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How Undervalued is the Covid-19 Vaccine? Evidence from Discrete Choice Experiments and VSL Benchmarks

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ABSTRACT

Two discrete choice experiments conducted early in the Covid-19 vaccination campaign show that people dramatically undervalue the Covid-19 vaccine, relative to benchmarks implied by the value of a statistical life (VSL). Our first experiment found that median willingness to pay (WTP) for initial vaccination is around $50, only 2 percent of the WTP implied by standard VSL calculations. Our second experiment found the median person was willing to accept (WTA) about $200 to delay the second dose, only 32 percent of the WTA implied by standard VSL calculations. While standard economic models imply that vaccines are undervalued because of their large externalities, we interpret the finding that WTP estimates are well below the VSL benchmarks as evidence that internalities play a substantial role. This evidence that people undervalue even the private benefits of vaccination suggests that there may be a role for government beyond conventional efforts to correct externalities.

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1 Introduction

Evidence from the clinical trials and from post-market studies shows that Covid-19 vaccines are highly effective at reducing the risk of severe illness, hospitalization, and death. Recent work suggests that the initial vaccine roll out managed to avert about $140,000$ Covid-19 deaths by May 9, 2021 in the US (Gupta et al., 2021). Despite these substantial benefits, only about 63% of people have received two doses of the Covid-19 vaccine and only 22% have received a booster dose. In other words, take-up seems quite low for what appears to be a very high value form of medical care. Economic theory suggests we should not be surprised that vaccine take-up is below the socially optimal level because vaccination generates positive externalities, creating an incentive for unvaccinated people to free-ride.

Although the externalities argument is a logical place to start the analysis, it does not seem to provide a sufficient explanation for low vaccine take-up in the US. The private benefits of vaccination are large, and government policies make the vaccine available for free, although people still incur time and effort costs. A zero out-of-pocket price – sound economic advice in the face of positive externalities – acts as a *Pigouvian-style* subsidy that should eliminate or at least substantially dull the incentive to free-ride. And yet, vaccine take-up is still relatively low.

Alternative theories from behavioral public finance may help explain why vaccine take-up is so low in the US. Previous research suggests that consumer demand may be affected by psychological factors that lead people to make systematic mistakes in the evaluation of costs and benefits (Mullainathan et al., 2012; Allcott and Knittel, 2019; Chetty et al., 2009; Allcott et al., 2014). In particular, low vaccine take-up may be rooted in an internality: people do not fully value even the private benefits of the vaccination. In this sense, the Covid-19 vaccine is an example of the broader concern that behavioral hazards lead people to under-consume "high value" medical care (Baicker et al., 2015). The possibility that people make systematic decision errors in vaccine choices undermines the idea that choices "reveal" preferences and that the only remaining economic problem is externality.

In this paper, we present the first evidence measuring the existence and magnitude of Covid-19 vaccine internalities. Our empirical strategy uses two discrete choice survey experiments distributed early in 2021 using banner ads on MSN.com. One experiment presented people with a vignette describing an opportunity to be vaccinated early for a price. The other described an opportunity to delay the second dose of the vaccine for a given reward. We use the experiments to estimate willingness to pay (WTP) for and willingness to accept (WTA) the vaccine. We also recover the value of statistical life (VSL) implied by people's

decisions in the experiments. A central idea is that these estimated parameters *may include or be shaped by* decision errors that generate internalities. To assess this possibility, we construct benchmarks of WTP and WTA using external estimates of VSL from other contexts. Since these external VSL estimates are unlikely to be subject to vaccine related decision errors, we view the benchmarks as a reasonable approximation to the "true" or "full" WTP and WTA in the absence of internalities. The ratio of the experimental parameters to the benchmarks provides a measure of undervaluation due to an internality.

The results of our analysis suggest that people substantially undervalue the private benefits associated with Covid-19 vaccination. The early vaccination experiments indicate that the median person was willing to pay about $50 to be vaccinated for Covid-19 four months early. In contrast, our VSL-benchmark implies that a typical person would have been willing to pay about $\$2,700$ to be vaccinated four months early if she valued the fatality risk reduction of the vaccine using a VSL of $11.4 million, which is the figure used by the federal Department of Health and Human Services (HHS). The VSL-benchmark WTP is more than 50 times higher than the median WTP recovered from the experiment. In the experiment, people act as if their own VSL is around $\$210,000$, which is far lower than the VSL people seem to use in other settings like labor markets and product markets. The delayed second dose experiments also point to undervaluation. They suggest that the median person was willing to delay the second dose of the vaccine in return for a transfer of about $200. Benchmarks based on the HHS VSL imply that WTA compensation for a delayed second dose should be closer to $619. Implicitly, people in the delayed second dose experiments seem to use a VSL of $3.7 million when making their choices, much smaller than standard estimates of the VSL in non-Covid settings.

Taken together, the results show that people value the first dose of the vaccine at about 2% of its full value and the second dose at about 32% of its full value. While standard models imply that vaccines are undervalued because of their large externalities, our results imply substantial undervaluation due to internalities.

2 Background

2.1 Behavioral Hazards in Health Care

There is a growing literature concerned with how to perform policy analysis in market settings where *behavioral biases* influence the choices people make (Mullainathan et al., 2012; Allcott, 2013). An overarching issue is that the possibility of behavioral biases implies that observed

choices cannot be trusted to reveal preferences. This creates a major methodological problem for efforts to understand the welfare consequences of different public policies or market environments. For example, health economists have devoted considerable effort to measuring the extent and size of moral hazard responses to health insurance by focusing on the price elasticity of demand for medical care (Pauly, 1968; Cutler and Zeckhauser, 2000). There is convincing evidence from experiments and quasi-experiments that the demand for medical care falls with exogenous changes in the out-of-pocket price of care (Einav and Finkelstein, 2018). However, recent work by Baicker et al. (2015) points out that – in the presence of behavioral biases – evidence that people reduce their medical care utilization in response to price changes *does not* necessarily indicate moral hazard. This kind of revealed preference argument falls away when people's decisions may reflect systematic mistakes that may be related to false beliefs, inattention, salience, or present bias. Baicker et al. (2015) argue that people often do seem to under-consume *high value* medical care and over-consume *low value* medical care. A strict proponent of the revealed preference theory would argue that consumers are the best evaluator of the value of medical care. If people choose not to consume health services that experts consider valuable, then perhaps the experts are simply wrong. What kind of evidence supports the claim that it is the consumers who are making mistakes about the value of care?

One kind of evidence that Baicker et al. (2015) discuss comes from studies that compare the price elasticity of demand for high value and low value medical care. They reason that – in the absence of decision errors on the part of consumers – one would expect demand for high value medical care to be more *price inelastic* than demand for low value medical care. In contrast to this intuition, they point to several examples from high quality empirical studies showing that exogenous increases in co-insurance and co-payment parameters generate similar declines in the utilization of health care that is thought to be high value and low value. For example, in their analysis of data from the Rand Health Insurance Experiment, Lohr et al. (1986) group health conditions into categories defined by expert opinions of the effectiveness of available medical treatments. Random assignment to the cost-sharing plan leads to similar and large reductions in utilization for conditions where medical care was considered highly effective and for conditions where medical care was considered less effective. In another example, Chandra et al. (2010) group prescription medications into three categories: acute care medications, chronic care medications, and lifestyle medications.[1] They find that

[1]Cutting back on acute care medications would be expected to generate adverse health events within 1-2 months. Cutting back on chronic care medication might generate adverse health events within a year. Lifestyle medications (antihistimines, acne medications, etc) probably do not protect against adverse events and instead provide relief from symptoms.

cost sharing leads to similar reductions in the utilization of all three classes of drugs.

Baicker et al. (2015) also discuss a second type of evidence that people under-consume high value care, which involves measuring the *health effects* of exogenous variation in co-insurance or co-pay parameters. For instance, the study by Chandra et al. (2010) find some evidence that increasing co-pays also generate more hospitalizations downstream. This suggests that people cut back on care that had substantial health benefits. Similarly, Choudhry et al. (2011) find that eliminating cost sharing for post-heart attack medications increases adherence and reduces some measures of subsequent vascular events. What makes these results surprising is that the reductions in health seem much more valuable than the savings from reduced health care spending. And indeed, when Baicker et al. (2015) combine these health responses with assumptions about the value of a statistical life, they do find that the change in utilization was too large to be explained by conventional theories of moral hazard.

While these two lines of evidence are not definitive proof that behavioral biases play a major role in health care consumption decisions, they do seem to indicate a disconnect between realized choices and what might be called *the underlying fundamentals* that economists and physicians think make up the value of specific types of medical care. At a high level, this comparison of observed choices and fundamentals represents a method for measuring the existence and magnitude of behavioral biases. In this paper, we are concerned with the possibility that behavioral biases – internalities – may be partly responsible for low take-up of the Covid-19 vaccine. The central hypothesis is that one or more behavioral biases leads people to undervalue the net (private) benefits of vaccination. To test the theory, we conducted discrete choice survey experiments that provide a platform for estimating WTP, WTA, and VSL in a setting that should incorporate Covid-19 vaccine perceptual errors. Then we use external estimates of VSL to construct benchmark measures of WTP and WTA for the vaccine in the absence of perceptual errors. The ratio of these two sets of measures provides a measure of the size of the behavioral distortion. This approach of combining survey experiments with VSL benchmarks to measure internalities provides a novel way to understand the reasons for low vaccine take-up and related concepts such as vaccine hesitancy. The overall method is closely connected to conventional (non-behavioral) research on WTP for the Covid-19 vaccine and with the broader literature on VSL and money-risk trade-offs. The remainder of this section gives some background on these areas of work.

Table 1: WTP Estimates for Covid-19 Vaccination

Country	Sample Size	Authors	Median WTP Estimate (US$2020)*
Bangladesh	894	Banik et al. (2021)	$5
Bangladesh	697	Kabir et al. (2021)	$7
Brazil	1402	Godói et al. (2021)	$21
Chile	531	Cerda and García (2021)	$232
Chile	566	García and Cerda (2020)	$185
China	2058	Wang et al. (2021)	$15
China	2126	Han et al. (2021)	$46
China	3541	Lin et al. (2020)	$28
China	1179	Zhang et al. (2021)	$112
Ecuador	1050	Sarasty et al. (2020)	$90
Ethiopia	301	Shitu et al. (2021)	$5
India	2451	Goruntla et al. (2021)	$7
Indonesia	1359	Harapan et al. (2020)	$31
Kenya	1050	Carpio et al. (2021)	$59
Malaysia	1159	Wong et al. (2020)	$31
Pakistan	2158	Arshad et al. (2021)	$3
Vietnam	651	Nguyen et al. (2021)	$14
Vietnam	495	Vo et al. (2021)	$86
United States	584	Catma and Reindl (2021)	$260
United States	1285	Catma and Varol (2021)	$319
United States	2000	Carpio et al. (2021)	$723
US, UK, Spain, Italy	4313	Costa-Font et al. (2021)	$150
Overall Median Estimate			$39

Notes: *Median WTP estimates were taken from each of the respective studies. If no median estimate was available, then the mean WTP estimate was used. If only a range was reported, then the mid-point value was used.

2.2 Willingness to Pay For The Covid-19 Vaccine

We reviewed 22 studies from 16 countries that estimate consumer WTP for the Covid-19 vaccine using a range of stated preference and contingent valuation methodologies. These studies all interpret the results through the lens of the standard (non-behavioral) model and do not consider the possibility that estimates are too low due to internalities. Table 1 shows the citation, sample size, and median WTP estimates for vaccination from each of these study.[2] The median WTP estimates range in value from $3 (Arshad et al., 2021) to $723 (Carpio et al., 2021). Figure 1 shows how estimated WTP varies with GDP per capita across studies conducted in different countries. The graph shows that WTP for the vaccine is typically higher in wealthier countries. All of the studies we reviewed were designed to estimate WTP to be fully vaccinated for Covid-19. None of the studies consider how people value expedited vs. delayed vaccination, which is important given that much of the policy debate revolved around how to target and ration the vaccine in the early stages of the roll out. In addition, none of the studies consider the possibility that people may "undervalue" the vaccine due to behavioral hazards.

