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Short on Shots: Are Calls on Cooperative Restraint Effective in Managing the Scarcity of Flu Vaccines?

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Salience

"We knew that once people heard there was a shortage, more people would try to get the vaccine." *San Francisco Chronicle*, October 11, 2004

Cooperative restraint

"There is a strong spirit of cooperation during this crisis. We have no intention of taking any draconian steps to enforce this state of emergency." *San Francisco Chronicle*, October 9, 2004

Cheating

"Flu shots, often a test of bravery, became a test of character ..., and not everyone was passing." *San Francisco Chronicle*, October 7, 2004

I. INTRODUCTION

While history is replete with situations where societies have been confronted with unexpected commodity shortages, the way shortages have been managed has been quite varied. When a market exists, rising prices serve as the main rationing device, with targeted subsidies eventually used to ease the burden of adjustment on designated groups considered at risk. When the price is fixed, allocation of the scarce commodity across wanting individuals has to be done by introducing rules to distribute the commodity to those presumed most in need. These rules can be implemented by screening and/or by calls on voluntary restraint that we refer to as "cooperative" restraint to underline the cooperative nature of this behavior. However, information about the shortage, including justifying the call on cooperative restraint, also induces an increase in demand associated with greater salience of the commodity and increased eagerness in acquiring it. Cooperative restraint by some in refraining from acquiring the commodity can thus be countervailed by increases in demand by others.

Given these contradictory behavioral responses, the net effect may lead to an aggregate decline in demand and good targeting or to an increase in demand and/or poor targeting. While, in the long run, initiatives can be taken to respond to the shortage by increasing supply, understanding what motivates the short-run demand responses to the shortage is important to help better manage scarcity in a non-market setting. In particular, policy makers would like to know how effective can broad-scale calls on cooperative restraint be in managing the shortage of a vital good since this is likely to be a less politically costly approach than coercive screening. This is the first objective that we address in this paper. The second objective is to use the observed behavioral responses to identify which types of individuals responded to information about scarcity and to calls on cooperative restraint.

To fulfill these objectives, we set up a randomized experiment in the context of the large unexpected flu vaccine shortage that occurred in the Fall of 2004. Because the approach followed by health authorities was to manage the shortage by a call on voluntary restraints, we designed the experiment to measure how far calls on cooperation can go in managing scarcity. The experiment took place at a flu clinic held at a California university campus medical center. Prior to the clinic, we subjected the campus population to two randomized experimental treatments: in treatment one (T_1), a group of departments received an email informing about the reduced number of vaccination clinics (scarcity) and their corresponding schedule (deadlines); in treatment two (T_2), another group of departments received an email with the same information as T_1, but additionally appealing (as the Center for Disease Control was recommending at the time) for non-members of defined priority groups to refrain from seeking vaccination (cooperative restraint).[1] The rest of the campus population did not receive an email from us and served as a control group C. Two weeks after this clinic, the medical center sent an email to the campus population announcing a last clinic.

This randomized design, and the surveys done at the two clinics, allow us to analyze both the demand for vaccination and the actual distribution of vaccines. Demand was measured by the population that came to the clinic seeking vaccination. Actual distribution was to those who did not walk away when informed of screening and who were not rejected by the clinic superficial screening. To analyze demand, we decompose quantitatively the different behavioral responses at play: the response to information about scarcity and deadlines is measured by the difference in behavior between T_1 and C; the response to calls on cooperative restraint, conditional on information about scarcity and deadlines, by the difference in behavior between T_2 and T_1; and the net effect of these two types of responses by the difference in behavior between T_2 and C. The relative contribution of subgroups in the campus population to each type of response can also be identified.

Results show a very large effect of information on scarcity and deadlines in increasing demand, particularly among non-priority people, which was only partially counteracted by cooperative restraint. Priority groups, by contrast, responded equally strongly to scarcity as to calls on cooperative restraint, resulting in a wash on demand, even though the calls on

[11] These priority groups, defined by the Center for Disease Control, are described in section 3.1.

cooperative restraint were not directed at them. To analyze the actual distribution of vaccines, we decompose the roles of information, cooperative restraint, and screening. An analysis of confidential self-declared membership in priority groups and of unusual patterns of declared membership in priority groups provides evidence on the extent of cheating among candidates for a flu vaccine. In the end, the strategy failed to reserve the scarce resources to the targeted population. The number of vaccinations distributed increased by 17% and all the addition went to non-priority people.

II. BEHAVIORAL RESPONSES TO A SHORTAGE: LESSONS FROM THE LITERATURE

It is well recognized that perceptions of scarcity can induce a sharp increase in demand due to rising salience of the scarce good, worsening whatever true shortage there might be. Some of the great famines in history like those in Bengal in 1943, Ethiopia in 1973, and Bangladesh in 1974 in fact occurred without any disruption in supply (Sen, 1981). The "Great Toilet Paper Shortage" caused in zest by Johnny Carson in 1973 also occurred without any change in supply.[2] In other cases, the scarcity effects of shortfalls in supply were greatly amplified by induced consumer buying. In a market setting, given a contraction in supply, if demand expands in response to the shortage, then the price increase is greater than the one caused solely by the leftward supply shift. With fixed prices, the "panic buying" effect induced by a fall in supply is amplified by lack of price response, requiring some type of rationing device. Examples are the oil "buyer panics" of 1971 and 1973 that resulted in long lines at the gas pumps as government froze prices, with time waiting in line becoming the rationing device (Adelman, 2004). That scarcity enhances desirability has long been recognized in the marketing literature (Folger, 1992; Lynn, 1992a and 1992b). The 2004 flu vaccine shortage analyzed here was similarly managed under price control.[3] A rise in demand was fully expected to happen as a response to the shortage, and rules were introduced to direct scarce supplies toward priority groups.[4] Because the commodity is of vital importance for people at risk, information about scarcity to justify a call on

[2] In his Late Night Show monologue, Johnny Carson said: "You know what's disappearing from the supermarket shelves? Toilet paper. There's an acute shortage of toilet paper in the United States." The consequence of this statement made in the early 1970's, a time of shortages -- oil in particular --, was that the next morning many of the 20 million television viewers ran to the supermarket and bought all the toilet paper they could find. By noon, most of the stores were out of stock since, despite trying to ration it, they couldn't keep up with demand.

[3] The few cases of price gouging resulted in legal charges.

[4] As a county Public Health Department spokesperson said: "We knew that once people heard there was a shortage, more people would try to get the vaccine." *San Francisco Chronicle*, October 11, 2004.

cooperative restraint among people not in priority groups also creates greater salience of vaccination among non-priority individuals, resulting in an obvious dilemma for the management of scarcity without strict screening.

