Can Appeals to Cooperation be Effective in Managing the Scarcity of a Vital Good? Responses to the 2004 Flu Vaccine Shortage[†]

Alain de Janvry, Elisabeth Sadoulet, and Sofia Villas-Boas

University of California at Berkeley

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Abstract

This paper uses an experimental design to assess the effectiveness of calls on cooperation in managing the shortage of a vital commodity through non-price mechanisms. Using the large unexpected shortage of flu-vaccines in the Fall 2004, we observed the responses of the members of a campus population to two distinct randomized treatments: providing information about a sharply reduced number of vaccination clinics and their schedule, and providing the same information plus an appeal on voluntary restraints to favor priority groups. We find that information about the reduced number of clinics induced a 119% increase in demand. Appeals to cooperation helped reduce this demand by only 19%. Information about scarcity was effective in increasing the vaccination of priority individuals by 54%, with virtually no exclusion errors among those who came to the clinic. However, a disconcerting result is that people not in priority groups responded even more to the information about scarcity than members of priority groups and, despite some screening done at the clinic itself, largely managed to be vaccinated. For every four legitimate additional vaccinations, three were given to non-priority persons. Furthermore, this number is certainly an underestimation, as it is based on self-declared membership in priority groups, and we have evidence that there was cheating on these declarations. The sobering conclusion is that broad-scale calls on cooperation, complemented by soft-screening, to manage the vaccine shortage were largely defeated by rising salience of vaccination and reduced procrastination among non-priority groups and by extensive cheating.

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Induced demand

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

Cooperation

"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, while targeted subsidies can be used to ease the burden of adjustment on selected groups considered at risk. When the price is fixed, allocation of the scarce commodity across wanting individuals is done by the introduction of rules to distribute the commodity to those presumed most in need. These rules can be implemented by screening and/or by appeals to voluntary restraints. However, information about the shortage, including through the very calls on restraint, also induces an increase in demand associated with greater salience of the commodity and reduced procrastination in acquiring the good. Cooperative responses by some in refraining from acquiring the commodity are thus countervailed not only by defaults on cooperation (cheating) by others, but also by increases in demand induced by the shortage.

Given these contradictory behavioral responses, the net effect may be in favor of cooperation and effective screening, leading to an aggregate decline in demand, or of increased salience and reduced procrastination, resulting in an increase in demand. 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 appeals on voluntary restraints 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 question we ask in this paper.

We took advantage of the large unexpected flu-vaccine shortage that occurred in the Fall of 2004 to set up a randomized experiment to decompose responses to the shortage. Because the approach followed by health authorities was to manage the shortage by a call on voluntary restraints complemented by soft screening, we use the observed behavioral responses to the experiment to measure how far can calls on cooperation go in managing scarcity. The experiment took place at a flu-clinic 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 and their corresponding schedule; 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. 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. For the first, we decompose quantitatively the different behavioral responses at play on demand for a vaccination: the influence of the reminder effect of the email plus information about scarcity is measured by the difference in behavior between T_1 and C; the influence of cooperation, conditional on information about scarcity, 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 in increasing demand, particularly among non-priority people, which was only mildly counteracted by voluntary restraints. For the second, we decompose the roles of information, cooperation, and screening on the distribution of vaccines. An analysis of the self-declared membership in priority groups provides evidence on the extent of cheating among candidates for a flu vaccine. Results show that information was effective in raising the vaccination of priority individuals, but that there was extensive cheating by non-priority individuals who, in the end, absorbed as much as 44% of the increase in vaccinations distributed.

The paper is organized as follows. In section II, we review the literature on responses to a shortage to classify the known categories of behavior. In section III, we describe the experimental design and the data collected at the two clinics. Section IV gives results from the impact analysis on the roles of information and cooperation on the demand for a flu shot. Section V presents an analysis of the recipients of a flu shot and gives evidence on cheating. Section VI concludes on the effectiveness of calls on cooperation in managing scarcity.

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

2.1. Increased salience as a response to scarcity

It is well recognized that announcements 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.[1] 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 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, where time waiting in line became the rationing device (Adelman, 2004). That scarcity enhance 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.[2] 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.[3] Because the commodity is of vital importance for people at risk, information to induce them to seek vaccination also creates greater salience of vaccination among non-priority groups, resulting in an obvious dilemma for the management of scarcity without strict screening.

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[1] 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.

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

[3] 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.

2.2. Decreased procrastination as a response to scarcity

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 applies as well 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, presence of a deadline increases the probability of redemption (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.