2.3 Valuing Mortality Risk

The concept of the *value of a statistical life* (VSL) is widely used in economics to characterize the tradeoffs people make between money and fatality risks. In its most basic form, the VSL is calculated by estimating how much people are willing to pay for small reductions in mortality risk. For example, a large literature estimates VSL by measuring compensating wage differentials across jobs with different fatality risks (Kniesner et al., 2012; Kniesner and Viscusi, 2019; Leeth and Ruser, 2003; Viscusi, 1993, 2018; Viscusi and Aldy, 2003). As a hypothetical example, consider two identical jobs that only differ in terms of safety. The low risk job is completely safe with a 0 in 100,000 chance of a workplace injury resulting in death. In the high risk job, 1 in 100,000 workers die from workplace injuries in a year. Suppose that the high risk job pays $110 more per year in extra salary. A person who prefers the low risk job is willing to give up $110 per year to reduce her mortality risk by 1 in 100,000. The

[2]The median WTP estimates were taken from each of the respective studies (Banik et al., 2021; Kabir et al., 2021; Godói et al., 2021; Cerda and García, 2021; García and Cerda, 2020; Wang et al., 2021; Han et al., 2021; Lin et al., 2020; Zhang et al., 2021; Sarasty et al., 2020; Shitu et al., 2021; Goruntla et al., 2021; Harapan et al., 2020; Carpio et al., 2021; Wong et al., 2020; Arshad et al., 2021; Nguyen et al., 2021; Vo et al., 2021; Catma and Reindl, 2021; Catma and Varol, 2021; Carpio et al., 2021; Costa-Font et al., 2021). If a study did not report a median WTP estimate, we report the mean WTP estimate instead. If a study only reports a range of values for WTP, we report the mid-point of the range. All WTP estimates are reported in US 2020 dollars.

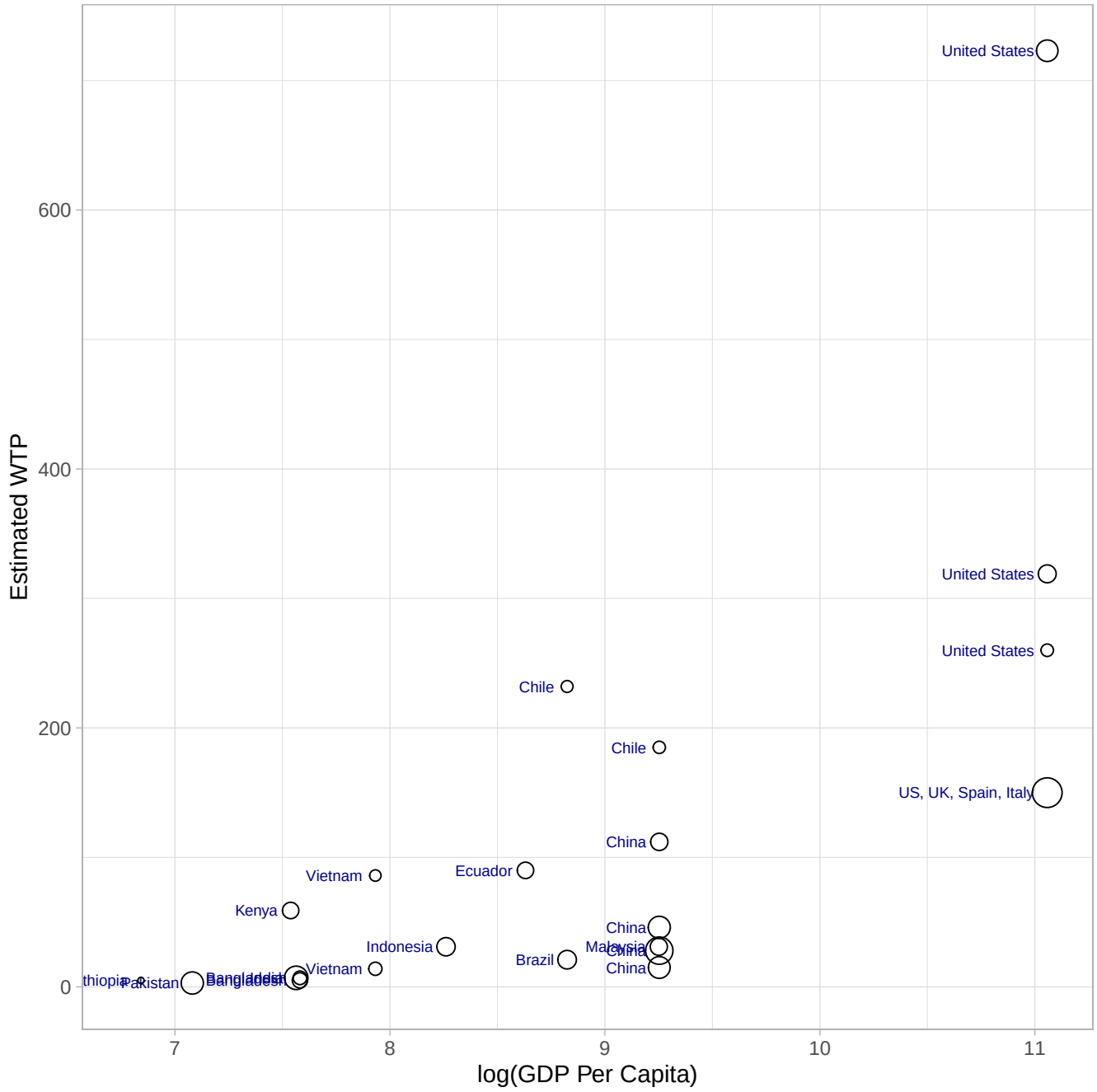
VSL is obtained by dividing the compensating wage differential by the difference in mortality risk. In this example, $VSL = \frac{\$110}{1/100{,}000} = \$11,000,000$. A pool of 100,000 workers – each paying $110 for safety – would be spending $11 million to save one life. Since the specific life saved is unknown ex ante, the measure is known as the value of a statistical life. Similar calculations are made by consumers in product markets where safety features are important. Economists have found people are willing to pay more for cars with air bags, healthy foods, high-quality medical care, etc. However, budget constraints play a role in what is affordable (Gayer et al., 2000; Liu et al., 2005; Gyrd-Hansen et al., 2008; Rohlfs et al., 2015).

Researchers have estimated VSL in the US and other countries using revealed preference approaches (labor and product market studies of compensating differentials), stated preference (questionnaires on risk valuation), or some combination across these types of studies. The results are remarkably consistent. Most studies imply that the VSL in the US is between $6 and $15 million (US$2020), with the best estimates centered around $11 million (US$2020) (Hammitt, 2020; Kniesner et al., 2012; Kniesner and Viscusi, 2019; Robinson and Hammitt, 2016; Viscusi, 2018, 2020b, 2021a,b; US Department of Transportation, 2016; US Department of Health and Human Services, 2016; Environmental Protection Agency, 2010). Three different federal agencies (HHS, DOT, and EPA) provide VSL guidelines to use in cost-benefit analyses (US Department of Health and Human Services, 2016; US Department of Transportation, 2016; Environmental Protection Agency, 2010). After updating for inflation and earnings, HHS recommends using an $11.4 million VSL, DOT recommends an $11.7 million VSL, and EPA recommends a $10.8 million VSL (US$2020). Other agencies typically use one of these estimates or turn to VSL estimates from the economics literature (Armey et al., 2021; Aldy and Viscusi, 2008; Council of Economic Advisors, 2019; Kniesner et al., 2015; Kniesner and Viscusi, 2019; Viscusi, 2018, 2019a; Viscusi and Aldy, 2003; Viscusi, 2021b).

2.4 Possible Problems With A VSL Benchmark

External VSL estimates will likely not include the perceptual errors involved in Covid-19 vaccine decisions. This makes them a natural benchmark for trying to gauge the magnitude of Covid vaccine decisions. If experimental estimates of WTP for the vaccine are substantially lower than a benchmark based on VSL estimates, that would suggest that low vaccine take-up is influenced by internalities. The ratio of the experimental WTP estimates and the VSL benchmarks provides a measure of the degree to which internalities lead people to undervalue the vaccine. We formalize the logic of this argument later in the paper.

Figure 1: Estimated WTP for the Covid-19 Vaccine Across Studies by Country GDP



Notes: The vertical axis shows the median WTP estimate from each of the 22 studies reported in Table 1. (If no median estimate was available, then the mean WTP estimate was used. If only a range was reported, then the mid-point value was used.) The horizontal axis shows the log of GDP per capita in the country where the study was conducted. The radius of the scatter points is proportional to the sample size of the study: the smallest study has $N = 301$ observations, and the largest study has $N = 4,313$ observations.

Despite the attractiveness of VSL benchmarking, there are some reasons to interpret them cautiously. One issue is that the VSL may vary substantially across people. It makes sense that the benefits of risk reduction are smaller when one has less time left, and the empirical literature does find evidence that VSL falls with age (Murphy and Topel, 2006; Robinson et al., 2021; Viscusi and Aldy, 2007; Aldy and Viscusi, 2008; Viscusi, 2020b). Since Covid-19 has a particularly high mortality rate among older people, age-specific VSL estimates may be important for thinking about how much people are willing to pay for the vaccine.[3] In addition to age, economic theory suggests that VSL may rise with income and wealth to the extent that safety is a normal good. Most studies find that the income elasticity of the VSL is around 1.0 (US Department of Transportation, 2016; Hammitt and Robinson, 2011; US Department of Health and Human Services, 2016; Lindhjem et al., 2011; Kniesner et al., 2010; Viscusi and Aldy, 2003; Viscusi and Masterman, 2017a,b; Masterman and Viscusi, 2018). Covid-19 fatalities have been concentrated among lower income sub-populations (Jung et al., 2021). In that sense, it is possible that WTP for the Covid-19 vaccine may be lower than anticipated because lower income groups are using a low VSL to make choices.

A second challenge arises because the VSL concept does not easily extend to non-marginal (large) changes in risk, in part because budget constraints matter more for non-marginal changes. For instance, most people do not seem willing to pay $1.1 million to reduce their risk of death by 1 in 10, even though this would be a good price for such a safety improvement at prevailing VSLs (Alolayan et al., 2017; Eeckhoudt and Hammitt, 2001, 2004; Hammitt, 2020; Kaplow, 2005; Robinson et al., 2021). In practice, the size of the risk reduction does not seem to distort VSL calculations until mortality risks exceed 1 per 1,000 (Hammitt, 2020; Robinson et al., 2021). Covid-19 mortality risks are far smaller than 1 per 1,000 for most people in the overall population. However, among high risk Covid-19 groups (nursing home populations, the oldest old, and people with multiple comorbidities), the fatality risk