Another response that can increase demand as a consequence of a shortage is that the strict deadlines associated with rationing in the distribution of a scarce good that will eventually run out may reduce the occurrence of normal-time procrastination. Procrastinators are individuals who delay tasks until a later period, and who, when the later period arrives, delay those tasks again and again if there are no strict deadlines for getting things done (Akerlof, 1991). Sirois et al. (2003) found empirical evidence that procrastination also applies to decisions related to individuals' own health. Procrastination can be overcome by the introduction of strict deadlines. This is consistent with studies that find, for example, that if manufacturers place a deadline on redemption of the coupons they distribute, the probability of redemption increases (Silk, 2004); and that the shorter the time students are given to complete a task, the lesser the likelihood that they will fail to complete it (Tversky and Shafir, 1992). If procrastinators postpone getting a flu shot in normal times when there are no deadlines, even among individuals in priority groups, strict deadlines introduced by the rationing scheme may induce many of them to overcome delaying and seek vaccination, adding to the rise in demand induced by the shortage.

Voluntary restraints in response to a call on cooperation can be expected to hold when there is clear information about expected benefits, effective monitoring and enforcement, and repeated interactions. For this reason, this is more likely to occur in small groups with long time horizons (Olson, 1965). In this perspective, responses to broad-scale demands for voluntary restraints in the face of scarcity can be expected to fail. Yet, there is also abundant evidence of willingness to cooperate in situations of relatively anonymous and sporadic relations. A number of recent behavioral experiments (e.g., Fehr and Gächter, 2000; Gintis et al., 2003) have found that individuals behave more cooperatively than the "self-interest individual model" would predict (Rabin, 1998). This applies, for instance, to tax payment where the observed rate of tax abidance cannot be explained by current levels of audit risks and penalties (Feld and Frey, 2002). "Tax morale" needs to be invoked to explain observed levels of compliance. Willingness to cooperate is possible even in large social groups as it can be motivated by the desire for social approval (Holländer, 1990), by conforming to social norms for fear that non-compliance by oneself will lead to their collapse (Azar, 2004), or by satisfaction in cooperating if it helps improve one's self-image (Trivers, 1971).

In calling on broad-scale cooperative restraint to manage a flu vaccine shortage, the expectation was that individuals not in priority groups would voluntarily incur the risk of being sick to allow the scarce resource to reach the people most in need. On a campus where many people know each other and identify with others, one could further expect restraint to be motivated by a sense of community and a concern with reputation.

III. EXPERIMENTAL DESIGN AND DATA COLLECTION

3.1. The flu vaccine shortage and the timeline of events

On Monday, October 4, 2004, the campus medical center in our experiment sent its routine annual reminder that everyone should receive a flu shot every year and informing of the schedule for the six planned vaccination clinics starting with October 6 and ending in December, 2004. On Tuesday, October 5, half of the U.S. supply of flu vaccine was pulled back from the market because of possible contamination.[5] Starting on Wednesday, October 6, numerous media articles about the flu vaccine shortage started to inform the American public. The United States Center for Disease Control (CDC) appealed to the public for people not in defined priority groups to voluntarily forego vaccination. On October 6, the campus medical center held the first of its six previously scheduled vaccination clinics. Two days later, on Friday October 8, it announced on its website a reduction to only two in the number of subsequent clinics, with occurrence of the originally announced other three subject to vaccine availability. On Saturday, October 9, some California counties declared an emergency to enforce a State directive restricting vaccination to priority groups. The county where the campus is located did not at that time officially announce enforcement of this directive.[6]

On Monday, October 11, one week after the shortage was first announced, the two experimental treatment emails (T_1 and T_2) were sent out to the campus population. Monday the 11th was a national holiday and on the next day, Tuesday October 12, the second clinic, henceforth referred to as clinic A, took place, offering flu shots to the campus population and the non-campus community, and soft-screening candidates. This screening measure was not previously announced by the medical center. Individuals had to sign an affidavit declaring that

[5] British regulators cut the U.S. vaccine supply in half by condemning 48 million doses at a Liverpool factory owned by Chiron Corporation, a U. S. company based in Emeryville, California, after bacterial contamination was found.

[6] "There is a strong spirit of cooperation during this crisis," said the corresponding County Public Health Officer. "We have no intention of taking any draconian steps to enforce this state of emergency." *San Francisco Chronicle*, October 9, 2004.

they belonged to one of the priority groups, but with no proof asked as health-center personnel were more concerned with servicing than with policing. Individuals did not even have to specify which priority group they belonged to. These groups were: children 6-23 months of age, adults 65 years of age and older, women expecting to be pregnant during the flu season, health care workers with direct patient care, out-of-home care givers, individuals with household contacts of children less than 6 months old, adolescents on chronic aspirin therapy, and persons ages 2 through 64 with a chronic medical condition (such as asthma, diabetes, heart disease, chronic kidney disease, or who had chemotherapy or immune-compromised conditions). These groups had always been designed as priority, even in previous years when vaccination was recommended to all. We conducted our first survey during clinic A.

On Wednesday the 13th, the campus medical center cancelled all remaining clinics and recommended the population to check for updates. The update came two weeks later. On Wednesday, October 27, the medical center sent a campus-wide email informing about the date for a final clinic and announcing that, given the shortage, all candidates for flu vaccination would be asked to sign an affidavit that they belong to one of the priority groups. By the time of this last clinic, which we henceforth call clinic B, screening of participants was common practice across the U.S. and, most likely, the information sent via email to the campus population was by then also known to the non-campus community. Signature of an affidavit was required from all candidates, certifying membership in one of the priority groups. However, no hard proof of qualification into one of these groups was requested by the screening personnel. On Monday, November 1, we conducted our second and last survey during clinic B.

3.2. The experiment at clinic A

We randomly selected departments to receive two different kinds of email treatments. Members of the first subset of departments (T_1) received an email informing that only two clinics would be offered and giving the dates for these clinics. Members of the second subset (T_2) received an email containing the same information as sent to T_1 plus, in accordance with CDC recommendations at that time, a call on cooperative restraint in seeking vaccination for people not in priority groups.[7] The priority groups were described in detail in the T_2 email. The remaining departments (the control group *C*) received no email.

[7] Cooperative restraint is here defined here as "being informed of the reduced number of clinics, and not coming to a clinic in response to the call for the population not in priority groups to defer vaccination".

The experimental design was thus intended to allow identification of the following behavioral responses:

- From comparison of the T_1 and C groups, the impact on vaccine demand and distribution of information about scarcity and deadlines.
- From comparison of the T_2 and T_1 groups, the impact on demand and distribution of sending a call on cooperative restraint, conditional on information about scarcity and deadlines.
- From comparison of the T_2 and C groups, the net impact on demand and distribution of sending information about scarcity and deadlines, and of calling on cooperative restraint.