2.3. Cooperation as a response to scarcity

Voluntary 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 be non-cooperative. Yet, there is also abundant evidence of voluntary cooperation 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. Voluntary cooperation is possible even in large social groups as it can be motivated by the quest for social approval (Holländer, 1990), 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 cooperation to manage a flu shot shortage, the expectation is that individuals not in priority groups may voluntarily incur the risk of being sick to allow the scarce resource to reach the people most in need, even under situations of large group anonymity, weak enforcement, and no expectation that the particular situation will be repeated. Observing the behavioral responses to the flu shot shortage in an experimental setting allows us to measure how strong this response is, and whether it indeed offers a meaningful possibility to manage the shortage of a vital good.

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.[4] 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 specified 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 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 flu-shots to priority groups. The county where the campus is located did not at that time officially announce enforcement of this directive.[5]

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,

[4] 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.

[5] "There is a strong spirit of cooperation during this crisis," said the respective 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.

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 they belonged to one of the priority categories, but with no proof asked. These categories 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). 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 a flu shot would be asked to sign an affidavit that they belong to one of the priority groups. By the time of this last clinic, that 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 for cooperation in refraining from seeking vaccination if a person did not belong to a priority group.[6] The priority groups were described in detail in the T_2 email. The remaining departments (the control group C) received no email.

[6] Cooperation is here defined as "being informed of the reduced number of clinics, and not coming to a clinic in response to the call for the population not at risk 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, impact on demand of providing a reminder of vaccination opportunity together with information about a reduced number of clinics and their schedule.
- From comparison of the T_2 and T_1 groups, impact on demand of sending a call on cooperation, conditional on information about the reduced number of clinics and their schedule.
- From comparison of the T_2 and C groups, net impact on demand of sending information about scarcity and the clinic schedule, and of calling on cooperation.

Emails were sent to faculty, staff, and graduate students by the management services officers (MSO) of the different departments. Of the 65 departments on campus, 10 were drawn for T_1 and 7 for T_2, leaving 48 for C. The emails to undergraduate students were sent by the student affairs officers (SAO) for declared majors and by the dean of the college for undeclared students. 8 majors and the undeclared from one college were drawn for T_1, and 3 majors and the undeclared from one college were drawn 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 39,800, 7,930 were in T_1, 11,648 in T_2, and 20,222 in C.

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 about the availability and schedule of the clinics and the call on cooperation, to mutual influence in appreciating the value of getting or not getting a flu shot, or to the fact that people who work together may go together to the clinic, a fact that we observed at the clinics. 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 influence of the email treatment from the indirect influence that would occur through the social network effect, as all members of the same professional category in a department received the same information.[7]

[7] 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

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 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.[8]

3.3. The data and randomization tests

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, and whether individuals got a flu-shot in each of the last three years.

On the day of clinic B, the response rate was once again close to 100% once we started handing out the survey forms.[9] The survey questionnaire was extended to ask whether individuals were or not in each of the priority categories in 2004 and in 2003.

At clinic A, 738 individuals filled questionnaires, with 498 from campus and 240 from the non-campus community. Out of the 468 campus members with departmental information, 32% were from the treatment group T_1, 21% from the treatment group T_2, and 47% from the

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department members, the indirect effect through department co-workers is almost as high as the direct effect of the treatment.

[8] 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. When asked how they had heard about the clinic, and in particular if it was through an email received from campus during the last two days, 17% of participants from C said they did. Despite this response, we are quite confident that very few members of C have effectively received this email. This is because there may have been some confusion with the campus-wide email sent only one week earlier. In addition, the question itself may have induced a “yes” response to something that may have appeared as if it should have been true.

[9] 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 got a shot.

control group. At clinic B, 610 persons filled questionnaires, 385 from campus and 225 from the non-campus community.

Because the randomization was based on departments and because departments have different configurations in terms of faculty, staff, and student composition (see Table 1), each of these groups gives rise to stratified samples of the campus population. Estimates of campus population statistics are thus derived from weighted averages of statistics by professional category.

To verify the validity of the experimental design, we perform randomization tests on observables for the three groups T_1, T_2, and C. Relevant dimensions that could affect behavior toward vaccination on which we have information are gender, age in 10 age categories, race, occupation, and wage category, for faculty and staff; and gender for graduate and undergraduate students. Results are reported in Table 2 for pair-wise differences between T_1, T_2, and C. The similitude between the three randomized groups is excellent for faculty and students. It is however less so for staff, where the control group tends to include more men, over 50, and with more than 4 years of service than the treated groups. This reflects a randomization done over the academic departments, with no attempt to include the large administrative units that henceforth are all in the control groups. These results suggest that findings for staff should be read with caution when comparison are done with the control group. Comparisons between the two treatment groups are reliable. All comparisons for faculty and students can be taken with confidence as the treatment and control groups meet the randomization tests.