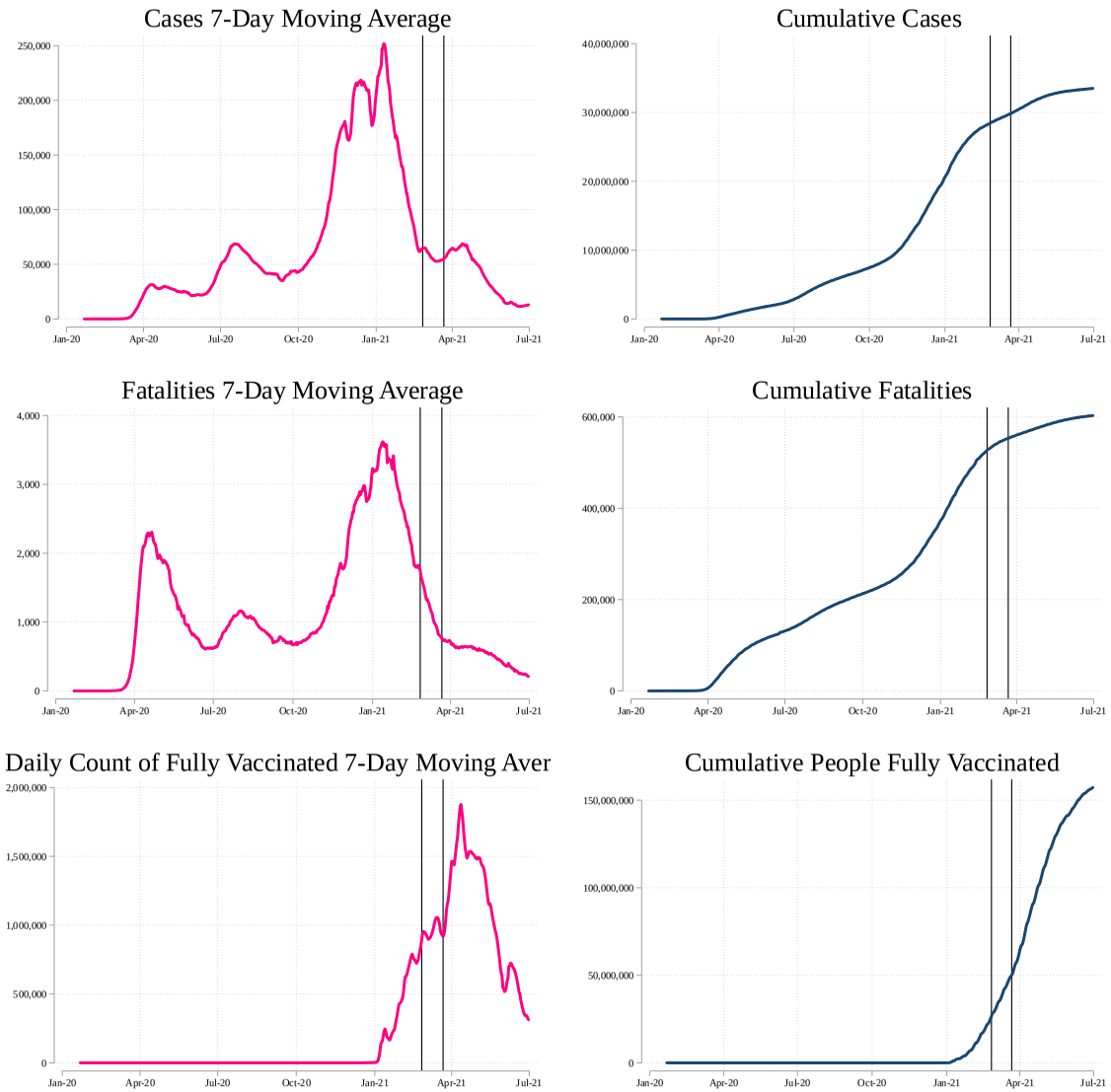
[3] A variety of papers report age-adjusted VSL in the context of the Covid-19 epidemic. Greenstone and Nigam (2020) was the first paper to apply age-adjusted VSL values to Covid-19 fatalities. They use a structural VSL model borrowed from Murphy and Topel (2006) and apply those values to projected fatalities from Ferguson et al. (2020). Using these assumptions, Greenstone and Nigam (2020) estimate a weighted average value of $4.51 million per statistical life in their analysis. Viscusi (2020b) applies a value per statistical life year (VSLY) methodology to adjust the Covid-19 fatalities by age. He finds an age-adjusted, weighted average value of $5.2 million per statistical life when applying the values across the distribution of Covid-19 fatalities. Robinson et al. (2021) use a similar methodology focusing on HHS guidelines and find an age-adjusted VSL of $4.47 million per statistical life after adjusting the values by the VSLY method. Most of the age-adjusted, Covid-19 VSL estimates in the literature are shown to be roughly equal to half of the overall population-wide VSL values. There have been ethical and empirical concerns raised in the past with using age-adjusted VSLs for benefit-cost analysis in society. These concerns are one of the main reasons why invariant VSLs (and not age-adjusted values) are typically used in analyses by government agencies. See Kniesner et al. (2006) and Viscusi (2020a) for a review of this literature.

from Covid-19 may exceed 1 in 1,000, and so these non-marginal risks may be important to consider.

A third concern is that the people may not value all reductions in mortality risk equally. A growing literature suggests people use a higher VSL when faced with risks related to a *dreaded* disease or cause of death.[4] These results may indicate that people have preferences defined over different deaths and injuries. However, it is also possible that the variation in cause of death specific VSLs reflects biases from the salience of particular symptoms, incorrect beliefs about health conditions and treatments, or non-standard decision processes. An important question is whether these kinds of biases are relevant for a disease like Covid-19 and for preventive actions like receiving the Covid-19 vaccine. In a paper closely related to this one, Liu et al. (2005) examine WTP to reduce the risk of infection and death from severe acute respiratory syndrome (SARS) in Taiwan during the spring of 2003. Their implied VSL estimates suggest that SARS fatalities were valued higher than normal fatalities in Taiwan by between 25% to 435%. Likewise, Gyrd-Hansen et al. (2008) use a survey questionnaire to determine WTP estimates to avoid fatalities related to an influenza pandemic in Norway. Combining the WTP estimates with fatality risk measures, the authors estimate a mean implied VSL of $9.7 million per statistical life (US$2008), which was at the upper range of the VSL estimates at the time (Viscusi and Aldy, 2003; De Blaeij et al., 2003; Elvik, 1995).

[4]The impact of dread and uncertainty on VSL and WTP estimates comes up quite often in the literature, not only for epidemics, but also for other types of deaths as well (Chilton et al., 2006; Hammitt and Liu, 2004; Jones-Lee and Loomes, 1995; Liu et al., 2005; McDonald et al., 2016; Riddel and Shaw, 2006; Robinson et al., 2010; Viscusi et al., 2014; Viscusi, 2019b). Viscusi et al. (2014) find cancer fatalities have a VSL premium of around 21% in comparison to "regular" deaths. Robinson et al. (2010) study WTP to avoid terror attacks and find that people seem to use a VSL that is twice as high as the typical VSLs reported in the literature.

Figure 2: Covid-19 Cases, Fatalities, and Vaccinations



Notes: The pink lines show the 7-day moving average Covid-19 cases, fatalities, and vaccinations in the US. The navy lines show the cumulative Covid-19 cases, fatalities, and vaccinations in the US. The figure plots trends from January 2020 through July 2021. The vertical, black lines indicate the study period which begins 2/25/21 and ends 3/22/21. Data come from the New York Times database.

3 MSN Survey and Experiments

We collected data using an Internet survey distributed using banner ads displayed on Microsoft News (www.msn.com) between February 25, 2021 and March 22, 2021. Participation in the survey was voluntary: people clicked on the banner ad to complete the survey (Dixon et al., 2021). The survey instrument collected information on the respondent's demographic characteristics, zip code, and Covid-19 related experiences, knowledge, and opinions. We linked the survey records with county-level data on confirmed Covid-19 cases and deaths from the database maintained by The New York Times (2021).[5] Figure 2 shows the state of the pandemic at the time of the survey. The graphs in the left panel show 7-day moving averages for cases, fatalities, and vaccinations while those in the right panel show cumulative cases, fatalities, and vaccinations. The vertical black lines represent the beginning and end of our survey period. The graphs indicate that the survey was in the field as cases and fatalities fell following a spike during Christmas and New Year and as the vaccination effort began in the US.

3.1 Discrete Choice Experiments

The survey also included an experimental module in which we implemented two discrete choice experiments. The first experiment examined willingness to pay for early vaccination. The second examined willingness to accept compensation for delaying the second dose of the vaccine.

Early Vaccination Experiment In the early vaccination experiment, respondents were presented with the following vignette:

> In a typical year, many people choose to be vaccinated at local pharmacies for diseases like influenza, shingles, meningitis, and chicken pox. You can pay for out-of-pocket for most vaccinations. Prices range from about $50 for a flu shot to about $180 for a shingles vaccine. Of course, many of these vaccines are also covered by health insurance plans and so many people do not pay an out-of-pocket price.

[5] To perform the zipcode to county record linkage, we used a crosswalk managed by the US Department of Housing and Urban Development's Office of Policy Development and Research. When a zipcode belongs to multiple counties, we assigned the zip code to the county that represents the highest share of its addresses. (HUD Policy Development and Research, 2021) The New York Times data on Covid-19 cases and deaths sometimes uses non-standard geography by combining cases and deaths from certain counties. For example, the five counties in New York City (New York, Kings, Queens, Bronx and Richmond) are combined into a single geographical unit called "New York". Therefore, we also combine New York City zip codes into a single geographic unit for our analysis.

> Suppose that in 4 months, you will be able to receive the COVID-19 vaccine for free. However, for a fee, you could receive the COVID-19 vaccine today at a local pharmacy in the same way that people are often vaccinated for other conditions such as influenza, shingles, meningitis. That is, you would pay the fee and a pharmacist or nurse practitioner would administer an injection.

Subjects were randomly assigned to one of five treatment arms with different price offerings: \$50, \$100, \$200, \$500, \$1000. Let p_j for $j = 1...5$ represent the price offering in the j^{th} treatment arm. After reading the vignette, people were asked the following discrete choice question:

> If the out-of-pocket fee was \$$p_j$, would you choose to pay the fee and be vaccinated today rather than wait 4 months?

People answered Yes or No to the question, and the responses became the main dependent variable in our analysis of the early vaccination experiment.

Delayed Second Dose Experiment In the delayed second dose experiment, respondents were presented with the following vignette:

> The Covid-19 vaccine is designed to be administered in two doses spaced a few weeks or months apart. Most of the protective effect of the vaccine occurs with the first dose. Given limited supplies of the vaccine, some researchers argue that we should delay giving people second doses until more people have received a first dose. Some people are more comfortable with this option than others. Suppose that you have just been vaccinated with your first dose.

As before, subjects were randomly assigned to one of five treatment arms differentiated by transfer offerings: \$5, \$10, \$50, \$100, \$200. Let t_j represent the bonus offering in the j^{th} group. After reading the vignette, people were asked the following question:

> Would you agree to delay your second dose for 3 months in return for a cash bonus of \$$t_j$?

People answered Yes or No to the question, and these responses became the main dependent variable in our analysis of the delayed second dose experiment.

3.2 Survey Weights

Because the survey sample is not a random sample from a well-defined population, we developed survey weights to bring the age, gender, and race/ethnicity distribution of the survey sample in line with the US population. To construct the weights, we obtained the 2019 American Community Survey (ACS) and limited the sample to adults (18+). We harmonized the ACS measures of age, gender, and race/ethnicity to match the coding used in the MSN data, and stacked the MSN and ACS data into a single data file. In the stacked data file, we let A_i be a dummy variable indicating that person was drawn from the ACS data rather than the MSN data. X_i is a vector of harmonized demographic covariates: indicators for gender, Hispanic ethnicity, racial groups (White, Black, Asian, Native American, Other), and six adult age ranges (18-19, 20-34, 35-44, 45-54, 55-64, 65+). We fit logistic regression models of ACS membership (A_i) on the covariates (X_i).[6] We used the estimated coefficients from the model to compute the predicted probability that each person belongs to the ACS sample. For each member of the MSN sample, the survey weight is $w_i = \hat{p_i}/(1 - \hat{p_i})$, where $\hat{p_i}$ is the predicted probability for person i. Table 2 shows summary statistics comparing the ACS estimates of the composition of adults in the US with the raw MSN sample and the weighted MSN sample.[7] The procedure re-weights the MSN sample so that it mimics the characteristics of the ACS data. Although the weights make the sample more representative on basic demographics, they do not guarantee that the MSN data are representative of the US population of adults on additional non-demographic factors that were not accounted for in our logistic regressions. Thus, the external validity of our results should be interpreted cautiously.

[6] We estimated the models using ACS sampling weights for the ACS data and setting the weight equal to 1 for each member of the MSN sample.

[7] In practice, we normalize the weights to sum to 1 across observations in the MSN sample.

Table 2: Weighted and Unweighted Demographic Variable Means

Variable	ACS	Raw MSN	Weighted MSN
18–39	0.39	0.08	0.44
40–64	0.40	0.40	0.38
65+	0.22	0.52	0.18
Female	0.51	0.38	0.54
White	0.63	0.78	0.56
Black	0.12	0.06	0.13
Hispanic	0.16	0.08	0.19
Asian	0.06	0.03	0.09
Other	0.03	0.04	0.03
N	2,600,000	1737	1737

Notes: The first column shows weighted averages in the ACS data for each demographic variable. The second column shows the averages in the raw data from the MSN experiment for each demographic variable. The third column shows the weighted averages in the data from the MSN experiment for each demographic variable. The overall number of respondents of the survey was 2157, but we drop 420 due to non-response to questions relevant for analysis. This leaves N=1737 total respondents included in the main analysis.