Emails were sent to faculty, staff, and graduate students by the management services officers (MSO) of the different departments. Of the 69 departments on campus, 10 were drawn for each of the treatment groups. However, 3 that had been selected for the second treatment did not follow up immediately upon our email. Given the extremely tight schedule of the experiment, this prevented them from participating to the experiment, leaving therefore 10 departments for T_1, 7 for T_2, and the remaining 52 for C. The emails to undergraduate students were sent by the student affairs officers (SAO) for declared majors corresponding to the selected departments and by the dean of the college for undeclared students. With 3 selected departments not having undergraduates and 6 SAO not responding immediately, the experiment included 8 majors and the undeclared from one college for T_1 and 3 majors and the undeclared from one college for T_2, leaving the rest for C. The numbers of treated faculty, staff, and students in the T_1, T_2, and C groups are given in Table 1. Of the campus population of 38,604, 8,695 were in T_1, 12,233 in T_2, and 17,676 in C. We address below the validity of the randomization process and the issues raised by non-compliance.

As the opportunity of getting a vaccine was offered at the workplace, it is likely that social interactions among co-workers influenced individual decisions to go to the clinic. This can be due to the transmission of information received, to mutual influence in appreciating the value of getting or not getting vaccinated for the flu, or to the fact that people who work together may go together to the clinic, a fact that we observed at the clinics.[8] These social interactions take place regardless of any treatment effect, including in the control groups. They, however, also affect the treatment effect itself, in so far as the treatment of one person has spillovers on the other members of the social network. Our experiment is not set up to distinguish the direct

[8] The importance of social interactions among students in seeking vaccination is shown in Rao et al., 2007.

influence of the email treatment from the indirect influence that would occur within a department, as all members of the same professional category in a department received the same information.[9]

The validity of our analysis in measuring the effect of sending an email relies on the stability assumption, i.e., that there was no interference across treatment units. Although this is not a guarantee that social interactions did not affect the experiment, clinic A occurred the day after a national holiday, giving people limited time to interact across departments on the morning of October 12, the day of clinic A, after they potentially read their emails. By sending the emails through administrative channels, we also believe that it minimized the chances of social interactions across departments.[10]

3.3. Randomization issues

The randomization scheme was initially designed to get a balanced sample of departments by distance to the health center for the two treatments. Although the campus is dense, and no part of the campus is far from others, the campus is partitioned into 25 departments that are further away (more than 0.5 miles) and 44 that are closer to the center. We drew 4 departments for each of treatments 1 and 2 from those far away, and 6 from closer. This very simple randomization scheme, combined with the non-compliance of a number of departments, raises a number of issues.

First is the issue that departments have different configurations in terms of faculty, staff, and student composition (see Table 1). Because these sub-populations, which we call professional categories, are expected to have very different behaviors in terms of their demands for flu vaccines, we derive estimates of campus population statistics from weighted averages of statistics by professional category. Each treatment sample is thus considered a stratified sample of the campus population, with the professional categories as strata. And randomization checks are to be done within each professional category.

Second, with the random drawing of so few units, in addition to non-compliance by a number of departments, it is important to verify the validity of the randomization at the individual

[9] This is in contrast with the experimental design used in Duflo and Saez (2003) who subjected a random sample of members of a subset of departments in a campus population to treatments. Their objective was to assess the role of information and peer-effects on the decision to enroll in employer-sponsored Tax Deferred Account retirement plans. They found the interesting result that when treating only 50% of the department members, the indirect effect through department co-workers is almost as high as the direct effect of the treatment.

[10] There is little motivation to forward the email to people outside the department since each email recipient believes it is likely that members of other departments were also receiving such email directly from their official administrative units.

level. Individual characteristics that could affect behavior toward vaccination on which we have information are gender, age in 10 categories, race, occupation, and wage category, for faculty and staff; and gender for graduate and undergraduate students. Two department-level characteristics are distance to the health center and discipline. Results are reported in Table 2 for pair-wise tests of equality between T_1, T_2, and C. Looking at individual characteristics, the similitude between the three treatment groups is excellent for all four professional categories. Only two comparisons (faculty occupation in T_2 vs. T_1 and staff age in T_2 vs. C) fail the randomization test at the 10% significance level among 36 pairs, a failure rate of only 5.6%. In contrast, distance is really not balanced at all among students. But, as we will verify later, distance has no influence on demand behavior, which is not surprising given how compact the campus is. What is potentially more problematic is that we did not stratify the departments along disciplines, and the result is a very imbalanced distribution across disciplines in several comparisons (T_1 vs. C for faculty, T_2 vs. C for staff, T_2 vs. C or T_1 for students). But here again, we will show that discipline is not correlated with demand for vaccine. In section 4.2, we will verify that the average treatment effects are robust to including these different control variables in the estimated equations.

Finally, there is the question of non-compliance. Although the circumstances under which the experiment was done suggest that many completely innocuous reasons could have led to this non-response, we will compute both intention to treat effects (ITE) and average treatment effects (ATE). We verify that the non-compliers behaved just like the non-treated and ITE is simply a slightly watered down ATE by a factor equal to the share of compliers, as would be expected if non-compliance resulted from factors uncorrelated to the treatment assignment.

3.4. Data collection

For clinic A, no screening had been announced. Yet, the list of qualifying priority groups was posted at the entrance of the medical center, and some screening was performed by the registration personnel. Among candidates for flu vaccination, some walked away upon reading the list of priority groups, others were screened out by the center personnel.

The survey forms were filled out by basically everybody. This may be either because the survey looked official, or because the opportunity cost of completing the survey while waiting on line was very small. We also surveyed the people who came in and, upon seeing the poster and noticing the screening, decided on their own to forgo vaccination. Information collected includes age, gender, campus affiliation by department and professional category, whether individuals got a flu shot in each of the last three years, and the reasons for them to come which included membership to the different priority groups. 738 individuals filled questionnaires, with 427 from campus and 311 from the non-campus community.[11] Out of the 394 campus members with departmental information, 37% were from the treatment group T_1, 25% from the treatment group T_2, and 38% from the control group.

This data is then completed by administrative information on the campus population. Information that is available in both the survey and administrative records are gender for the students and gender and age category for staff and faculty. Hence one can reconstitute the demand behavior (whether came to clinic A or not) of all campus individuals, by professional category, department, gender and age. This is the core database for the measure of the average treatment effects.

Although no experiment took place on the day of clinic B, we administered the survey. The response rate was again almost perfect once we started handing out the survey forms.[12] 610 persons filled questionnaires, 385 from campus and 225 from the non-campus community.