IV. IMPACT OF TREATMENTS ON DEMAND FOR A FLU SHOT

Note that, as the information was collected at the clinics themselves, we do not have individual data on the population of interest, namely the whole campus population. For reasons of privacy, we only obtained some aggregate statistics from the campus administration. We know the total number of members of each professional category for each department or major. We obtained the gender and age distribution by professional category, not for each department or major but for each of the three treatment groups of departments or majors. This information allows us to compute participation rates to the clinics for each unit of randomization. To analyze differences in participation rates by subsets of the campus population along characteristics that we observe in the survey (age, gender, whether the individual had been vaccinated in 2003 or not, and whether the individual is member of a priority group or not), we have to rely on the assumption of orthogonality of the treatment to all such characteristics in each professional category.

4.1. Impact analysis: Average treatment effects

We are interested in measuring the impacts of the treatments on demand for a flu shot. Demand under treatment T is defined as the proportion of the campus population that would come to the clinic under that treatment. Equivalently, it is the average probability that a random campus member will come to clinic A under treatment T:

$$P\left(Y^A = 1 | T\right),$$

where Y^A is a binary variable equal to 1 if an individual comes for a flu-shot at clinic A, and T is the treatment (C, T_1, or T_2). Obviously, for each individual member of campus, we only observe one of the three potential outcomes $Y^A | T$. To estimate campus demand under the different treatments, we rely on the randomization scheme and compute campus participation as a weighted average of the participation rates by professional category K and department D in each treatment group:

$$\hat{P}\left(Y^A = 1 | T, K\right) = \sum_{D \in T} \hat{P}\left(Y^A = 1 | T, K, D\right) \frac{N_{KD}}{\sum_{D \in T} N_{KD}}$$

$$\hat{P}\left(Y^A = 1 | T\right) = \sum_{K} \hat{P}\left(Y^A = 1 | T, K\right) \frac{N_K}{N}$$

where $\hat{P}$ are observed participation rates, N_{KD} and N_K are population of category K in department D and on campus, respectively, and N is total campus population. Variances of these estimators are weighted averages of the binomial variances for each professional category and department. Standard errors thus include clustering effects that could be due to social interactions among members of the same professional category in each department.

Table 3 reports demand $\hat{P}\left(Y^A = 1 | T\right)$ under each treatment $T \in \{C, T_1, T_2\}$ and average treatment effects $\hat{P}\left(Y^A = 1 | T\right) - \hat{P}\left(Y^A = 1 | T'\right)$ of T relative to T' in clinic A for the four professional categories and for the whole campus population. Results show that sending information about the reduced number of clinics ($T_1 - C$) induced a doubling of the demand for flu vaccine (from 0.9 to 2.0% of the campus population), and this across all professional categories. The difference between the two treatment groups ($T_2 - T_1$) measures the effect of sending a call on cooperation in addition to information on the reduced number of clinics. This effect on behavior was to decrease demand to 1.6% of the campus population, or by 19.2%, with the largest effect among undergraduate students (-51.1%). These two effects result for the whole campus population in a significant 76.7% net increase between C and T_2. This net increase is largely due to faculty and staff. While this shows that calls on cooperation can indeed be heard, reduced demand by cooperators is far from sufficient to compensate for the increase in demand induced by the reminder email and information about the shortage itself.

4.2. Intention to treat vs. treatment

The treatment effects of *sending emails* that we have thus far considered can be considered as either interesting in themselves, to the extent that they measure the impact of a well defined type of information campaign, or because they provide measures of the intention to treat effects of the treatment defined as *being informed*. In the latter case, the sending of emails can be viewed as an encouragement to be informed. The intention to treat is usually interpreted as a conservative estimate of the treatment effect, although it need not be the case if there is a direct effect of the email beyond the information that it conveys (Hirano, Imbens, Rubin, and Zhou, 2000).

Following the literature on encouragement design, one can distinguish four subpopulations. The never-takers are those individuals that remain untreated despite the encouragement. In our experiment, those are the (never-informed) individuals that have been sent an email but have not read it, and remained uninformed. The subpopulation of always-takers consists of the (always-informed) individuals that are informed regardless of the encouragement. These people found the information on the reduced number of clinics and their schedule or have heard of the Center for Disease Control's call on cooperation from other sources. Given the very short time between the decision to restrict the number of clinics and the clinic itself and the lack of any direct publicity by the health center, this information probably did not spread much (cancellation of the last three clinics was decided and posted on the medical center's website on October 8 but not further advertised). However, the call on cooperation from the Center for Disease Control may have been heard more widely. There is no subpopulation of defiers in this experiment.[10] The last subpopulation, the compliers, consists of the individuals who acquire the information from the email sent to them.

