911	0.238	1		0.020796	0.020734
0.3	0.25	0.1-0.12	0.565	0.06	0.666667		0.026907	0.026838
0.3	0.25	0.1-0.15	0.873	0.234	0.995726		0.019281	0.019229
0.3	0.25	0.1-0.2	0.999	0.837	1		0.011992	0.011956
0.3	0.25	0.2-0.4	1	1	1		0.006821	0.006812
0.3	0.3	0.05-0.1	0	0.051	0		0.025485	0.025492
0.3	0.3	0.1-0.12	0	0.041	0		0.032761	0.03273
0.3	0.3	0.1-0.15	0	0.056	0		0.023187	0.02318
0.3	0.3	0.1-0.2	0	0.039	0		0.014181	0.01417
0.3	0.3	0.2-0.4	0	0.052	0		0.00757	0.007561
0.3	0.35	0.05-0.1	0.887	0.215	0.990698		0.030343	0.030213
0.3	0.35	0.1-0.12	0.638	0.065	0.707692		0.038569	0.038478
0.3	0.35	0.1-0.15	0.868	0.197	0.994924		0.027235	0.027142
0.3	0.35	0.1-0.2	0.998	0.764	1		0.01649	0.016449
0.3	0.35	0.2-0.4	1	1	1		0.008368	0.00836
0.3	0.4	0.05-0.1	0.992	0.658	1		0.035135	0.035051
0.3	0.4	0.1-0.12	0.71	0.071	0.915493		0.04455	0.044379
0.3	0.4	0.1-0.15	0.981	0.599	1		0.03127	0.03118
0.3	0.4	0.1-0.2	1	1	1		0.018779	0.018705
0.3	0.4	0.2-0.4	1	1	1		0.009228	0.009185
0.3	0.5	0.05-0.1	1	0.999	1		0.044396	0.044173
0.3	0.5	0.1-0.12	0.837	0.147	0.986395		0.055573	0.05551
0.3	0.5	0.1-0.15	1	0.982	1		0.03914	0.038837
0.3	0.5	0.1-0.2	1	1	1		0.023424	0.023383
0.3	0.5	0.2-0.4	1	1	1		0.011063	0.01101