IV. IMPACT OF TREATMENTS ON DEMAND FOR A FLU VACCINE

4.1. Average treatment effect on demand for a flu vaccine

We first compute the average treatment effect on demand for a flu vaccine for the campus population. Individual demand for vaccine is defined as coming to the clinic. This is done in a regression framework by estimating the demand from individual *i* of professional category *k* in department *d*:

$$D_{ikd} = \delta_0 + \delta_1 T_{kd}^1 + \delta_2 T_{kd}^2 + \varepsilon_{ikd}\ , \qquad (1)$$

where D_{ikd} is a binary variable indicating whether the person came to the clinic, T_{kd}^1 and T_{kd}^2 are the treatments 1 and 2 variables, and ε_{ikd} the unobserved individual heterogeneity effect, clustered at the department-professional category level. The parameter δ_0 gives the aggregate campus demand without treatment (*C*), and the parameters δ_1 and δ_2 the treatment T_1 and T_2 effects relative to the control. Of interest is also $\delta_2 - \delta_1$, which gives the effect of the call on

[11] This includes staff from the campus administrative units as they were not part of the experimental design.
[12] This time, the clinic started about one hour earlier than announced to accommodate the long lines, so our survey team missed the first hour of people who came to the clinic.

cooperative restraint, conditional on receiving the information on scarcity and deadlines given in both treatments.[13]

Controls are then added to check the robustness of these results:

$$D_{ikd} = \upsilon_k + \upsilon_{dist} + X_{ikd}\beta + \delta_1 T_{kd}^1 + \delta_2 T_{kd}^2 + \varepsilon_{ikd}\,, \tag{2}$$

where υ_k and υ_{dist} are professional categories and distance to the health center fixed effects, along which the randomization was stratified, and X_{ikd} includes discipline and the only two individual characteristics that we observed in the campus population at large, namely gender and age.[14]

Results are reported in Table 3. Estimated parameters have all been multiplied by 100, so that they can be read as percentages of the campus population. Column (1) shows that sending information about the reduced number of clinics (T_1) induced an increase of demand by 0.85 percentage points, representing a doubling (110% increase) of the demand for flu vaccine over the base value of 0.77 percentage points in the control. The effect of calling on cooperative restraint ($T_2 - T_1$) on behavior was to decrease demand by 0.61 percentage points, or by 37.5% of the demand under T_1. These two effects resulted for the whole campus population in a (non significant) 0.24 percentage points, or 31.2% net increase between C and T_2. These results are robust to adding controls (columns 2 and 3), which contribute to increasing the precision of the estimation without affecting the point estimates. In particular, adding the professional category variables importantly increases the precision of the estimated values, suggesting heterogeneous behavior across these groups. However, neither distance nor discipline is a significant factor (with *p*-values for the *F*-test on the corresponding dummy variables equal to 0.89 and 0.36, respectively). The fact that the results are remarkably stable to the addition of covariates confirms that the randomization was successful in making treatment orthogonal to the other factors influencing the demand for vaccine.

Although we argued that non-participation of some departments despite their assignation to treatment did not probably reflect any profound difference that could induce a selection bias in the estimation, we provide the Intention to Treat Effects (ITE) in columns (5) and (6) of Table 3.

[13] The treatment effects of *sending emails* can be considered as either interesting in themselves, to the extent that they measure the impact of a well-defined type of information campaign. They also provide measures of the intention to treat effects of the treatment defined as *being informed*. Their relationship to the effect of the information itself is mitigated by several issues that could make them either higher or lower than the pure effect of the information contained in the emails (Hirano, Imbens, Rubin, and Zhou, 2000).

[14] Since age is not available for students, faculty and staff less than 30 years old are regrouped in the same age category, and all students are assigned to that age category.

Recall that 3 departments assigned to T_2 did not forward the information to faculty, staff, and graduate students. For undergraduates, 6 majors assigned to T_1 and 1 major assigned to T_2 did not comply. These non-compliers represent small numbers, 1.6 and 10.9% of the intended T_1 and T_2 populations, respectively, and 14% of the control population. Comparison of columns (5) and (1), with no controls, shows ITE only slightly lower than ATE, by an order of magnitude corresponding to the non-compliers' share, and identical intercepts, which is to be expected if there is no selection in compliance. In column (4), we verify that the non-compliers behave no differently from the rest of the control group, dispelling the fear that they are a selected group.

Table 4 reports the estimated impacts for the professional categories separately. As expected, the demand for flu shot varies across these groups, which have a very different age structure. In the control group, the demand is highest among faculty, at 3.1%, followed by staff at 1.3%, and students, at 0.7% for graduates and 0.4% for undergraduates. Faculty are also those who respond most to receiving the information, but also to the call on cooperative restraint.

4.2. Heterogeneity of impact on priority and non-priority groups

We contrast the behavioral responses of members and non-members of priority groups in Table 5. To determine what we can measure with the available data, let R = 1/0 indicate if the individual is a member of the priority groups. R is not observed in the population at large. Hence, we cannot estimate the participation rate $P\left(D=1|R,T\right)$ conditional on R. What we can estimate is the probability of being a participant with characteristic R:

$$P\left(D=1,R|T\right)$$

or the relative impact of T_1 on the probability of participation conditional on R,

$$\frac{P\left(D=1|R,T=T_1\right)}{P\left(D=1|R,T=C\right)}=\frac{P\left(D=1,R|T=T_1\right)/P\left(R|T=T_1\right)}{P\left(D=1,R|T=C\right)/P\left(R|T=C\right)}=\frac{P\left(D=1,R|T=T_1\right)}{P\left(D=1,R|T=C\right)}, \qquad (3)$$

where $P\left(R|T=T_1\right)=P\left(R|T=C\right)$ because of orthogonality of T_1 and C to R. Similar expressions can be written for the other treatment comparisons. Assuming orthogonality of treatment to R is not in itself different from the assumption that the randomization was successful. In particular, as the treatment groups exhibit similar gender and age structure within each professional category, we can safely assume that they would also have no different proportion of person belonging to the priority groups.

In a regression framework, we are estimating a linear probability model similar to (2) in which the dependent variable is $D_{ikd}R_{ikd}$, equal to 1 if individual i of professional category k in department d was member of a priority group and came to clinic A, and to 0 if either it did not come to clinic A or came but was not in a priority group:

$$D_{ikd}R_{ikd} = \delta_0 + \delta_1 T_{kd}^1 + \delta_2 T_{kd}^2 + X_{ikd}\beta + \varepsilon_{ikd} . \tag{4}$$

The ratio in (3) is equal to $1+\dfrac{\delta_1}{\delta_0 + X_{ikd}\beta}$, greater than 1 whenever $\delta_1 > 0$.

Results reported in Table 5 show that there was a much larger increase in demand by non-members of priority groups due to information (T_1) (with an impact of 0.56 percentage points over a base of 0.25, or 224%) than by members of priority groups (with an impact of 0.29 over a base of 0.51, or 36%). Increase in salience due to information about scarcity thus mobilized a huge response among those not in priority groups to receive a vaccine. Members of priority groups demonstrated remarkable cooperative restraint that cancelled their increase in demand due to information about scarcity and deadlines. That cooperative restraint would result in no increase in demand among this group was clearly not part of the scarcity management strategy. By contrast, the call on cooperative restraint only cancelled less than half the increase due to information among non-members of priority groups. The consequence is that the totality of the increase in demand originated with non-members of priority groups, a definitely unintended effect as well.