[10] These would be individuals who would be informed without the email but not informed when sent an email.

Under the condition that there is no direct effect of the email on the always-informed and the never-informed, sending the email only has an effect on the compliers. The effect of sending an email (intention to treat effect) is thus a conservative measure of the effect of the *being informed* treatment.

That the never-informed are not influenced by the encouragement design is not controversial. However, for the always-informed, receiving the email provides a reminder effect in addition to the information effect. In this case, the reminder effect of the October 11 email may not have been large since a campus-wide reminder had been sent only a week before. We, however, cannot assess its importance in this context.

How does the effect of "*sending an email*" relate to the effect of "*receiving (or having read) the email*"? Denote by RT the treatments "have read the information on the reduced number of clinics and their schedule" and "have read the call on cooperation from the CDC, in addition to the information on the remaining clinics". Those two RT treatments differ from the treatments that we analyze as not everyone has read its email that same day.[11] However, because of the random design of the email treatments, we can assume that having read one's email is orthogonal to the treatments T, and obtain intention to treat effects of RT that are simple scaling downs of the treatment effects by the email reading rate. This can be seen as follows.

The conditional probability of coming to clinic A can be decomposed into:

$$P\left(Y^A = 1 \middle| T\right) = P\left(Y^A = 1 \middle| Read = 1, T\right) P\left(Read = 1 \middle| T\right) + P\left(Y^A = 1 \middle| Read = 0, T\right) P\left(Read = 0 \middle| T\right).$$

With randomized treatment, the percentage of the campus population that has read its email that same morning is independent of the treatment, i.e., $P\left(Read \middle| T\right) = P\left(Read\right)$. In addition, for the population that has not read the email, treatment is de facto ineffective, i.e., $P\left(Y^A = 1 \middle| Read = 0, T\right) = P\left(Y^A = 1 \middle| Read = 0\right)$. Therefore:

$$P\left(Y^A = 1 \middle| T\right) = P\left(Y^A = 1 \middle| Read = 1, T\right) P\left(Read = 1\right) + P\left(Y^A = 1 \middle| Read = 0\right) P\left(Read = 0\right).$$

Treatment effects are thus scaled down by the probability that the emails were read:

$$P\left(Y^A = 1 \middle| T\right) - P\left(Y^A = 1 \middle| T'\right) = \left[P\left(Y^A = 1 \middle| RT\right) - P\left(Y^A = 1 \middle| RT'\right)\right] P\left(Read = 1\right).$$

In summary, our experiment allows us to strictly measure the effect of sending emails (providing a reminder effect on opportunity for vaccination together with new information on scarcity and the reduced number of clinics, and the call on cooperation from the Center for

[11] 76% of the persons that came to the clinic from T_1 and 69% of those from T_2 said that they had learned about the clinic schedule from an email received in the last two days.

Disease Control) to all the members of a department. The measured effect includes the social network effect internal to the department. It is proportional to the effect of receiving an email, with the scaling factor equal to the email reading rate. Its relationship to the effect of the information itself is mitigated by several issues that could make it either higher or lower than the pure effect of the information contained in the emails.

4.3. Insights on the role of information on demand

We use here the behavioral difference between T_1 and C. Information about the reduced number of clinics from the expected five to only two, and announcement of the schedule for these two remaining clinics, can induce an increase in demand as a consequence of three types of behavioral responses:

i) Rescheduling: These are people who had planned to participate in a clinic that was cancelled and reschedule their visit to the health center to one of the two available clinics. Rescheduling is done by both old-timers (people who were vaccinated in 2003) and first-timers (people who were not vaccinated in 2003).

ii) Response to salience: Increased demand for vaccination induced by greater appreciation of the importance of getting a flu shot. This is a genuinely new demand from people who otherwise would not have wanted to be vaccinated.

iii) Reminder and reduced procrastination: Procrastinators are individuals who wanted to be vaccinated in the past (latent demand), but always postponed doing so under normal times as there were no strict deadlines.[12] Imposition of strict deadlines as a consequence of scarcity prevents them from postponing vaccination forever, resulting in an increase in effective demand.

While the role of rescheduling affects both old- and new-timers, the other two effects generate demand among people who would not otherwise have come to the clinic and hence are likely first-timers. We use the contrast between old- and new-timers to assess the importance of salience and procrastination on demand.