4 Vaccine Choices With Internalities

We interpret the discrete choice experiments from the perspective of a behavioral model in which people's choices may be influenced by errors in the way people perceive the benefits of vaccination (Mullainathan et al., 2012). These errors drive a wedge between the choices people make in the experiment and the choices that would maximize their "experienced utility".

In the early vaccination experiment, people are asked whether they would prefer to be vaccinated now for a specified price of p, rather than wait four months to be vaccinated for free. Suppose that c_i is the person's initial wealth and ω_i is the benefit of early vaccination expressed in wealth-equivalent units. e_i is a perceptual error – also expressed in units of wealth. The person's utility is an increasing and concave function of final wealth, net of real and perceived vaccination benefits and costs. Specifically, the person's utility function is $u_i(c_i + v_i(\omega_i + e_i - p))$, where $v_i = 1$ if the person opts for early vaccination and $v_i = 0$ if she chooses to forgo early vaccination. The person opts for early vaccination if $\omega_i + e_i > p$. When $e_i = 0$, there is no perceptual error, and the person chooses early vaccination when the private benefits exceed private costs. In contrast, when $e_i < 0$, the person makes choices with an internality: *perceived* private benefits are smaller than the real benefit she would experience if vaccinated. The internality leads people to forego early vaccination inappropriately if $w_i - p < -e_i$, so that the perceptual error is larger than the net-of-price true benefit of early vaccination.[8]

In the absence of the perceptual error, ω_i is the highest price the person would be willing to pay (WTP) for early vaccination. In other words, ω_i is is the person's true or *experienced* WTP for early vaccination. In contrast, $\omega_i + e_i$ represents the person's *decision* WTP, which is the highest price the person is willing to pay for early vaccination when the perceptual error – e_i – is included in the calculus. We assume the benefits of vaccination are derived from its effects on the risk of mortality. Suppose that being vaccinated four months early reduces mortality by m deaths per $100,000$. Then the person's true or experienced VSL is $VSL_i^E = \frac{\omega_i}{m}$. This is a measure of how much wealth the person would be willing to give up in return for the mortality reduction produced by the vaccine, in the absence of the perceptual errors.[9] When perceptual errors do influence decisions, the person will make choices that

[8] A perceptual error smaller than $-(wi - p)$ would create a non-binding internality because it would not change the person's discrete choice decision outcome. When $e_i > 0$, the perceptual error would lead people to overvalue the vaccine and could lead some people to opt for vaccination even when the true benefits do not justify the costs.

[9] In this framework, we assume that the private benefits a person experiences from the vaccine are derived only from the way the vaccine reduces the person's risk of mortality. In practice, the vaccine may produce

imply a different valuation of mortality risk. In particular, $VSL_i^D = \frac{\omega_i + e_i}{m}$ represents the person's decision VSL. In the case of an internality, $VSL_i^D < VSL_i^E$ implies that people make choices about risk using a VSL that is too low relative to their true preferences.

In the delayed second dose experiment, the person's utility is $u_i(c_i + s_i(t_i - \mu_i - e_i))$, where $s_i = 1$ when the second dose is delayed, t_i is transfer payment for delaying, μ_i is the benefit from an on-schedule second dose, and e_i is the perceptual error. In the absence of perceptual errors, μ_i is the smallest transfer that the person would be willing to accept (WTA) in return for delaying the second dose. When perceptual errors are involved, $\mu_i + e_i$ is the smallest transfer the person would be WTA to delay. When $e_i < 0$, people do not internalize the full private benefits of the on-schedule second dose. If completing an on-time second dose reduces the risk of mortality by m deaths per $100,000$, then the person's experienced VSL is given by $VSL_i^E = \frac{\mu_i}{m}$. Her decision VSL is $VSL_i^D = \frac{\mu_i + e_i}{m}$. An internality manifests as a lower effective WTA: people are *too willing* to give up their second dose. They act as if their VSL is low because $VSL_i^D < VSL_i^E$.

The ratio of decision utility parameters to experience utility parameters provides one way to measure the degree to which perceptual errors affect the demand for early vaccination and second doses. Specifically, $I_{EV} = \frac{\omega_i + e_i}{\omega_i} \times 100$ expresses the person's decision WTP as a percentage of her true WTP for early vaccination. Similarly, $I_{DSD} = \frac{\mu_i + e_i}{\mu_i} \times 100$ represents the person's decision WTA as a percentage of her true WTA compensation for delaying the second dose. Both of these measures will be less than 100 in the case of an internality.

5 Econometric Methods

5.1 Experimental Estimates of WTP, WTA, and VSL

We use the experimental data to construct estimates WTP, WTA, and VSL that are *inclusive* of perceptual errors related to Covid-19 vaccines. Let $i = 1...N_{MSN}$ index the participants in the discrete choice experiments. In the early vaccination experiment, let $V_i(p) = 1(\omega_i + e_i \geq p)$ be a binary potential outcome indicating whether the person would choose early vaccination if the out-of-pocket price was p. As before, ω_i represents the person's true benefits for earlier vaccination, and e_i represents a perceptual error. Then $WTP_i^E = \omega_i$

other benefits as well. In addition to reducing mortality, it may reduce the risk of non-fatal illness, reduce the risk of transmitting Covid-19 to another person, and provide a sense of social participation. In principle, the person's valuation of the vaccine – ω_i is a valuation of the entire bundle of attributes. In this more complex setting, the VSL would have to be derived by estimating the component of ω_i that comes from mortality risk, holding constant these other attributes. We abstract from that complexity in our analysis. But we return to the issue of non-mortality benefits of vaccination in the discussion section of the paper.

is the person's "experienced WTP", and $WTP_i^D = \omega_i + e_i$ is the person's "decision WTP". P_i is the person's randomly assigned price offering. Random assignment makes it credible to assume that price offerings are statistically independent of a person's true benefits and perceptual errors regarding vaccination. As a result, each of the five arms of the experiment uncovers a point along the cumulative distribution function of WTP_i^D in the study sample. To see the argument, consider subjects assigned to the j^{th} arm of the study so that $P_i = p_j$. One minus the expected value of the realized choice in this arm of the study is:

$$\begin{aligned} 1 - E[V_i|P_i = p_j] &= 1 - E[V_i(p_j)|P_i = p_j] \\ &= 1 - Pr[\omega_i + e_i \geq p_j|P_i = p_j] \\ &= 1 - Pr[\omega_i + e_i \geq p_j] \\ &= F_{WTP}^D(p_j) \end{aligned}$$

The first and second equality replace realized choice outcomes with the underlying structure. The third equality imposes independence, and the fourth equality follows from the definition of a cumulative distribution, where $F_{WTP}^D(p) = Pr(\omega_i + e_i \leq p)$. A parallel argument applies to the analysis of the Delayed Second Dose experiment. The take-up rates in the five arms of that study uncover values of $F_{WTA^D}()$ at each of the compensation offerings.[10]

The randomized experimental design provides a credible way to identify several points along the distributions represented by $F_{WTP^D}(p_j)$ and $F_{WTA^D}(t_j)$. We estimate these points from the data in two different ways. First, we form non-parametric estimates by simply computing the take-up rates in each arm of the experiment. For instance, $\widehat{F_{WTP}^D(100)} = 1 - \frac{\sum_{i=1}^N V_i \times 1(P_i=100)}{\sum_{i=1}^N 1(P_i=100)}$ is a simple estimator of the share of the population with decision WTP less than \$100. To improve the external validity of the estimated distributions, we also estimate these sample means using the survey weights. To improve statistical precision, we also use simple regression models to estimate points along the two CDFs. Specifically, we fit regressions of take-up on a linear function of price: $V_i = \alpha + \beta \times P_i + \epsilon_i$. With estimated regression coefficients in hand, the height of the CDF can be estimated using fitted values for each price point of interest. For example, $\widetilde{F_{WTP}^D(100)} = 1 - (\hat{\alpha} + \hat{\beta} \times 100)$ is a simple

[10]In this case, we let μ_i represent the person's experienced benefit from an on-schedule second dose, and we let e_i represent the perceptual error. Then $WTA_i^E = \mu_i$ is the person's true or *experienced* WTA compensation in return for delaying the second dose, and $WTA_i^D = \mu_i + e_i$ is the person's *decision* WTA. Then $F_{WTA^D}(t)$ is the cumulative distribution of decision WTA in the study population. $S_i(t)$ is a potential outcome indicating whether the person chooses to delay the second dose when offered a transfer worth t. The five arms of the study uncover values of $F_{WTA^D}()$ at each of the compensation offerings.

estimator of fraction of people with decision WTP less than \$100. Our preferred approach fits the models using weighted least squares to incorporate the survey weights, which improves external validity. We use the same basic approach to estimate the distribution of decision WTA from the second dose experiments. In practice, the linear regressions provide a very close correspondence to the sample means but with tighter standard errors. Throughout, we estimate the distribution of WTP^D and WTA^D for the whole sample and for three separate age groups (18-39, 40-64, and 65+) to accommodate age-related differences in Covid-19 mortality (Centers for Disease Control and Prevention, 2021). We also explore heterogeneous responses that allow the coefficient on price to vary with age, stated vaccine hesitancy, and geographic variation in the local severity of the epidemic.

To construct estimates of decision VSL in the experiments, we estimate the expected reduction in fatality risk from being vaccinated four months early. Among all adults in the US, there were 25.04 Covid-19 deaths per $100,000$ during the four month period between March 1 and June 30, 2021. Haas et al. (2021) find that the vaccine is 96.7% effective at preventing Covid-19 deaths.[11] Thus, $\widehat{m_{EV}} = .967 \times 25.04 = 24.21$ represents the expected reduction in fatality risk from being vaccinated four months early for all adults.[12] We compute the implied VSL at the price in each arm of the experiment. Specifically, $VSL_p = \frac{p}{\hat{m_{EV}}}$ represents the implied decision VSL for a person with $WTP_i^D = p$.

The WTA estimates from the delayed second dose experiment also imply a VSL. To construct an estimate of the mortality effects of delayed dose, we assume that delaying the second dose increased expected fatality risks by about 24.7%.[13] Pooling all adults, there were 21.96 Covid-19 deaths per $100,000$ people in the US during the three month window between March 1 and May 31, 2021. Accordingly, we let $\widehat{m_{DSD}} = .247 \times 21.96 = 5.42$ be the expected increase in fatality risk from delaying the second dose by three months. The implied VSL at transfer t is $VSL_p = \frac{c}{\hat{m_{DSD}}}$. This is the VSL a person would be using if her WTA compensation for delaying the second dose was exactly equal to \$$c$.

5.2 Benchmark VSL Calculations

The challenge in determining whether people undervalue vaccines due to internalities is that the experiments do not separately identify the true WTP/WTA parameters and the per-

[11] This study was conducted in Israel. While the setting is not the same for the respondents in our study, the time periods are very similar. Their analysis was conducted from January 24 to April 3, 2021.

[12] We compute corresponding estimates for other age groups by changing the baseline Covid-19 fatality rates.

[13] To arrive at this number, we took 96.7% and subtracted 72% using Dagan et al. (2021)'s estimate of first dose efficacy. This study was conducted in Israel December 20, 2020 to February 1, 2021.

ceptual errors. To make progress, we compare the experimental estimates with benchmarks based on external estimates of VSL. Specifically, let VSL_{HHS} represent the point estimate of VSL that the US Department of Health and Human Services (HHS) uses in its benefit-cost analyses. If $\hat{m}$ is the expected reduction in fatality risk from earlier vaccination, then a person using the standard VSL would be willing to pay $\omega_{HHS} = VSL_{HHS} \times \hat{m_{EV}}$ in order to be vaccinated four months early. Similarly, if $\hat{m_{DSD}}$ is the increase in fatality risk associated with delaying the second dose, then a person using the HHS VSL would be willing to accept $\nu_{HHS} = VSL_{HHS} \times \hat{m_{DSD}}$ in compensation for delaying the second dose of the vaccine by three months. With the the benchmark estimates in hand, we measure the degree of undervaluation due to internalities using the ratio of the approximate median decision WTP to the implied WTP from the VSL benchmark. These are our empirical analogues to I_{EV} and I_{DSD} metrics described in the theoretical model. In addition to benchmarks based on the HHS VSL, we also examine benchmarks based on other external VSL estimates, including age-adjusted VSLs.