4.5. The large increase in first-timers

The exceptional rise in demand in 2004 can be measured in a non-experimental set-up from the survey. Denote by D_t the indicator for having received a flu vaccine in year t (2002 or 2003). The proportion of first-timers (meaning not having been vaccinated the previous year) in the population that was vaccinated in year t is the conditional probability $P\left(D_{t-1}=0 \middle| D_t = 1\right)$. We do not observe this ratio in the population at large, although we do observe it in the population that came to get a flu shot at clinic A, $P\left(D_{t-1}=0 \middle| D_t = 1, D^A = 1\right)$, where D^A denotes coming to clinic A.

Using standard conditional probability relationships, we can write:

$$P\left(D_{t-1}=0 \middle| D_t = 1\right) = P\left(D_{t-1}=0 \middle| D^A = 1, D_t = 1\right) \frac{P\left(D^A = 1 \middle| D_t = 1\right)}{P\left(D^A = 1 \middle| D_{t-1} = 0, D_t = 1\right)} .$$

We make, in addition, the reasonable assumption that the probability of coming to clinic A in 2004, conditional on having received a flu vaccination in year *t*, is independent of whether one had or not received a flu vaccination the previous year $t-1$:[15]

$$P\left(D^A=1 \middle| D_{t-1}=0, D_t=1\right)=P\left(D^A=1 \middle| D_{t-1}=1, D_t=1\right)=P\left(D^A=1 \middle| D_t=1\right).$$

This gives: $P\left(D_{t-1}=0 \middle| D_t=1\right)=P\left(D_{t-1}=0 \middle| D^A=1, D_t=1\right)$, meaning that the observed ratio of first-timers each year in the population that came to clinic A measures the share of first-timers in the population at large. This reasoning is independent of treatment and hence apply to clinic B also.

Results in Table 6 show a sharp increase in first-timers' demand for vaccination in 2004 compared to previous years.[16] This is seen by the incidence of first-timers for flu vaccination among participants, compared to the incidence of first-timers in the previous year, in the non-campus community and in campus group *C* in clinic A, and in the non-campus community in clinic B. These are the three groups that did not receive any special information from campus about deadlines or affidavits, and hence who were responding to general knowledge about scarcity. At clinic A, 11.9% of non-campus community participants were first-timers in 2004, compared to rates of first-timers of 3.1 and 3.7% the two previous years. In group *C*, 25.7% were first-timers in 2004, compared to rates of 13.5% and 5.9% the two previous years[17]. The phenomenon of rising demand was even sharper at clinic B, with information on shortage more widely available in the press. At this clinic, 22.6% of non-campus community participants were first-timers in 2004 compared to rates of 5.8% and 3% the two previous years.

These sharp increases in first-timers for flu vaccines could be due to any year 2004 effect. However, the dominant phenomenon that year was greater information in the media about the existence and importance of flu vaccination, and about the existence of a shortage. We can thus conclude that, as expected from the literature on responses to scarcity, the spread of information

[15] This recognizes path dependency in demanding vaccination, but assume that is a Markov process. This is saying that, conditional on the behavior in 2003, there is no direct effect of the behavior in 2002 on coming to clinic A in 2004. And similarly, conditional on behavior in 2002, there is no effect of behavior in 2001 on coming to clinic A in 2004.

[16] In 2004, demand is measured by "coming to the clinic to seek vaccination". For the previous years, we use "has received a flu vaccine" as demand since there was no restriction.

[17] Although six clinics were announced in 2002 for this health center, delays in shipment disturbed the announcement of clinic dates, which were progressively scheduled as vaccines became available, and at the end only five clinics were effectively held. To the extent that unreliable supply and uneven announcements discourage potential newcomers more than regular customers, this could explain a lower value for the ratio of first-timers in 2002 compared to 2003.

about a fall in supply led to a sharp increase in demand from people who had never requested a flu shot before.

Why did first-timers come to the clinics compared to previous users? To answer this question, Table 7 compares first-timers and previous users at clinic A in terms of membership into priority groups and other reasons invoked for desiring a flu shot. We restrict the analysis to members from the community and campus groups C and T_1, which were not subject to the call on restraint. The results are quite revealing of who the first-timers are. While 64.2% of the previous users are members of a priority group, this applies to only 33.4% of the first-timers. The reasons these non-priority individuals invoke for wanting a flu shot are that they cannot afford to miss a day of work or study, that they are concerned with a potential epidemic, and that they are at risk of contagion due to exposure to others (living in dorms, being in contact with people, traveling abroad). Hence, first-timers are driven by anxiety, salience, and decreased procrastination more than by seriousness of medical consequences, which would qualify them as priority.

V. THE VACCINE RECIPIENTS: EVIDENCE ON CHEATING

5.1. Evidence on cheating under screening

How can cheaters be detected? The anonymous survey, filled by candidates for a flu shot, asked for a confidential self-declaration as to whether the person belonged or not to each priority group, with the possibility of belonging to more than one. Some people walked away after filling the questionnaire as they admitted not belonging to any priority group. For those who remained in line, the medical personnel engaged in superficial verification (with no proofs asked) that the individual qualified for receiving a vaccination. Screening was unexpected at clinic A, but fully expected at clinic B as it was explicit in the clinic announcement. All candidates for a flu shot thus had to officially announce membership in one of the priority groups in order to be considered for vaccination, had they declared confidentially in the survey that they were in one or not. The screening nurse then decided to accept or reject the candidate. We thus have information from each candidate about: (1) whether self-declared in a priority group or not, and (2) whether the individual received a flu shot or not (as he either walked away or was denied). This allows us to construct four categories of candidates in columns 1 through 4 of Table 8:

- Effective screening: These are the candidates who declared in the survey not belonging to a priority group and who were not serviced, either because they walked away by themselves or were screened out by the center staff. Many of them might have been uninformed about the call for cooperative restraint and screening (screening was not announced for clinic A), while others probably came with the intention to cheat (the schedule for clinic B was always given with information that screening would be enforced).

- Legitimate service: Those are the candidates who declared in the survey belonging to the priority groups and were indeed serviced.
- Exclusion error (Type II): Those are the candidates who declared belonging to the priority groups, but were however denied a flu shot. While this could be a genuine exclusion error, it is more likely a category of persons that were properly detected not being priority while they self-declared being priority in an attempt to cheat.
- Inclusion error (Type I): Those are non-priority persons who were serviced (cheaters). They probably spoke the truth in the survey, but still orally declared being in a priority group to the staff, signed the affidavit, and were not screened out.

Effective screening, revealing lack of information or intention to cheat, was unimportant for non-campus community participants (column 1): the rejection rate was very low (2.9% at clinic A and 1.9% at clinic B). However, this was not the case among campus candidates at clinic A where it reached 25.1% in group *C* and was significantly higher in T_1 (36.1%) and T_2 (32.5%) than in *C*. While non-priority candidates may have come to the clinics because of lack of information on the existence of priority groups, this could not be the case for at least campus group T_2 at clinic A and for the whole campus population at clinic B (where screening had been announced). And yet, it is interesting that screening was higher in the treatment group T_2 than in the control group, although again not precisely measured and not significantly higher because of the small number of observations. This suggests that attempting to cheat the system was reinforced by anxiety created by information about scarcity, even when accompanied by explicit calls on cooperative restraint.