Let X = 1/0 indicate if the individual was vaccinated in 2003 (old-timer). X is not observed in the population at large. Hence, we cannot estimate the participation rate $P\left(Y^A = 1 \middle| X, T\right)$ conditional on X. What we can estimate is the probability of being a participant with characteristic X:

[12] Although there was always a limited number of clinics offered in previous years at this particular medical center, vaccination remained available at many other places in the area throughout the season.

$$P\left(Y^A = 1, X \middle| T\right)$$

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

$$\frac{P\left(Y^A = 1 \middle| X, T = T_1\right)}{P\left(Y^A = 1 \middle| X, T = C\right)} = \frac{P\left(Y^A = 1, X \middle| T = T_1\right) \big/ P\left(X \middle| T = T_1\right)}{P\left(Y^A = 1, X \middle| T = C\right) \big/ P\left(X \middle| T = C\right)} = \frac{P\left(Y^A = 1, X \middle| T = T_1\right)}{P\left(Y^A = 1, X \middle| T = C\right)},$$

where $P\left(X \middle| T = T_1\right) = P\left(X \middle| T = C\right)$ because of orthogonality of T_1 and C to X.

While the difference between first- and old-timers reported in Table 4 is not significant, results suggest a much larger response from first-timers (+168.9%) than from old-timers (+106%). This suggests that there was an effect of salience and procrastination in excess of rescheduling.

Contrast between members and non-members of priority groups are similarly established. Results show that there was a very large increase in demand from people who are not members of priority groups (+273%), five times larger than the increase in demand from people in priority groups (+56.4%). This suggests how increased salience occured: while people who are members of priority groups already knew of the importance of vaccination, people who are not responded to information about the shortage by attributing greater salience to flu vaccination and by reducing procrastination. A striking result is that, in absolute numbers, the increase in demand due to information about scarcity by members of non-priority groups (from 0.26% to 0.97% of the campus population) was twice as large (196% larger) as the increase in demand by members of priority groups (from 0.64% to 1.01%). Combining the effects on demand of information about scarcity and calls on cooperation shows that the share of non-priority people in the increase in demand from C to T_2 was as high as 62%.

The exceptional rise in first-timer demand in 2004 versus 2003 is also suggestive of the role of information on demand. Denote by Y_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 $P\left(Y_{t-1} = 0 \middle| Y_t = 1\right)$. 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, $P\left(Y_{t-1} = 0 \middle| Y_t = 1, Y^A = 1\right)$.

Using standard conditional probability relationships, we can write:

$$P\left(Y_{t-1} = 0 \middle| Y_t = 1\right) = P\left(Y_{t-1} = 0 \middle| Y^A = 1, Y_t = 1\right) \frac{P\left(Y^A = 1 \middle| Y_t = 1\right)}{P\left(Y^A = 1 \middle| Y_{t-1} = 0, Y_t = 1\right)}.$$

We make, in addition, the reasonable assumption that the probability of coming to clinic A, 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:

$$P\left(Y^A = 1 \middle| Y_{t-1} = 0, Y_t = 1\right) = P\left(Y^A = 1 \middle| Y_{t-1} = 1, Y_t = 1\right) = P\left(Y^A = 1 \middle| Y_t = 1\right).$$

This gives: $P\left(Y_{t-1} = 0 \middle| Y_t = 1\right) = P\left(Y_{t-1} = 0 \middle| Y^A = 1, Y_t = 1\right)$, 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. In absolute numbers, 52% of the increase in demand due to information about scarcity came from first-timers.

Results in Table 5 show that there was a sharp increase in first-timers for vaccination in 2004 compared to previous years. This is seen by the incidence of first-timers for a flu shot among participants this year, 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, 12.4% of non-campus community participants were first-timers in 2004, compared to rates of first-timers of 3.5 and 3% the two previous years. In group *C*, 20.6% were first-timers in 2004, compared to a rate of 9.2% and 5.9 the two previous years[13]. The phenomenon of rising demand was even sharper in 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 a rate of first-timers 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 shots, 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 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 old-timers? To answer this question, Table 6 compares first and old-timers at clinic A in terms of membership in priority groups, and other reasons invoked for desiring a flu shot. The results are quite revealing of who the first-

[13] 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.

timers are. While 64.6% of the old-timers are members of a priority group, this applies to only 33.7% of the first-timers. The reasons they invoke are that they cannot afford to miss a day of work or study, that they are concerned with a potential epidemic, and other reasons that relate to risk of contagion due to exposure to others (living in dorms, being in contact with people, traveling abroad). Hence, new-timers are driven by anxiety, salience, and decreased procrastination more than by seriousness of medical consequences.

In clinic B to which only members of priority groups came as screening had been announced, 2/3 of the first-timers are people who were in the same priority group last year, and yet had not been vaccinated (the other third being people who became at risk this year). This certainly includes a response to the increased salience of vaccination and in particular to the explicit focus this year on priority groups. However, with vaccination campaigns targeting every year these priority groups, most were certainly aware of the importance for them of being vaccinated, suggesting that we are seeing among them many procrastinators responding to imposition of strict deadlines as a consequence of the shortage.