6 Results

Table 3 shows the age, gender, and racial composition of each of the five treatment groups in the two discrete choice experiments. There are between 343 and 349 people in each arm of the experiment. The groups are well-balanced on measured covariates, supporting the stronger assumption that the groups are also well-balanced on unmeasured traits that determine WTP, WTA, and VSL.

Table 3: Covariate Balance in the WTP and WTA Experiments

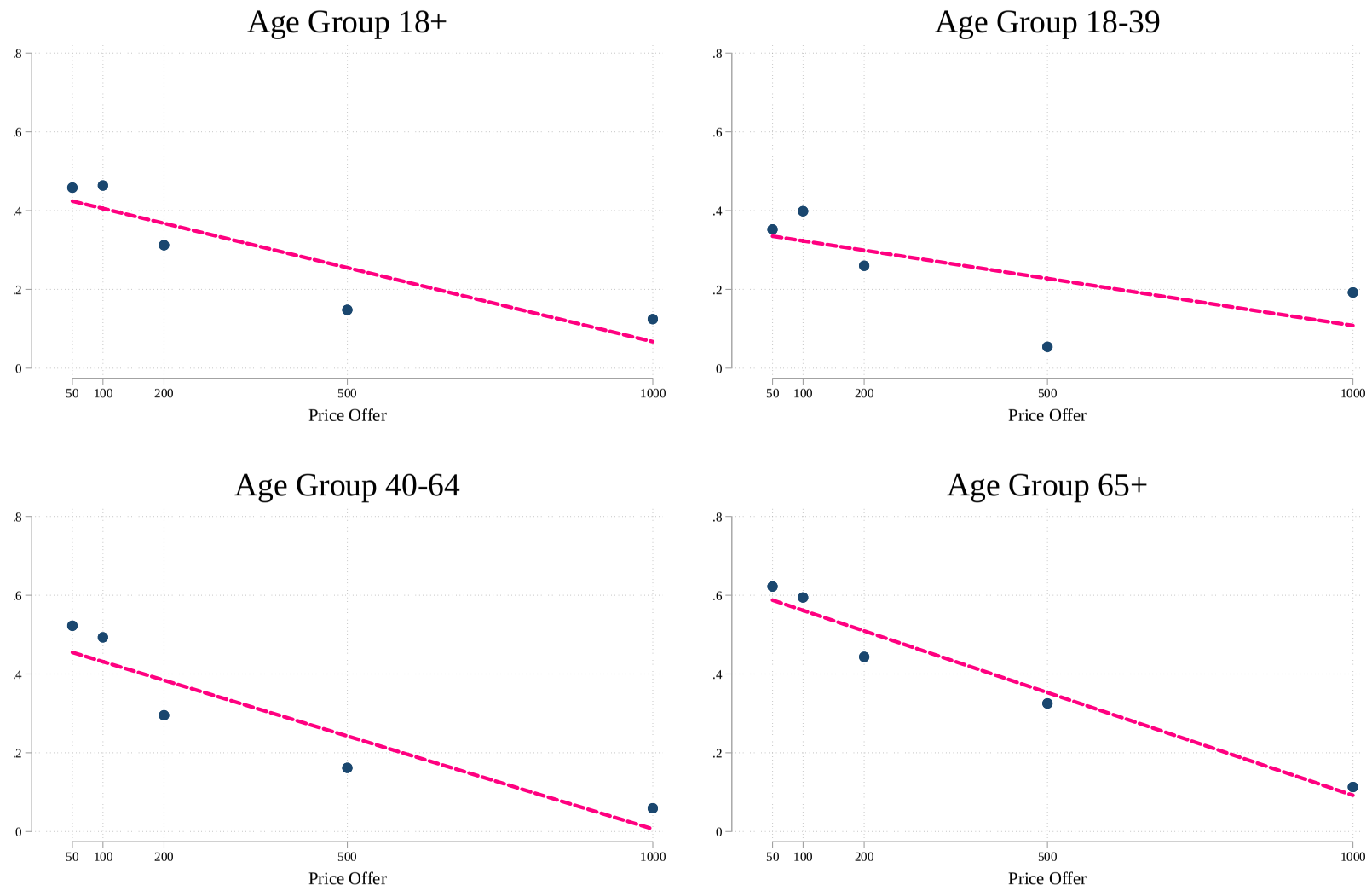
Panel A: WTP Experiment					
	\$50 Offer	\$100 Offer	\$200 Offer	\$500 Offer	\$1000 Offer
18–39	0.10	0.12	0.07	0.09	0.09
40–64	0.50	0.47	0.55	0.49	0.47
65+	0.40	0.41	0.38	0.41	0.44
Female	0.37	0.38	0.33	0.41	0.39
White	0.72	0.78	0.80	0.78	0.80
Black	0.07	0.04	0.05	0.07	0.06
Hispanic	0.10	0.08	0.07	0.08	0.06
Asian	0.03	0.04	0.03	0.02	0.03
Other	0.07	0.05	0.05	0.05	0.05
N	345	346	348	349	349
Panel B: WTA Experiment					
	\$5 Offer	\$10 Offer	\$50 Offer	\$100 Offer	\$200 Offer
18–39	0.09	0.10	0.09	0.08	0.12
40–64	0.48	0.50	0.49	0.54	0.50
65+	0.43	0.41	0.42	0.38	0.37
Female	0.35	0.34	0.39	0.42	0.37
White	0.77	0.76	0.77	0.80	0.78
Black	0.06	0.07	0.05	0.06	0.06
Hispanic	0.07	0.09	0.07	0.07	0.08
Asian	0.03	0.05	0.04	0.02	0.03
Other	0.07	0.04	0.07	0.05	0.05
N	348	343	345	343	345

Notes: Panel A shows the average of each demographic variable for each of the five price offerings. Panel B shows the average of each demographic variable for each of the five transfer offerings. The averages are unweighted.

Figure 3 plots the fraction of participants who preferred to be vaccinated four months early in each of the five price offering groups. The navy points show the average take-up rate in each arm of the study, computed using the survey weights. The dashed pink lines show fitted values from a linear regression of take-up on randomly assigned prices, also estimated using the survey weights. The top left panel shows the full sample (18+), and the remaining panels show the results for people 18-39, 40-64, and 65+. In all four age groups, the proportion of participants who says they would choose early vaccination is falling as out-of-pocket price rises, and the regressions provide an excellent approximation to the simple averages. Column 1 of Table 6 shows the estimated intercept and slope from the linear model for the full sample. The slope on price is 0.0004, which implies that a $100 increase in prices would reduce vaccine take-up by about 4 percentage points. Column 2 shows estimated coefficients from a regression of take-up on dummy variables each treatment arm from $100 to $1,000; the $50 arm is the reference group. Fitted values from these regressions are equal to the sample means plotted in the graph. The coefficients from the models show that the vaccine demand was substantively and statistically significantly lower in the higher price treatment arms. The top panel of Figure 3 shows that – in the overall sample (18+) – the take-up rate never rises above 0.50. About 42% of the sample chose early vaccination when assigned a price offering of $50. This implies that $50 represents the $100 \times (1 - .42) = 58^{th}$ percentile of the distribution of WTP in our study population. In other words, more than half of the respondents were willing to pay less than $50 to be vaccinated. The oldest age group (65+) had higher early vaccine take-up rates than younger age groups; the lone exception to this pattern is in the $1000 experimental offer where the 18-39 age group had a slightly higher take-up rate.

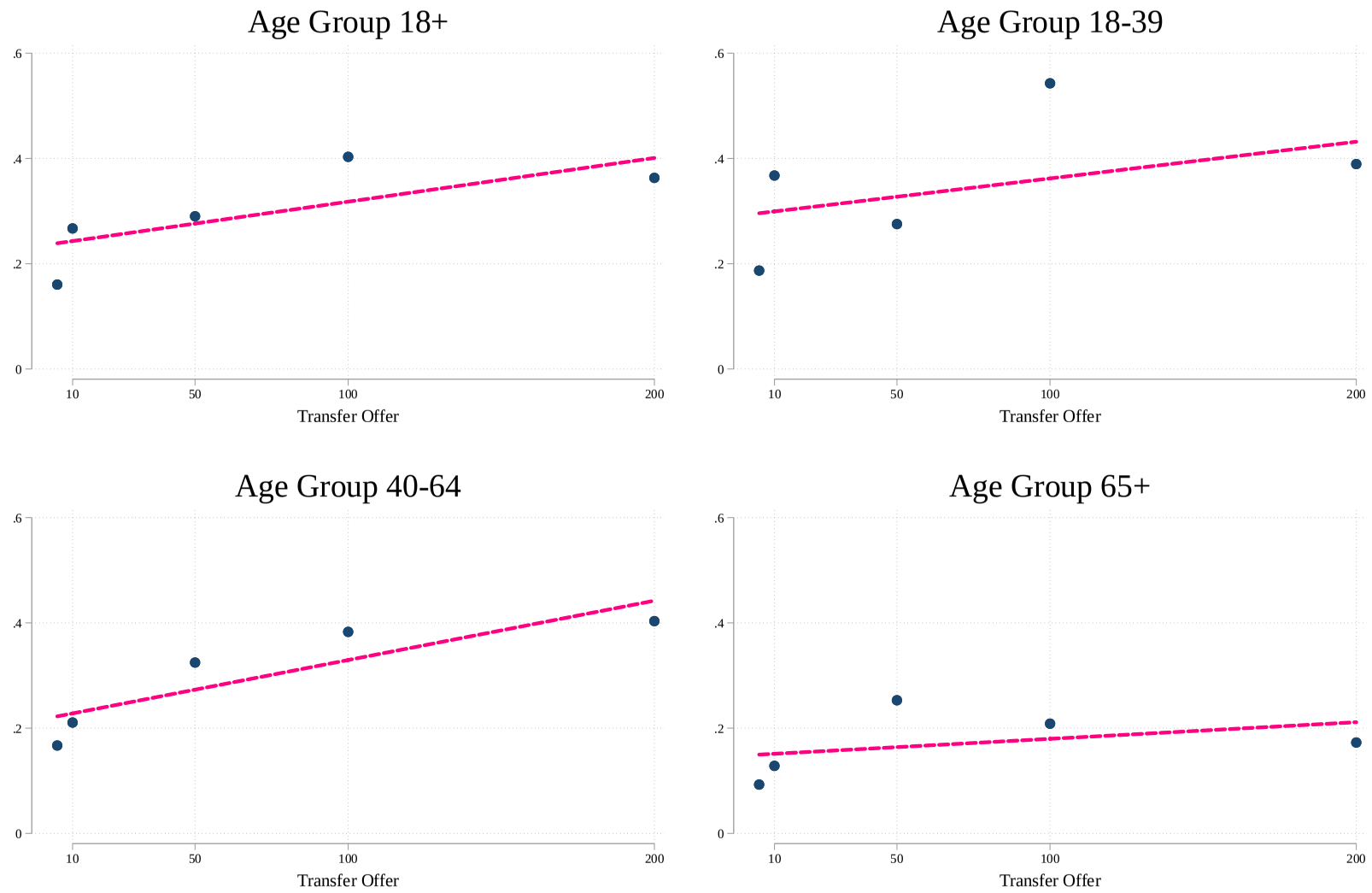
Figure 4 shows a parallel set of results from the Delayed Second Dose experiment. This time, the graphs plot the weighted proportion of participants who said they would be willing to delay their second dose for three months at each transfer offering. The navy points show the take-up rates in each arm of the experiment. The dashed pink lines are the fitted values from the weighted regressions. The lines slope up, which illustrate that more people are willing to delay the second dose as the transfer on offer increases in value. Table 7 shows parameter estimates from the linear model and dummy variable specifications for the full sample.