Legitimate service (column 2) was almost universal in the non-campus community (92% at clinic A and 97.2% at clinic B). It was also high among campus participants to clinic B (88.2%). It was low, however, among campus participants to clinic A, and lower in the treatment groups T_1 and T_2 than in *C* due to the importance of screening and cheating for these participants.

Exclusion errors, whereby members of priority groups were denied vaccination, were almost non-existent in both clinics and for all groups (column 3). Screening by nurses was thus on the side of concern with exclusion errors, at the cost of greater inclusion errors. If the health center's objective was to weight exclusion errors more heavily than inclusion errors, to make sure that a minimum number of people at risk would be left un-serviced, then screening was indeed very effective.

Finally, in column 4, cheaters are those who self-declared not being in a priority group, yet were given a flu shot. Cheating behavior is better measured as the share of non-members of priority groups that obtained a vaccine as reported in column 6. This share is very high among non-campus community members, which is understandable given the higher cost for them of getting to the clinic. Among campus members, it increased from 24.1% in C, to 28.3% in T_1, and to 33.8% in T_2.

What we used above to identify cheaters was presumed truthful self-reporting in the survey of not being in a priority category, and yet making it through scrutiny of the medical personnel and receiving a flu vaccine. There can, however, be cases where self-reporting may not have been truthful in spite of guaranteed anonymity. In this case, cheaters are people who falsely declared themselves to be in a priority category in the survey, did this again on the required affidavit, and were not detected by medical personnel because providing hard proof of being in the category was not demanded. We will show evidence in the next paragraph that indeed there was misreporting on at least one priority criterion. How does potential cheating on the survey affect the proposed classification? Suppose that some candidates declared themselves as members of a priority group while they are not. If they are denied a vaccine, they should be classified as effective screening rather than exclusion error. If they received the vaccine, they pertain to the category of cheaters rather than legitimate service. Potential misreporting on the survey thus creates a downward bias on the measure of cheating and an upward bias on the extent of exclusion errors. Our measure is thus a conservative estimation of cheating.

The contrast between first-timers and previous users is also quite revealing of who the first-timers are. First-timers contain a greater share of individuals uninformed and/or intent on cheating, both in the control and treatment groups. However, among non-priority people (column 6), it is previous users that accounted for the highest share of cheaters. While this may not be the only interpretation, it can be due to loss aversion (Bowman et al., 1999; Tversky and Kaneman, 1991)

5.2. Impact of the information campaign, call on cooperative restraint, and light screening on distributed vaccines.

In Table 9, we report the impact of treatments T_1 and T_2 on the number of vaccines distributed, contrasting vaccinations given to members and non-members of priority groups. Overall, 75% of those that came to the clinic in the control group received a flu-shot, for a total of 0.57 percentage points. Most of these flu-shots (0.51) were given to members of priority groups, although a few (0.06) went to non-members of priority groups.

Sending information by email reminding people that there was a shortage and hence only two remaining clinics increased substantially not only the demand for vaccines (+110.1% in Table 3) but also the number of vaccines distributed after screening, by 0.44 percentage points, or 77.2% of the number of vaccines in the control group. Calls on cooperative restraint induced a significant decline in demand (-37.5% in Table 3) and in vaccines distributed (by 0.36 percentage points or 35.6% of the number of vaccines under treatment 1, in Table 9). In the end, information, cooperative restraint, and screening resulted in an (insignificant) increase of 0.08 percentage points or 14.4% over the control number of vaccines distributed to the campus population (Table 9).

What is striking in these results is that while information was effective in bringing to the clinic a large number of members of priority groups that the CDC certainly wanted to vaccinate (a 52.8% increase over C in column 2), it induced a far greater increase in vaccination among non-priority groups (a 282% increase in column 3), despite a certain level of screening at the clinic itself. The cooperative restraint response further exacerbated this contrast, inducing a somewhat larger decrease in the priority population than in the non-priority population (-39.5% vs. -21.8%). Hence, considering together information, cooperative restraint, and screening, the overall increase in vaccination (14.4%) is solely due to an extraordinary increase in vaccination of non-priority people (+198.3%). Their share in the vaccinated population rose from 11% to 27%. This indicates that cheating was indeed extensive among those who were vaccinated.

5.3. Evidence on cheating from the survey questionnaire

How else can cheaters be detected? How can we know that self-reporting was not truthful? Only if there are obvious statistical irregularities in the risk categories invoked.

One such irregularity that reveals cheating is in the pattern of ages declared. Figure 1 representing the distribution of self-declared ages is striking in showing a peak at age 65, preceded by a dip with missing numbers between ages 60 and 64. The 65 years old group is two to three times larger than the average per age between 66 and 70. This is true for non-campus community as well as campus participants, so we pool all the data from the two clinics in analyzing this pattern. Existence of an abnormally high number of participants of age 65 is formally analyzed with the estimation of an age profile for participants.

Discontinuity at age 65 is due to two effects: one is the age eligibility criterion that would imply a discontinuity between ages 64 and 65, with more participation of 65 years old; the other is cheating on age where people younger than 65 declare themselves to be 65. The discontinuity that would reveal cheating must consequently be measured from above. To do this, we estimate the age profile of participants 66 years old and above only, and predict from above the participation at age 65. We explored different functional forms (3rd degree polynomials in age, 1/(1+ age)). The estimated curves are reported in Figure 1.

Cheating at 65 is measured by the difference between observed and predicted number of participants to the clinics. Predicted numbers of 65 years old are 30.7 (standard error of 3.2) with the 3rd degree polynomial and 34.6 (standard error of 1.7) when function of 1/(1+ age). The observed number of 77 is more than twice the predicted values, estimated with relatively high precision. This suggests widespread cheating on age. Because there was no verification of age, most of this cheating could go undetected. Estimation of "missing" 61-64 years old is not precise as the profile of candidates between 50 and 61 years old is not smooth. However, the corresponding estimate of the distance between observed numbers and predicted numbers for these four age groups gives a missing number of 75.3 persons (standard error of 23.3).

VI. CONCLUSION

The first objective of this paper was to analyze the effectiveness of the strategy used to manage the 2004 flu vaccines scarcity crisis based on responses observed on a U.S. university campus. The strategy mandated by the Center for Disease Control consisted in the definition of priority groups and in a call on cooperative restraint by the rest of the population, supported by soft-screening. The expected outcome of the strategy was a reduction in demand to accommodate the shortage, while servicing the priority groups.

Results from the controlled experiment we set up to decompose the effects of information on scarcity/deadlines and of calls on cooperative restraint show that the outcome was quite different from what was expected both in terms of the magnitude of the response and of who responded.