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 self-declaration as to whether the person belonged or not to each priority category, 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 category. For those who remained in line, the medical personnel engaged in soft 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 categories 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 for vaccination 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 7:

- 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 self-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 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% in clinic A and 1.9% in clinic B). However, this was not the case among campus candidates in Clinic A where it reached 20.8% in group C and was significantly higher in T_1 (32.1%) and T_2 (26.4%) 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 in Clinic A and for the whole campus population in 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 significant 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 cooperation.

Legitimate service (in column 2) was almost universal in the non-campus community (92.5% at clinic A and 97.2% at clinic B). It was also high among campus participants in 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 are denied a vaccination, were almost non-existent in both clinics and for all groups (column 3). Screening was thus on the side of concern for exclusion errors, at the cost of greater inclusion errors. If the 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. There were very few in the non-campus community (4.6% in A and 0.9% in B) and few among campus participants to Clinic B as well (4.2%). Percentages are, however, important among campus participants in Clinic A, and higher when deadlines and scarcity are better known. Thus, the incidence of cheating reached 7.9% in C, and 17% in both T_1 and T_2. Once again, the incidence of cheaters rose with salience and deadlines, and it was not reduced by calls on cooperation.

The contrast between first-timers and old-timers is also quite revealing of who the first-timers are. This group contains a greater share of individuals uninformed and/or intent on cheating, both in the control and treatment groups. They are also more effective at cheating.

5.2. Impact of the information campaign, calls on cooperation, and soft screening on distributed vaccines.

In Table 8, 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. 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 (+118.7% in Table 3) but also the number of vaccines distributed after screening (+85.6% in Table 8). Calls on coooeration induced a significant decline in demand (-19.2% in Table 3), but not in vaccines distributed (-11.9% in Table 8). In the end, information, cooperation, and screening resulted in a 63.4% increase in vaccines distributed to the campus population with no significant role for cooperation.

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 54.1% increase in column T_1 - C), it induced a far greater increase in vaccination among non-priority groups (a 368.7% increase, or almost five fold), despite a certain level of screening at the clinic itself. In absolute numbers, the increase in members of non-priority groups (from 0.07% to 0.34% of the campus population) was equal to 76% of the increase in members of priority group (from 0.64% to 0.99% of the campus population), meaning that for every four additional vaccinations administered to people from a priority group, the clinic vaccinated three additional persons who self-declared not being member of a priority group. While the cooperative response was larger among members of non-priority groups (-28.6% in Table 4) than among members of priority groups (-10.2%), although not statistically different due to the small sample size, this was far from sufficient to compensate for the effect of information on the number of vaccines distributed to non-priority individuals. Hence, considering together information, cooperation, and screening, the share of non-priority people in the vaccinated population rose from 10% (0.07/0.72) to 23% (0.27/1.17). Their share in the increase in vaccinations from C to T_2 was as high as 44% (an increase of 0.20% of campus population (from 0.07% to 0.27% in table 8) over a total increase of 0.45% (from 0.72% to 1.17%) of campus population). 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? 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. How can we know that self-reporting was not truthful? Only if there are obvious statistical irregularities in the risk categories invoked. Two types of irregularities can be detected. One is unusual increases in particular risk conditions between 2003 and 2004 among old-timers. The other is in the age profile of candidates.

While we have no way of verifying if reasons invoked for being in a priority group were truthful or not, we can infer cheating among old-timers from categories where there was a large number of people who declared a change in medical condition between last and this year. Note that new timers do not provide a "smell test" along this line as, if the CDC recommendations were followed, this group would mainly be composed of procrastinators and people newly at risk. Conditions that naturally change from one year to the next, such as pregnancy and caring for infants, cannot be used for this test. Suspicious reported increases over one year are in priority categories such as chronic aspirin therapy (25.3%), chronic medical condition (19.5%), out-of-home care giver (9.1%), and health care worker (5.2%). With no hard verification, it is difficult for a nurse to detect lying on these conditions. The observed large percentages of people who declared a change in their medical condition over the last year suggest that these categories may have been abused to qualify as member of a priority group.

An even more obvious case of cheating can be seen on the age 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 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. The corresponding estimate of the distance between observed numbers and predicted numbers for these four age groups give a missing number of 75.3 persons (standard error of 23.3).