Figure 3: Take-up of the Early Vaccine by Price Offer



Notes: The navy points show the weighted average take-up of the first shot for each randomly assigned price in the vignette. The dashed pink line shows the predicted values of a weighted linear regression of take-up of the first shot on price. Each panel presents a different age group. Table 6 shows the regression results for the full sample; column 1 presents the regression results that form the pink line, while column 2 presents the non-parametric results that form the navy scatterplot.

Figure 4: Willingness to Delay Second Dose by Transfer Offer



Notes: The navy points show the weighted proportion of respondents who agreed to delay the second shot for each randomly assigned transfer in the vignette. The dashed pink line shows the predicted values of a weighted linear regression of agreeing to delay the second shot on transfer. Each panel presents a different age group. Table 7 shows the regression results for the full sample; column 1 presents the regression results that form the pink line, while column 2 presents the non-parametric results that form the navy scatterplot.

Comparing the two experiments suggests that demand for early vaccination (Figure 3) is more price sensitive than demand for timely second doses (Figure 4). For example, using the extreme values ($50 and $1000) of the price offerings, the arc price elasticity of demand for early vaccination is $\frac{.07-.42}{.245} \times (\frac{\$1000-\$50}{\$525})^{-1} \approx -.8$, implying that a 10% increase in the price would reduce take-up of early vaccination by about 8%. In contrast, computed with the extreme values of $5 and $200, the arc elasticity of delayed second doses was $\frac{.40-.24}{.32} \times (\frac{\$200-\$5}{\$102.50})^{-1} \approx .3$. Raising the compensation offer by 10% increases the fraction of people willing to delay the second dose by 2.6%. As we explained earlier, the results of the clinical trials and post-market studies, suggest that mortality risk falls by about 24.21 deaths per $100,000$ for the first dose and by about 5.42 deaths per $100,000$ for the second dose. Following the intuition about the standard (non-behavioral) case in Baicker et al. (2015), we would have expected demand for the first dose to be more inelastic than demand for the second dose. The pattern of elasticities across the two experiments suggests that behavioral errors may play a role in vaccine demand. In addition to being less price sensitive when it comes to delaying the second dose, people also have lower levels of take-up for delaying the dose at any price. Fewer than half of the respondents in each age group said they would be willing to delay the second dose at any of the price offerings. Once again, the oldest age group (65+) appears to value the vaccines more. Across every experimental offering, the 65+ age group has a lower proportion of participants willing to delay their second dose.

Table 4 shows our efforts to compare decision utility and experience utility based valuations for the early vaccination experiments. Panel A shows the take-up rates observed in each arm and age group in the experiment, based on fitted values from the linear regression models. Subtracting these take-up rates from 1 leads to estimates of the CDF of decison WTP at each price level. For example, the estimates imply that in the full sample, $50 represents the 58^{th} percentile of the distribution of decision WTP among adults. For simplicity, we treat $50 as the approximate median of the decision WTP distribution.

Panel B shows the Covid-19 fatality risk for the adults over 18 population during the four months following our experiment was about 25.04 per $100,000$ people. Recent work by Haas et al. (2021) suggests that vaccination reduces the fatality rate by 96.7%, which implies that being vaccinated four months early would reduce a person's Covid-19 fatality risk by 24.21 per $100,000$. This is a large reduction in risk overall. However, there are substantial differences across age groups in the fatality risk reduction associated with early vaccination. Among people over 65, early vaccination would reduce fatality risk by 78.87 per $100,000$. But in the 18-39 age group, early vaccination would reduce fatality risk by only 1.74 per 100,000.

Table 4: Vaccinations, Willingness to pay, Fatality Risk, and Implied Covid-19 VSL Estimates (Four Month Early Vaccination Experiment)

	18+	18–39	40–64	65+
Panel A: Proportion Who Respond Yes				
$50 Experiment	0.42	0.34	0.45	0.59
$100 Experiment	0.41	0.32	0.43	0.56
$200 Experiment	0.37	0.30	0.38	0.51
$500 Experiment	0.26	0.23	0.24	0.35
$1000 Experiment	0.07	0.11	0.01	0.09
Panel B: Covid-19 Fatality Risk				
Covid-19 fatalities (from 3/1/2021 - 6/30/2021)	64,577	1,767	18,518	44,292
Population (in millions)	257.94	98.38	105.26	54.30
Covid-19 Fatality Risk (per 100,000)	25.04	1.80	17.59	81.56
96.7% Fatality Risk Reduction (per 100,000)	24.21	1.74	17.01	78.87
Panel C: Implied VSL Estimates (in 2020 millions of dollars)				
$50 Experiment Offer	$0.21	$2.88	$0.29	$0.06
$100 Experiment Offer	$0.41	$5.76	$0.59	$0.13
$200 Experiment Offer	$0.83	$11.52	$1.18	$0.25
$500 Experiment Offer	$2.07	$28.79	$2.94	$0.63
$1000 Experiment Offer	$4.13	$57.58	$5.88	$1.27
Panel D: Expected Median WTP Estimates				
HHS Age-Invariant VSL (in 2020 millions of dollars)	$11.41	$11.41	$11.41	$11.41
Expected Median WTP Estimate (using invariant VSL)	$2,761	$198	$1,940	$8,996
Age-Adjusted VSL (in 2020 millions of dollars)	$11.41	$12.62	$9.86	$3.99
Expected Median WTP Estimate (using age-adjusted VSL)	$2,761	$219	$1,678	$3,148

Notes: Proportions in Panel A are based on weighted averages of take-up of early vaccination for each price offering and age group. The population data in Panel B are available at: https://www.census.gov/data/tables/2020/demo/popest/2020-demographic-analysis-tables.html. The fatality data in Panel B are available at: https://data.cdc.gov/NCHS/Provisional-COVID-19-Deaths-by-Sex-and-Age/9bhg-hcku/data. The Implied VSL Estimates in Panel C assume a 96.7% fatality risk reduction (Haas et al. 2021) due to early vaccination by four months. The Expected Median WTP in Panel D is based off HHS estimates of Age-Invariant and Age-Adjusted VSL.

Panel C shows the implied VSL estimates for each of the experimental price offerings, which are constructed by dividing each price by the expected reduction in mortality risk. Using the full sample of adults, a person who is willing to pay \$50 to be vaccinated 4 months early would be using (implicitly) a VSL of $\frac{\$50}{24.21 \times 100{,}000^{-1}} \approx \$210,000$. Likewise, someone willing to pay $\$1,000$ for early vaccination would be using a VSL of \$4.13 million. Moving across the columns in Panel C gives implied VSLs for each age group: the reduction in mortality risk is larger for older people, and so the implied VSL at any price point is falling with age.

Panel D in Table 4 shows how the implied VSL estimates from the discrete choice experiments compare to more standardized values in the general VSL literature (US Department of Health and Human Services, 2016; Robinson et al., 2021). The calculations from Panel B indicate the 18+ age group could reduce their fatality risk by 24.21 per 100,000 by being vaccinated four months early. How much should someone be willing to pay for this fatality risk reduction?

HHS recommends using a VSL of \$11.41 million for benefit-cost analysis; this number is a typical point estimate from the better studies in the economics literature. The HHS VSL implies that people are willing to pay \$114.10 to reduce their mortality risk by 1 per $100,000$. Thus, a person with the HHS VSL would be willing to pay up to $\$11.41 \text{ million} \times 0.0002421 = \$2,761$. Right away, it is clear that a WTP of $\$2,761$ would be a substantial outlier in the distribution of WTP implied by our Early Vaccination experiment. As Panel A shows, the take-up rate in the \$50 group was only 42%, implying that median willingness to pay to be vaccinated four months early was less than \$50 in the experiment. Treating \$50 as the median decision WTP and $\$2,761$ as the median experienced WTP suggests that decision utility is only $\frac{\$50}{\$2,761} \times 100 \approx 1.8\%$ of experienced utility. Equivalently, we can measure undervaluation using the ratio of implied VSL to the HHS VSL produces at the median WTP. That gives: $\frac{\$210,000}{\$11,410,000} \times 100 \approx 1.8\%$ of the full HHS VSL.

The implied VSLs shown in Table 4 are below the HHS value of \$11.41 million for nearly all of the age groups and price levels that would occupy a substantial share of the WTP distribution. And this same basic result holds even when we allow for an age-adjusted VSL. The age-adjusted calculations indicate the average person should have WTP of $\$2,761$ with values ranging from \$219 for younger people and $\$3,148$ for the older people. These benchmark estimates of WTP given conventional VSL numbers are much larger than the WTP estimates we recovered from the discrete choice experiment. Taken together, our analysis suggests that the people in our study undervalued the mortality risk reduction provided by the Covid-19 vaccines by an economically important margin. One interpretation

is that the WTP derived in the experiment comes from *decision utility* functions that contain perceptual errors that lead people to undervalue the vaccine relative to the utility the person would actually experience from the reduction in risk created by the vaccine. In other words, the results suggest that there are large internalities associated with the demand for the Covid-19 vaccine.

Table 5 compares the way people make trade-offs between money and risks in the Delayed Second Dose experiment. Panel A presents the fraction of respondents who indicated they would delay the second dose for three months at each level of the randomly assigned transfer, based on fitted values from the linear regressions. Panel B shows the implied fatality risk from delaying the second dose caseloads during the March 1, 2021 through May 31, 2021 period and estimates of second dose efficacy (Haas et al., 2021; Dagan et al., 2021). These figures imply that delaying the second dose would increase fatality risk by 24.7%, which implies an additional 5.42 additional deaths per $100,000$ for the overall sample. The expected fatality increases range from 0.38 per $100,000$ for the 18-39 age group to 17.86 per $100,000$ for people over age 65.

Panel C shows the VSL estimates implied by each level of compensation for the delay. The results suggest people use a higher VSL when considering whether to delay the second dose than when thinking about whether to pay for an earlier first dose. In the overall sample (18+), the VSL estimates remain well below the HHS standard of $11.4 million at each transfer level. But the results differ by age. Among people age 18-39, someone with WTA compensation in the $50 to $200 range is implicitly using a VSL in the range of $13 million to $53 million. These VSLs are larger than the HHS standard, which suggests people are actually over valuing the second dose relative to its effects on mortality risk. In contrast, older adults are too willing to accept compensation for the delay. People over age 65 who would delay the second dose for even $200 would be using an implicit VSL of about $1.1 million.

Table 5: Delayed Doses, Willingness to accept, Fatality Risk, and Implied Covid-19 VSL Estimates (Three Month Second Dose Delay Experiment)

	18+	18–39	40–64	65+
Panel A: Proportion Who Respond Yes				
$5 Experiment	0.24	0.30	0.22	0.15
$10 Experiment	0.24	0.30	0.23	0.15
$50 Experiment	0.28	0.33	0.27	0.16
$100 Experiment	0.32	0.36	0.33	0.18
$200 Experiment	0.40	0.43	0.44	0.21
Panel B: Covid-19 Fatality Risk				
Covid-19 fatalities (from 3/1/2021 - 5/31/2021)	56,638	1,499	15,872	39,267
Population (in millions)	257.94	98.38	105.26	54.30
Covid-19 Fatality Risk per 100,000	21.96	1.52	15.08	72.31
24.7% Fatality Risk Increase (per 100,000)	5.42	0.38	3.72	17.86
Panel C: Implied VSL Estimates (in 2020 millions of dollars)				
$5 Experiment Offer	$0.09	$1.33	$0.13	$0.03
$10 Experiment Offer	$0.18	$2.66	$0.27	$0.06
$50 Experiment Offer	$0.92	$13.29	$1.34	$0.28
$100 Experiment Offer	$1.84	$26.57	$2.68	$0.56
$200 Experiment Offer	$3.69	$53.14	$5.37	$1.12
Panel D: Expected Median WTA Estimates				
HHS Age-Invariant VSL (in 2020 millions of dollars)	$11.41	$11.41	$11.41	$11.41
Expected Median WTA Estimate (using invariant VSL)	$619	$43	$425	$2,037
Age-Adjusted VSL (in 2020 millions of dollars)	$11.41	$12.62	$9.86	$3.99
Expected Median WTA Estimate (using age-adjusted VSL)	$619	$47	$367	$713

Notes: Proportions in Panel A are based on weighted averages of willingness to accept payment for the delay of the second dose for each transfer offering and age group. The population data in Panel B are available at: https://www.census.gov/data/tables/2020/demo/popest/2020-demographic-analysis-tables.html. The fatality data in Panel B are available at: https://data.cdc.gov/NCHS/Provisional-COVID-19-Deaths-by-Sex-and-Age/9bhg-hcku/data. The Implied VSL Estimates in Panel C assume a 24.7% fatality risk increase (Haas et al. 2021; Dagan et al. 2021) due to delaying the second dose by three months. The Expected Median WTA in Panel D is based off HHS estimates of Age-Invariant and Age-Adjusted VSL.