Analysis of demand shows that there was a very large effect of information on scarcity and approaching deadlines (+110%), but that the call on cooperative restraint was also valuable, reducing demand by 38% (i.e., canceling 72% of the increase in demand due to information). However, in the end, the strategy could not prevent a 31% net increase in demand at the clinic where we ran the experiment. Neither could soft-screening of candidates prevent a 17% increase in vaccines effectively distributed compared to no strategy. The most disturbing result is that the increase originated entirely in non-priority people (+198%) whose share of distributed vaccines increased from 11% to 27% as a consequence of the strategy.

In terms of shortage management, the main lesson learned from this experiment is thus that calls on cooperative restraint supported by soft-screening appear to be insufficient to manage the scarcity of such a vital good as a flu vaccine. This lesson in crisis management should be remembered as repeats of flu shortages may be looming in the future.

The second objective of the paper was to identify the behavioral responses of different types of individuals. The most unexpected result was the uncalled-for cooperative restraint observed among priority individuals that fully erased their desired increased share of distributed vaccines. Disturbing is that this management strategy reinforced cheating. Because there was soft-screening at the clinic, the possibility was offered of walking away when learning about the screening or of trying to get through and be vaccinated despite admitting of not being member of a priority group. Based on self-declaration of being member or not of a priority group, the percentage of cheaters among non-priority individuals increased from C, to T_1, and to T_2. How cheating occurred is evident on age declaration, with the number of "65 years old" more than twice the predicted value, while about half of the predicted 61-64 years old are missing.

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Table 1. Number of faculty, staff, and students by random treatment and control groups

Campus population	C	T_1	T_2	Total
Professional categories				
Faculty	1,640	834	559	3,033
Staff	1,915	650	452	3,017
Graduate students	4,463	3,263	1,897	9,623
Undergraduate students	9,658	3,948	9,325	22,931
Total	17,676	8,695	12,233	38,604

C = control, T_1 = reminder and information on only two remaining clinics, T_2 = same information and call on cooperative restraint.

Table 2. Randomization tests on treatment groups

	T_1 vs. C	T_2 vs. T_1	T_2 vs C
Professional categories	p-value for the test of equality between the randomized groups		
Faculty			
Gender	0.44	0.77	0.57
Age	0.16	0.16	0.34
Race	0.18	0.77	0.20
Occupation	0.38	0.04	0.13
Wage category	0.77	0.76	0.47
Distance to health center	0.47	0.99	0.55
Discipline	0.03	0.53	0.56
Staff			
Gender	0.40	0.72	0.22
Age	0.35	0.27	0.06
Race	0.83	0.68	0.70
Occupation	0.73	0.61	0.46
Wage category	0.30	0.78	0.69
Distance to health center	0.80	0.55	0.70
Discipline	0.13	0.58	0.09
Graduate students			
Gender	0.84	0.45	0.47
Distance to health center	0.52	0.61	0.21
Discipline	0.14	0.52	0.30
Undergraduate students			
Gender	0.59	0.27	0.14
Distance to health center	0.66	0.05	0.00
Discipline (undeclared included)	0.18	0.00	0.00

C, T_1, and T_2 as in Table 1. The age distribution is given in 10 categories: less than 25 years old, 5-year intervals between 25 and 65 years old, and 65 years old and over. Races are white, asian, and others. Occupations for faculty are: tenured professors, non-tenured professors, recall and emeritii, lecturers, and others. Occupations for staff are executives and managers, professional staff, and support staff. Wage categories are: less than $40K, $40K-50K, $50K-60K, $60K-70K, and more than $70K. Distance to health center is more/less than 0.5 mile. Discipline categories are social sciences, sciences, and other. 38% of all undergraduates have not yet declared a discipline.
Tests are Pearson's chi-squared corrected for the sampling design.
Source: Human Resources Customized Pivot Tables.

Table 3. Impact of information and call for cooperative restraint on demand

Dependent variable: Came to clinic A	(1) ATE	(2) ATE	(3) ATE	(4) ATE	(5) ITE	(6) ITE
T_1	0.85	0.85	0.84	0.89	0.81	0.86
	(2.76)	(3.63)	(2.85)	(2.97)	(2.69)	(2.92)
T_2	0.24	0.25	0.30	0.34	0.22	0.36
	(0.75)	(1.56)	(1.20)	(1.38)	(0.76)	(1.65)
Non-compliers				0.38		
				(1.58)		
Constant	0.77				0.77	
	(8.61)				(7.86)	
Difference						
T_2 - T_1	-0.61	-0.59	-0.54	-0.55	-0.60	-0.50
	(1.42)	(2.12)	(1.17)	(1.19)	(1.52)	(1.16)
Controls						
Professional category	N	Y	Y	Y	N	Y
Distance	N	Y	Y	Y	N	Y
Gender	N	N	Y	Y	N	Y
Age category	N	N	Y	Y	N	Y
Discipline	N	N	Y	Y	N	Y
Observations	38604	38604	38597	38597	38604	38597
R-squared	0.00	0.01	0.02	0.02	0.00	0.02

C, T_1, and T_2 as in Table 1. t-statistics in parentheses, standard errors clustered at the department x professional category level. All coefficients are multiplied by 100.

Table 4. Heterogeneity of impact across professional categories.

Dependent variable: Came to clinic A	(1) Faculty	(2) Faculty	(3) Staff	(4) Staff	(5) Graduate	(6) Graduate	(7) Undergraduate	(8) Undergraduate
T_1	3.43	3.06	0.59	0.53	0.46	0.36	0.70	0.80
	(2.89)	(2.49)	(0.99)	(0.81)	(1.87)	(1.32)	(2.28)	(1.94)
T_2	0.71	1.03	1.18	1.16	0.05	0.09	0.13	-0.54
	(0.59)	(1.15)	(1.22)	(1.24)	(0.18)	(0.36)	(1.82)	(0.73)
Constant	3.05		1.25		0.74		0.41	
	(5.91)		(3.62)		(5.00)		(5.82)	
Difference								
T_2 - T_1	-2.72	-2.03	0.59	0.63	-0.40	-0.27	-0.57	-1.34
	(1.78)	(1.47)	(0.57)	(0.67)	(1.33)	(0.95)	(1.89)	(1.22)
Controls								
Distance	N	Y	N	Y	N	Y	N	Y
Gender	N	Y	N	Y	N	Y	N	Y
Age category	N	Y	N	Y	N	N	N	N
Discipline	N	Y	N	N	N	Y	N	Y
Observations	3033	3028	3017	3016	9623	9622	22931	22931
R-squared	0.01	0.04	0.00	0.02	0.00	0.00	0.00	0.00

C, T_1, and T_2 as in Table 1. t-statistics in parentheses, standard errors clustered at the department level. All coefficients are multiplied by 100.