VI. CONCLUSION

In response to the sudden shortage of a vital pharmaceutical commodity with a fixed price, broad-scale appeals to voluntary restraints combined with soft-handed screening were used to manage the crisis and save supplies for the population most in need. The dilemma of managing a shortage in this fashion is immediately apparent as calls on cooperation because of the shortage raise demand as a consequence of increased salience of the vaccine and reduced procrastination. The outcome is consequently undetermined and can indeed backfire if there is a sharply increased demand among non-priority groups and little willingness to screen them out.

We used the flu vaccine shortage of 2004 combined with a randomized treatment of information across departments on a California university campus to identify separately the roles of information about scarcity and calls on cooperation on demand. We then analyzed the vaccinations effectively distributed to assess the effective coverage of priority populations and the importance of cheating.

Results show that information about scarcity and presence of deadlines resulted in a 119% increase in demand for vaccination. This increase came largely from first-timers who increased their demand by 169% and from non-members of priority groups who increased their demand by 273%. A striking result is that, in absolute numbers, the increase in demand due to information about scarcity was twice as large coming from non-members of priority groups than from members of priority groups. Calls on cooperation only had a modest effect, reducing demand by 19%, with the largest response (-29%) coming as expected from members of non-priority groups. The joint effect of responses to information about scarcity and of cooperative behavior was thus a rise in demand by 77%, with first-timers increasing their demand by 119% and non-members of priority groups by 166%. In the end, 62% of the net increase in demand from these two effects originated in non-priority people.

Analysis of the vaccine recipients shows that the information campaign was effective in raising the vaccination of priority individuals by 54%. The population at risk that came to the clinics was effectively serviced, with only minimal exclusion errors. This success was, however, achieved at the cost of extensive cheating. Information about scarcity combined with soft screening raised the participation of non-priority people by 369%. For every four additional vaccinations administered to people from priority groups, three were given to non-priority people. And calls on cooperation had no significant effect on vaccines distributed. The share of non-priority people rose from 10 to 23% of the vaccinated population in spite of calls on cooperation. These non-priority individuals absorbed 44% of the increase in vaccinations distributed. Furthermore, this number is certainly an underestimation of cheating in the screening process as it is based on self-declared membership in priority groups. We have evidence that there was cheating on these self-declarations, most particularly on age. The conclusion is thus that strict screening, as opposed to calls on cooperation and soft screening, may be a more effective way of managing the shortage of a vital commodity outside the market, but clearly at a greater political cost.

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

Campus population	T_1	T_2	C	Total
Professional categories				
Faculty	436	202	803	1,440
Staff	294	226	5,332	5,851
Graduate Students	3,264	1,896	4,457	9,618
Undergraduate Students	3,935	9,324	9,631	22,891
Total	7,930	11,648	20,222	39,800

C =control, T_1=reminder and information on only two remaining clinics, T_2= same information and call on cooperation.
Source: Campus profile database 2004.

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	0.458	0.792	0.570
Age	0.141	0.165	0.332
Race	0.259	0.305	0.225
Occupation	0.830	0.114	0.089
Wage category	0.500	0.727	0.408
Staff			
Gender	0.112	0.901	0.053
Age	0.296	0.334	0.064
Race	0.150	0.676	0.506
Occupation	0.086	0.629	0.195
Wage category	0.225	0.739	0.598
Graduate students			
Gender	0.181	0.214	0.850
Undergraduate students			
Gender	0.238	0.232	0.981

The age distribution is given in 10 age categories: less than 25, 5-year intervals between 25 and 65, and 65 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.

Tests are Pearson's chi-squared taking into account the sampling design.

Source: Human Resources Customized Pivot Tables

Table 3. Impact of information and cooperation on demand: Average treatment effects

	Demand for flu vaccine			Impact of		
				Information	Cooperation	Info&coop
	C	T_1	T_2	$T_1 - C$	$T_2 - T_1$	$T_2 - C$
Demand by professional category (in percentage of each category in the campus population)						
Faculty	6.2	12.4	10.4	6.2	-2.0	4.2
	(0.8)	(1.5)	(2.0)	[3.5]	[0.8]	[1.9]
% difference				98.8%	-15.9%	67.2%
Staff	1.8	4.1	4.9	2.3	0.8	3.1
	(0.2)	(1.1)	(1.3)	[2.0]	[0.5]	[2.3]
% difference				128.9%	19.5%	173.5%
Graduate students	0.7	1.2	0.8	0.5	-0.4	0.1
	(0.1)	(0.2)	(0.2)	[2.0]	[1.5]	[0.2]
% difference				61.4%	-33.8%	6.8%
Undergraduate students	0.4	1.1	0.5	0.7	-0.6	0.1
	(0.1)	(0.2)	(0.1)	[3.9]	[3.1]	[1.3]
% difference				169.2%	-51.1%	31.7%
Total demand (in % of campus population)	0.9	2.0	1.6	1.1	-0.4	0.7
	(0.1)	(0.2)	(0.2)	[5.0]	[1.3]	[3.1]
% difference				118.7%	-19.2%	76.7%
Number of observations	224	143	98			

Standard errors in parentheses. t-stat in brackets.
Source: Flu-shot survey, Fall 2004.