Panel D shows estimates of the WTA compensation for delayed second doses that would prevail if people used a standard VSL. At the HHS age-invariant VSL, adults (18+) would be willing to delay the second dose for $619. Panel A shows that about 40% of people would be willing to delay the second dose for less than $200. If we treat $200 as the (not quite) median WTA compensation for delaying, then the experiments suggest that the typical person values the second dose at about $\frac{\$200}{\$619} \times 100 \approx 32\%$ of its full value. However, the VSL benchmark of WTA compensation for a delayed second dose differs across age groups. People ages 18-39 would be willing to delay the second dose in return for only $43 if they value fatality risks using the HHS VSL. Treating $200 as the approximate median WTA among 18-39 year olds suggests that they value the second dose at $\frac{\$200}{\$43} \approx 4.6$ times its true value. In contrast, adults over age 65 who use the HHS VSL would need a transfer of $\$2,037$ in order to delay the second dose willingly.[14] Using the age-adjusted VSL figures partially reconciles the gap. If older adults use an age-adjusted VSL of $3.9 million, they would be willing to delay the second dose in return for a $713 transfer.

The results from the delayed second dose experiment suggest that people undervalue the second dose of the vaccine: given the VSL benchmarks, most people in our sample should require a bigger transfer in order to delay the second dose than their behavior in the experiment would indicate. However, the discrepancy between the *VSL Benchmark* valuation and the experimental results is smaller for the second dose than the first dose. This may indicate that some form of endowment effect takes hold once people begin a two-dose regimen. One implication is that inducing people to obtain their first shot is apt to be more difficult than motivating them to obtain their second dose. Tables A.1 and A.2 in the Appendix present the same exercise with unweighted predicted responses in Panel A. The results are not very sensitive to weighting.

We also examined models that allow the responses to the randomly assigned price and transfer variables to differ by age, self-reported vaccine hesitancy, and the local severity of the Covid-19 epidemic at the time of the survey. Table 6 shows estimates where the outcome is a binary variable indicating that the person would opt for early vaccination. The first column is a weighted linear regression only including price, which matches the pink lines in Figure 3. In the second column, we include dummies for each price offering (leaving out $50 as the reference category), which matches the blue points in Figure 3. The results in both of the first two columns indicate take-up is falling in price. The results from the third column

[14]The experiment does not provide a non-parametric approximation of the median WTA: only 20% of people over 65 have WTA compensation less than $200, and the extrapolation needed to compute the median would move well outside the range of the data. Thus, we do not compute a measure of undervaluation or overvaluation of the second dose for people over age 65.

of Table 6 show that interest in early vaccination take-up is falling in the price offering for both younger and older participants. The arc price elasticity of demand for early vaccination was about -.81 for people under 65 and -2.06 for people over age 65, suggesting that demand for early vaccination was more sensitive to price among older people.[15] The fourth column allows for heterogeneity by vaccine hesitancy. The non-hesitant group has a higher baseline demand, and their demand is falling in price. In contrast, demand for early vaccination among people who are vaccine hesitant is lower and almost flat with respect to price. The price elasticity of demand is -1.03 among the non-hesitant and .18 (weakly positive but not statistically different from zero) among the hesitant. One interpretation of these results is that it may be difficult to motivate a vaccine hesitant population using price instruments. The fifth column explores the possibility that demand for early vaccination depends on the prevailing prevalence of Covid-19. We allow the effects of the randomly assigned prices to vary with the log of the Covid-19 deaths in a person's zip code. The results provide no significant evidence that people living in areas with more severe epidemics were more price responsive.

Table 7 reports results for models where the outcome indicates that the person was willing to delay the second shot. The third column shows estimates of price responsiveness among older and younger people. The fraction willing to delay the second dose rises with the value of the transfer, and the coefficient on the interaction term (Transfer Offer $\times$ 65+) is small and not statistically different from zero. The fourth column shows estimates from models that allow the effects of the transfer to differ by self-reported vaccine hesitancy. Among non-hesitant respondents, the fraction who would delay the second dose rises with the transfer offer and the arc elasticity is .35. But the interaction term (transfer $\times$ Hesitant) is negative and statistically significant. The fraction that agrees to delay the second dose is actually falling in the transfer offer for the hesitant sample, and the arc elasticity is -.36. This counter-intuitive result suggests that the hesitant population may be difficult to motivate using price incentives. The fifth column examines how the effects of transfer offerings on openness to a delayed second dose depends on prevailing epidemiological conditions. The results suggest that transfers are more effective at inducing people to delay their second dose when local epidemiological conditions are more severe: the coefficient on the transfer (main effect) is positive and significant, and the coefficient on the interaction term (Transfer $\times$ $ln(DeathsPer1000)$) is also positive. The arc elasticity for people living in areas at the 20th

[15] We compute the arc elasticity as $\epsilon = (\frac{\hat{Y_{max}} - \hat{Y_{min}}}{\hat{Y_{mid}}}) \times (\frac{p_{max} - p_{min}}{p_{mid}})^{-1}$ where p_{max}, p_{min}, and p_{mid} are the maximum, minimum, and midpoint price or transfer offers, $\hat{Y}_p$ is the average predicted value when the price is set to p, and all other covariates are equal to their means.

percentile of the distribution of deaths per 1000 is .18. In contrast, among people living in areas at the 80th percentile of the distribution of deaths, the arc elasticity is .36. Demand for on-time second doses is more price elastic when the local epidemic is severe than when the local epidemic is mild. Tables A.3 and A.4 replicate these results with demographic covariates and show similar results.

7 Discussion

This paper uses discrete choice survey experiments in conjunction with VSL benchmarks to assess the degree to which people may undervalue the Covid-19 vaccines. The qualitative pattern of the experimental results is logical. Demand for early vaccination is falling in the price, while the fraction of people who are willing to delay the second dose rises with the level of compensation. However, the experiments suggest that people value first and second doses much less than would be expected given external estimates of the value of a statistical life. A person making choices using a "typical" VSL would be willing to pay more than $\$2,700$ to be vaccinated four months early. The experiment implies median willingness to pay for early vaccination is less than \$50. In other words, the median person values the vaccine at less than 2% of its "true" value. The level of demand for the vaccine exhibited in the experiment suggests people are using a VSL of around $\$210,000$, well below the VSLs found in the economics literature, which are typically closer to \$11 million. The behavioral public finance literature offers one interpretation of our results: people may exhibit "internalities" when making choices about Covid-19 vaccination. Internalities that lead people to undervalue the private benefits of the vaccine offer an explanation for low vaccine take-up that is distinct from arguments rooted in positive externalities, although it is possible for both internalities and externalities to occur at the same time.

Table 6: WTP Regression Results

	(1)	(2)	(3)	(4)	(5)
Price Offer	–0.0004***		–0.0003***	–0.0004***	–0.0004***
	(0.0001)		(0.0001)	(0.0001)	(0.0001)
1(price=100)		0.0054			
		(0.0669)			
1(price=200)		–0.1461**			
		(0.0618)			
1(price=500)		–0.3104***			
		(0.0529)			
1(price=1000)		–0.3338***			
		(0.0597)			
65+			0.2068***		
			(0.0461)		
Price Offer*65+			–0.0002**		
			(0.0001)		
Hesitant				–0.4749***	
				(0.0483)	
Price Offer*Hesitant				0.0006***	
				(0.0001)	
ln(Deaths per 1000)					–0.0263
					(0.0540)
Price Offer*ln(Deaths per 1000)					0.0000
					(0.0001)
Constant	0.4429***	0.4584***	0.4069***	0.4536***	0.4872***
	(0.0292)	(0.0473)	(0.0346)	(0.0411)	(0.0333)
Mean of Dependent Var	0.3552	0.3552	0.3552	0.2671	0.3586
R-squared	0.0818	0.1029	0.0983	0.1676	0.1070
N	1,672	1,672	1,672	941	1,550

Notes: The dependent variable is equal to 1 if the respondent is willing to pay for the early vaccination offering and zero otherwise. The symbols *** indicate statistically significant at the one percent level, ** at the five percent level, and * at the ten percent level. Column 1 presents the simple weighted regression of take-up of early vaccination on price. Column 2 presents a non-parametric version where we include dummy variables for each price offering ($50 is the reference group). Columns 3-5 include interactions of age, vaccine hesitancy, and logged deaths in the county, respectively.

Table 7: WTA Regression Results

	(1)	(2)	(3)	(4)	(5)
Transfer Offer	0.0008***		0.0009***	0.0013***	0.0007**
	(0.0003)		(0.0003)	(0.0004)	(0.0003)
1(transfer=10)		0.1067*			
		(0.0554)			
1(transfer=50)		0.1298**			
		(0.0524)			
1(transfer=100)		0.2427***			
		(0.0554)			
1(transfer=200)		0.2027***			
		(0.0558)			
65+			–0.1087***		
			(0.0379)		
Transfer Offer*65+			–0.0006		
			(0.0004)		
Hesitant				0.0439	
				(0.0780)	
Transfer Offer*Hesitant				–0.0019**	
				(0.0008)	
ln(Deaths per 1000)					–0.0347
					(0.0486)
Transfer Offer*ln(Deaths per 1000)					0.0009**
					(0.0005)
Constant	0.2348***	0.1604***	0.2569***	0.2474***	0.2436***
	(0.0261)	(0.0322)	(0.0317)	(0.0392)	(0.0292)
Mean of Dependent Var	0.2517	0.2517	0.2517	0.2969	0.2532
R-squared	0.0182	0.0325	0.0350	0.0422	0.0344
N	1,667	1,667	1,667	937	1,545

Notes: The dependent variable is equal to 1 if the respondent is willing to accept payment for the delay of the second dose and zero otherwise. The symbols *** indicate statistically significant at the one percent level, ** at the five percent level, and * at the ten percent level. Column 1 presents the simple weighted regression of willingness to delay the second dose on transfer. Column 2 presents a non-parametric version where we include dummy variables for each transfer offering ($5 is the reference group). Columns 3-5 include interactions of age, vaccine hesitancy, and logged deaths in the county, respectively.

Our study has important limitations. We interpret our results through the lens of behavioral public finance theories that distinguish between decision utility and experienced utility. We operationalize the distinction by using the data from the discrete choice experiments to measure WTP, WTA, and VSL parameters that are derived from people's decision utility functions. We try to approximate the same parameters from people's experienced utility functions using benchmark VSL calculations from the literature. Although we think this framework is natural and plausible, we cannot guarantee that these two analytic approaches map directly to the underlying theoretical constructs. One obvious concern is that our evidence on parameters from the "decision utility" function comes from survey experiments rather than real world decisions with actual consequences. Stated preference methods like the ones we use are always controversial. Hausman (2012) summarizes some of the strongest criticisms of the method. One of the biggest worries is that stated preference approaches often generate "hypothetical response bias", which usually induce people to overstate how much they value a given choice option. For instance, some analysts argue that the upward bias is severe and that stated preference valuation estimates should be divided by 2 or 3 (Hausman, 2012). It is worth noting that our study does not suggest people overvalue the vaccine: the main conclusion is they undervalue the vaccine substantially.

Another limitation of our study is that it says little about the source of the internalities that seem to drive undervaluation. We are not able to explore or disentangle behavioral biases related to false beliefs, present bias, or salience bias. In addition, the implied VSL and WTP estimates calculated in this study all assume the WTP values are for personal mortality risk reduction, which may not be entirely accurate. It is possible that non-fatal valuations may influence behavior as well (Kniesner and Sullivan, 2020; Robinson et al., 2021). Other factors such as dread and uncertainty over the negative effects from the vaccine, inaccurate risk perceptions, anchoring bias, and reservations about jumping the queue may also play a role in the calculations.