Table 5. Heterogeneity of impact by priority status

Dependent variable:	(1) In priority groups and came to clinic	(2) Not in priority groups and came to clinic
T_1	0.29	0.56
	(2.21)	(3.18)
T_2	-0.03	0.29
	(0.31)	(3.10)
Difference		
T_2 - T_1	-0.32	-0.27
	(2.07)	(1.30)
Controls		
Professional category	Y	Y
Distance	Y	Y
Observations	38604	38604
Mean of dependent variable in C	0.51	0.25
R-squared	0.01	0.00

C, T_1, and T_2 as in Table 1. t-statistics in parentheses, standard errors clustered at the department x professional category level.

Table 6. Increase in demand induced by the shortage: Share of first-timers in 2002-2004 among participants

	Clinic A Campus group *C*	t-stat for difference with previous year[1]	Clinic A Non-campus community	t-stat for difference with previous year	Clinic B Non-campus community	t-stat for difference with previous year
2002	5.9		3.7		3.0	
2003	13.5	1.8	3.1	0.4	5.8	1.1
2004	25.7	2.9	11.9	4.0	22.6	4.7

[1]Standard errors clustered at the department x professional category level.

Table 7. Contrasting first-timers and previous users

	First-timers	Previous users	t-stat on difference
	(percent)		
Membership to official priority groups			
Adults 65 years of age or older	6.3	27.9	-2.8
Under chronic medical conditions	13.3	27.7	-1.8
Women who will be pregnant during the flu season	5.2	6.8	-0.3
Contacts with infant	6.8	5.5	0.3
Health-care worker	1.8	1.6	0.1
At least one of the above	33.4	64.2	-2.7
Other reason	68.7	44.5	2.4
Number of observations	112	466	
Reasons for wanting a flu shot among non members of priority groups			
Contact with children	4.0	17.9	-0.9
Can't afford to miss work or study	73.7	61.2	0.6
Believe shortage is just temporary	2.3	1.8	0.2
Concerned by shortage or potential epidemic	45.7	15.3	3.1
Other reasons[1]	17.4	43.3	-1.3
At least one of the above	91.4	88.1	0.3
Number of observations	57	69	

Non-campus community and campus groups C and T_1 (using sampling weight) from Clinic A.
Standard errors clustered at the department x professional category level.
[1] Other reasons include: living in dorms, being in contact with people, don't want to be sick, travel abroad.

Table 8. Evidence on effective screening, legitimate service, exclusion errors, and inclusion errors (cheating)

	Effective screening: Non-priority not serviced	Legitimate service: Priority serviced	Exclusion error: Priority not serviced	Inclusion error: Non-priority serviced	p-value for test of equality with group above	Share of cheaters among non-priority
	(1)	(2)	(3)	(4)	(5)	(6)
Criteria for definition of types						
Self-declared priority group	No	Yes	Yes	No		
Received flu vaccine	No	Yes	No	Yes		
	(Percent of participants in each category)					(%)
Clinic A: categories of participants						
Non-campus community	2.9	92.0	0.00	5.1		64.0
Campus group C	25.1	66.9	0.00	8.0	0.000	24.1
Campus group T_1	36.1	48.7	0.95	14.3	0.116	28.3
Campus group T_2	32.5	50.4	0.63	16.6	0.917	33.8
Clinic B: categories of participants						
Non-campus community	1.9	97.2	0.00	0.9		33.3
Campus population	6.8	88.2	0.79	4.2	0.002	38.1
Clinic A, campus group C						
Previous users	13.5	81.4	0.00	5.1		27.2
First-timers	56.7	28.8	0.00	14.5	0.000	20.4
Clinic A, campus groups T_1 and T_2						
Previous users	25.9	61.5	0.36	12.3		32.3
First-timers	50.7	25.8	1.92	21.7	0.003	30.0

C, T_1, and T_2 as in Table 1.

Table 9. Impact of information and call on cooperative restraint on the number of vaccines distributed

	(1)	(2)	(3)
Dependent variable:	Received a vaccine	In priority groups and received a vaccine	Not in priority groups and received a vaccine
T_1	0.44	0.27	0.17
	(2.89)	(2.13)	(3.33)
T_2	0.082	-0.04	0.12
	(0.53)	(0.34)	(1.71)
Difference			
T_2 - T_1	-0.36	-0.31	-0.05
	(1.78)	(2.03)	(0.62)
Controls			
Professional category	Y	Y	Y
Distance	Y	Y	Y
Mean of dependent variable in C	0.57	0.51	0.06
Observations	38604	38604	38604
R-squared	0.01	0.01	0.00

C, T_1, and T_2 as in Table 1. t-statistics in parentheses, standard errors clustered at the department x professional category level.

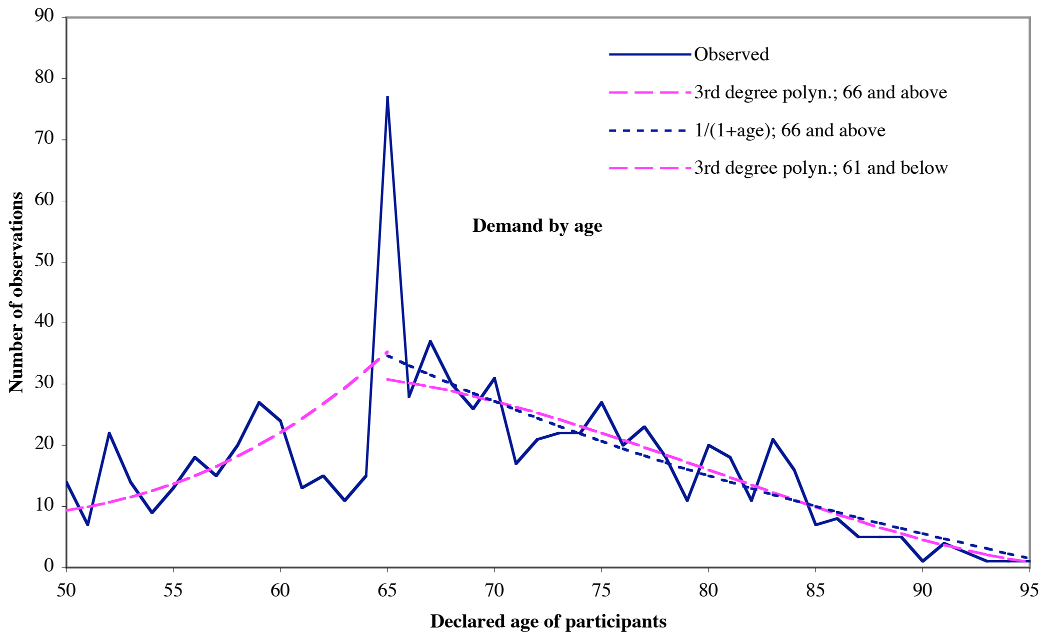

Figure 1. Evidence of cheating in self reporting: Bunching at age 65