Table 4. Heterogeneity in the impact of information and cooperation on demand

	Number of observations	Demand for flu vaccine			Impact of		
					Information	Cooperation	Info&coop
		C	T_1	T_2	$T_1 - C$	$T_2 - T_1$	$T_2 - C$
		(in percentage of campus population)			(in percentage difference)		
Old timers	332	0.65	1.35	1.12	106.0	-16.5	72.0
					[3.4]	[0.9]	[2.2]
First-timers	118	0.21	0.57	0.47	168.9	-18.4	119.4
					[2.5]	[0.7]	[1.8]
Difference between old- and first-timers					[0.9]	[0.1]	[0.6]
Member of a priority group	291	0.64	1.01	0.91	56.4	-10.2	40.4
					[2.1]	[0.5]	[1.3]
Not member of a priority group	174	0.26	0.97	0.69	273.1	-28.6	166.4
					[3.6]	[1.6]	[2.5]
Difference members and non members of priority groups					[2.7]	[0.6]	[1.8]

t-stat in brackets.
Source: Flu-shot survey, Fall 2004.

Table 5. 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	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		3.0		3.0	
2003	9.2	0.9	3.5	0.3	5.8	1.1
2004	20.6	3.8	12.4	3.4	22.6	4.7

Standard errors clustered at the department*campus category level.
Source: Flu-shot survey, Fall 2004.

Table 6. Contrasting first-timers and old-timers

	First-timers	Old-timers	t-stat on difference
	(percent)		
Members of official priority groups			
Adults 65 years of age or older	6.7	30.2	-2.6
Under chronic medical conditions	10.7	24.3	-1.9
Women who will be pregnant during the flu season	5.7	8.7	-0.6
Contacts with infant	9.2	5.2	0.7
Health-care worker	1.4	0.9	0.5
At least one of the above	33.7	64.6	-2.5
Reasons for wanting a flu shot			
Contact with children	2.2	8.1	-1.3
Can't afford to miss work or study	50.1	13.8	3.7
Believe shortage is just temporary	1.2	0.6	0.6
Concerned by shortage or potential epidemic	32.9	6.7	3.6
Other reasons[1]	11.5	5.7	1.0
At least one of the above	68.0	26.4	4.6
Number of observations	112	466	

Non-campus community and campus groups C and T_1 (using sampling weight) from Clinic A.
Standard errors clustered at the department*campus category level.
[1] Other reasons include: living in dorms, being in contact with people, don't want to be sick, travel abroad.
Source: Flu-shot survey, Fall 2004.

Table 7. 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
	(1)	(2)	(3)	(4)	(5)
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	92.5	0.00	4.6	
Campus group C	20.8	71.2	0.00	7.9	0.000
Campus group T_1	32.1	50.2	0.75	17.0	0.062
Campus group T_2	26.4	56.2	0.39	17.0	0.822
Clinic B: categories of participants					
Non-campus community	1.9	97.2	0.00	0.9	
Campus population	6.8	88.2	0.79	4.2	0.002
Clinic A, campus group C					
Old-timers	10.4	83.8	0.00	5.9	
First-timers	52.8	33.7	0.00	13.5	0.000
Clinic A, campus groups T_1 and T_2					
Old-timers	21.3	64.4	0.25	14.0	
First-timers	46.1	28.1	1.42	24.4	0.022

Source: Flu-shot survey, Fall 2004.

Table 8. Impact of information and cooperation on the number of vaccines distributed

	Number of observations	Number of vaccines distributed			Impact of		
		C	T_1	T_2	Information $T_1 - C$	Cooperation $T_2 - T_1$	Info&coop $T_2 - C$
		(in percentage of campus population)			(in percentage difference)		
Members of priority groups	288	0.64	0.99	0.90	54.1	-9.5	39.5
					[2.1]	[0.4]	[1.3]
Non members of priority groups	52	0.07	0.34	0.27	368.7	-19.3	278.4
					[2.0]	[0.5]	[1.7]
Difference					[1.7]	[0.2]	[1.4]
Total number serviced	340	0.72	1.33	1.17	85.6	-11.9	63.4
					[3.0]	[0.6]	[2.1]

t-stat in brackets.
Source: Flu-shot survey, Fall 2004.

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