The results of the study suggest people value first and second doses quite differently and in ways that indicate an internality may be at work. Specifically, we found that people tended to use a lower (implicit) VSL when deciding whether to pay to be vaccinated early than when deciding whether to accept compensation for delaying the second dose. This is concerning since the first dose likely produces a larger effect on mortality risk. The endowment effect is one possible explanation for this result. Whatever the reason, our results suggest the biggest hurdle for getting the public vaccinated is "getting them in the door" for their first shot.

Broadly speaking, the results suggest getting a super-majority of the public to become vaccinated may prove to be extremely difficult. Other research has found small vaccinations

incentives ($10 or $50) do not appear to work very well with large portions of the population (Chang et al., 2021). Our results are in line with what these other studies have found. If policy makers choose to offer financial incentives to vaccinate the unvaccinated, then the dollar values offered will likely have to be much higher than those offered in previous studies and particularly high for the extremely vaccine hesitant population. A large share of the participants in our study appears to have negative WTP for the vaccine. Using regressions to extrapolate outside the range of our data suggests that about 20% of the sample would need to be paid more than $950 to be vaccinated early.

As Covid-19 transitions from the pandemic to an endemic disease, it seems likely that annual Covid-19 vaccination will become an important public health goal. Our research suggests that – at least at present – internalities may be an important barrier to high levels of regular vaccination. Further research on public policy responses to vaccine internalities may help improve social outcomes related to many transmittable diseases going forward.

A Appendix

Table A.1: Vaccinations, Willingness to pay, Fatality Risk, and Implied COVID-19 VSL Estimates (Four Month Early Vaccination Experiment)

	18+	18–39	40–64	65+
Panel A: Proportion Who Respond Yes				
$50 Experiment	0.51	0.35	0.47	0.59
$100 Experiment	0.48	0.33	0.44	0.57
$200 Experiment	0.44	0.30	0.40	0.52
$500 Experiment	0.29	0.22	0.26	0.36
$1000 Experiment	0.06	0.08	0.02	0.10
Panel B: Covid-19 Fatality Risk				
Covid-19 fatalities (from 3/1/2021 - 6/30/2021)	64,577	1,767	18,518	44,292
Population (in millions)	257.94	98.38	105.26	54.30
Covid-19 Fatality Risk (per 100,000)	25.04	1.80	17.59	81.56
96.7% Fatality Risk Reduction (per 100,000)	24.21	1.74	17.01	78.87
Panel C: Implied VSL Estimates (in 2020 millions of dollars)				
$50 Experiment Offer	$0.21	$2.88	$0.29	$0.06
$100 Experiment Offer	$0.41	$5.76	$0.59	$0.13
$200 Experiment Offer	$0.83	$11.52	$1.18	$0.25
$500 Experiment Offer	$2.07	$28.79	$2.94	$0.63
$1000 Experiment Offer	$4.13	$57.58	$5.88	$1.27
Panel D: Expected Median WTP Estimates using HHS Values				
HHS Age-Invariant VSL (in 2020 millions of dollars)	$11.41	$11.41	$11.41	$11.41
Expected Median WTP Estimate (using invariant VSL)	$2,761	$198	$1,940	$8,996
HHS Age-Adjusted VSL (in 2020 millions of dollars)	$11.41	$12.62	$9.86	$3.99
Expected Median WTP Estimate (using age-adjusted VSL)	$2,761	$219	$1,678	$3,148

Notes: Proportions in Panel A are based on unweighted averages of take-up of early vaccination for each price offering and age group. The population data in Panel B are available at: https://www.census.gov/data/tables/2020/demo/popest/2020-demographic-analysis-tables.html. The fatality data in Panel B are available at: https://data.cdc.gov/NCHS/Provisional-COVID-19-Deaths-by-Sex-and-Age/9bhg-hcku/data. The Implied VSL Estimates in Panel C assume a 96.7% fatality risk reduction (Haas et al. 2021) due to early vaccination by four months. The Expected Median WTP in Panel D is based off HHS estimates of Age-Invariant and Age-Adjusted VSL.

Table A.2: Delayed Doses, Willingness to accept, Fatality Risk, and Implied COVID-19 VSL Estimates (Three Month Second Dose Delay Experiment)

	18+	18–39	40–64	65+
Panel A: Proportion Who Respond Yes				
$5 Experiment	0.20	0.33	0.22	0.15
$10 Experiment	0.20	0.33	0.22	0.15
$50 Experiment	0.23	0.34	0.27	0.17
$100 Experiment	0.27	0.36	0.32	0.19
$200 Experiment	0.35	0.38	0.43	0.23
Panel B: Covid-19 Fatality Risk				
Covid-19 fatalities (from 3/1/2021 - 5/31/2021)	56,638	1,499	15,872	39,267
Population (in millions)	257.94	98.38	105.26	54.30
Covid-19 Fatality Risk per 100,000	21.96	1.52	15.08	72.31
24.7% Fatality Risk Increase (per 100,000)	5.42	0.38	3.72	17.86
Panel C: Implied VSL Estimates (in 2020 millions of dollars)				
$5 Experiment Offer	$0.09	$1.33	$0.13	$0.03
$10 Experiment Offer	$0.18	$2.66	$0.27	$0.06
$50 Experiment Offer	$0.92	$13.29	$1.34	$0.28
$100 Experiment Offer	$1.84	$26.57	$2.68	$0.56
$200 Experiment Offer	$3.69	$53.14	$5.37	$1.12
Panel D: Expected Median WTA Estimates using HHS Values				
HHS Age-Invariant VSL (in 2020 millions of dollars)	$11.41	$11.41	$11.41	$11.41
Expected Median WTA Estimate (using invariant VSL)	$619	$43	$425	$2,037
HHS Age-Adjusted VSL (in 2020 millions of dollars)	$11.41	$12.62	$9.86	$3.99
Expected Median WTA Estimate (using age-adjusted VSL)	$619	$47	$367	$713

Notes: Proportions in Panel A are based on unweighted averages of willingness to accept payment for the delay of the second dose for each transfer offering and age group. The population data in Panel B are available at: https://www.census.gov/data/tables/2020/demo/popest/2020-demographic-analysis-tables.html. The fatality data in Panel B are available at: https://data.cdc.gov/NCHS/Provisional-COVID-19-Deaths-by-Sex-and-Age/9bhg-hcku/data. The Implied VSL Estimates in Panel C assume a 24.7% fatality risk increase (Haas et al. 2021; Dagan et al. 2021) due to delaying the second dose by three months. The Expected Median WTA in Panel D is based off HHS estimates of Age-Invariant and Age-Adjusted VSL.

Table A.3: Full WTP Regression Results

	(1)	(2)	(3)	(4)	(5)
Price Offer	–0.0004***		–0.0004***	–0.0004***	–0.0004***
	(0.0001)		(0.0001)	(0.0001)	(0.0001)
1(price=100)		–0.0007			
		(0.0654)			
1(price=200)		–0.1689***			
		(0.0600)			
1(price=500)		–0.3204***			
		(0.0492)			
1(price=1000)		–0.3482***			
		(0.0593)			
65+	0.1349***	0.1425***	0.1995***	0.0213	0.0992**
	(0.0421)	(0.0414)	(0.0544)	(0.0441)	(0.0473)
Price Offer*65+			–0.0002**		
			(0.0001)		
Hesitant				–0.4655***	
				(0.0509)	
Price Offer*Hesitant				0.0006***	
				(0.0001)	
ln(Deaths per 1000)					–0.0220
					(0.0522)
Price Offer*ln(Deaths per 1000)					0.0000
					(0.0001)
40–64	0.0300	0.0417	0.0293	0.0353	–0.0026
	(0.0409)	(0.0399)	(0.0408)	(0.0394)	(0.0465)
Female	–0.0391	–0.0425	–0.0392	–0.0639	–0.0116
	(0.0358)	(0.0354)	(0.0357)	(0.0424)	(0.0352)
Black	–0.0813	–0.0784	–0.0825	–0.0608	–0.1337*
	(0.0670)	(0.0663)	(0.0659)	(0.0705)	(0.0696)
Hispanic	–0.0495	–0.0473	–0.0479	–0.1145**	–0.0687
	(0.0557)	(0.0544)	(0.0557)	(0.0549)	(0.0588)
Other Race	–0.0545	–0.0636	–0.0543	0.0012	–0.0077
	(0.0675)	(0.0668)	(0.0673)	(0.0697)	(0.0781)
Constant	0.4576***	0.4763***	0.4463***	0.4994***	0.5038***
	(0.0458)	(0.0586)	(0.0478)	(0.0560)	(0.0515)
Mean of Dependent Var	0.3552	0.3552	0.3552	0.2671	0.3586
R-squared	0.1035	0.1265	0.1061	0.1868	0.1280
N	1,672	1,672	1,672	941	1,550

Notes: The dependent variable is equal to 1 if the respondent is willing to pay for the early vaccination offering and zero otherwise. The symbols *** indicate statistically significant at the one percent level, ** at the five percent level, and * at the ten percent level. Column 1 presents the simple weighted regression of take-up of early vaccination on price. Column 2 presents a non-parametric version where we include dummy variables for each price offering ($50 is the reference group). Columns 3-5 include interactions of age, vaccine hesitancy, and logged deaths in the county, respectively. This table presents results similar to Table 6 but for regressions which include covariates.

Table A.4: Full WTA Regression Results

	(1)	(2)	(3)	(4)	(5)
Transfer Offer	0.0008***		0.0009***	0.0012***	0.0006**
	(0.0003)		(0.0003)	(0.0004)	(0.0003)
1(transfer=10)		0.1026*			
		(0.0547)			
1(transfer=50)		0.1364***			
		(0.0528)			
1(transfer=100)		0.2403***			
		(0.0552)			
1(transfer=200)		0.1905***			
		(0.0545)			
65+	–0.1914***	–0.1968***	–0.1532***	–0.1602***	–0.2124***
	(0.0438)	(0.0436)	(0.0511)	(0.0538)	(0.0495)
Transfer Offer*65+			–0.0005		
			(0.0004)		
Hesitant				0.0286	
				(0.0771)	
Transfer Offer*Hesitant				–0.0018**	
				(0.0008)	
ln(Deaths per 1000)					–0.0315
					(0.0487)
Transfer Offer*ln(Deaths per 1000)					0.0009*
					(0.0005)
40–64	–0.0602	–0.0676	–0.0597	–0.0593	–0.0845*
	(0.0446)	(0.0444)	(0.0446)	(0.0491)	(0.0508)
Female	0.0096	0.0053	0.0086	–0.0341	0.0351
	(0.0371)	(0.0368)	(0.0371)	(0.0464)	(0.0371)
Black	0.0463	0.0460	0.0465	0.0796	0.0293
	(0.0713)	(0.0700)	(0.0711)	(0.0913)	(0.0749)
Hispanic	–0.0793	–0.0745	–0.0789	–0.1693***	–0.0763
	(0.0595)	(0.0586)	(0.0595)	(0.0623)	(0.0638)
Other Race	–0.0885	–0.0902	–0.0874	–0.1075	–0.0859
	(0.0649)	(0.0649)	(0.0650)	(0.0771)	(0.0739)
Constant	0.3107***	0.2401***	0.3041***	0.3439***	0.3231***
	(0.0498)	(0.0537)	(0.0513)	(0.0622)	(0.0544)
Mean of Dependent Var	0.2517	0.2517	0.2517	0.2969	0.2532
R-squared	0.0449	0.0598	0.0460	0.0796	0.0677
N	1,667	1,667	1,667	937	1,545

Notes: The dependent variable is equal to 1 if the respondent is willing to accept payment for the delay of the second dose and zero otherwise. The symbols *** indicate statistically significant at the one percent level, ** at the five percent level, and * at the ten percent level. Column 1 presents the simple weighted regression of willingness to delay the second dose on transfer. Column 2 presents a non-parametric version where we include dummy variables for each transfer offering ($5 is the reference group). Columns 3-5 include interactions of age, vaccine hesitancy, and logged deaths in the county, respectively. This table presents results similar to Table 7 but for regressions which include covariates.

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