LOW TAKE-UP RATES: THE ROLE OF INFORMATION

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Abstract

This paper exploits a quasi-natural experiment to study the role of information in determining take-up patterns of social benefits in a non-stigma environment. We find that take-up rate of households who have the incentive to search for information for a longer period of time is between 8 and 13 percentage points higher as compared to a control group of households. This result is robust to the inclusion of various household characteristics. Our finding provides strong empirical support for information as an important explanation for low take-up rates.

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Introduction

Low take-up rate occurs across countries as well as programs. Estimates of the extent of take-up of social benefits, including our case, range between 40 and 80 percent. In a recent survey on the explanations for low take-up of social benefits Currie (2004) has concluded that "*after many years of research, we still have relatively little insight into precisely what types of cost matter most.*" The goal of this paper is to use a quasi-natural experiment to shed light on the importance of information cost as a potential explanation for low take-up rate of social benefits.

Information cost as a significant explanation for low take-up rate was first emphasized by Coe (1979), who found that the most frequent reason for non-take-up is lack of information.[4] However, this result had limited influence in terms of establishing strong support for the information explanation mainly because self-reporting is seen as evasive.[5] More recently, Daponte et al (1999) used a randomized experiment to provide direct evidence on the importance of information. They found that out of 31 eligible households, 11 households (35 percent) had applied after they were given information while 20 households (65 percent) did not apply for food stamps even after they were informed of their eligibility.

Throughout the years, and especially since the pioneering work of Moffitt (1983) who modeled the decision to take social benefit in a cost-benefit framework, stigma was a much more prevalent explanation for low take-up rate. Several works also point to administrative cost as an important explanation for low take-up rate. For example, Warlick (1982) has shown that residents of small cities, which is a proxy for higher administrative cost, exhibit lower take-up rates.[6] McGarry (1996) found that take-up rates tend to fall as the level of schooling increases. That result was interpreted as

[4] In a subsequent paper Coe (1983) found similar results where the majority of eligible households that do not use food stamps report lack of information as the reason for non-take-up.

[5] In a more detailed questionnaire, people were influenced by stigma even though they cited lack of information as a reason for non-take-up (Currie, 2004).

[6] Administrative cost appears to be an important factor in Dorsett et al (1991), Konig and Ridder (1997) and Bitler et al (2003). In fact, Daponte et al (1999) could be also seen as evidence for the importance of administrative cost. Half of those households that did not apply were eligible for modest monthly benefits ($10 or less).

evidence for the relatively small role of information cost compared to administrative cost.

The search for explanations encounters two main difficulties in examining the relative importance of information. First, the extensive literature on the reasons for low take-up rate of social benefits is based mainly on means-tested social programs. Because the potential effect of stigma on take-up is very evident in those programs, it is difficult to isolate the effect of the other two factors: information and administrative cost.

Second, it is difficult to estimate the role of information based on a general purpose survey or administrative database. The attempt to employ household characteristics to estimate the significance of information as a reason for low take-up rate is questionable. Each attribute tends to influence more than one factor at the same time. For example, education is commonly used to explain variation in take-up rates, but it simultaneously affects stigma, information and administrative cost. High education levels tend to lower the cost of information but at the same time might be associated with higher social and psychological cost (stigma). In addition, higher education may increase (foregone wage) or decrease the cost of administration (lower cost of filling out forms).

The positive correlation between the level of benefits and take-up rates, which is one of the most solid empirical findings in the literature, could not be used as supporting evidence for either the importance of information/administration or for stigma[7]. The cost structure of these three factors may consist of a fixed cost component. Thus, the positive correlation could be in line with all three explanations.

The unique data set on water consumption bills in Israel provides a quasi-natural experiment to study to the role of information cost in determining take-up patterns of social benefits. The water pricing structure in Israel consists of three increasing blocks tariffs (IBT). In 2002, the lowest price applies to the first 96 cubic meters on a yearly basis (first block), additional consumption up to 84 cubic meters is subject to an

[7] For example, Blank and Card (1991) found that higher benefit replacement rates correlated with higher take-up rates. For a comprehensive review see Currie (2004) and Hernanz et al. (2004).

intermediate price (second block), and any extra consumption is charged at the highest price (third block). This pricing structure has an additional feature. Households larger than four persons are entitled to an additional 36 cubic meters per person per year at a low rate.

The monetary value of that additional quantity of water could be up to 8 percent of annual water expenditures in each year for the next 18 years (approximately). This social benefit is non-automatic and a household must complete a very simple form (half a page) to take-up that social benefit. Every household, regardless of its income or wealth, is entitled to this social benefit (i.e., it is not means-tested). The social benefit here should not be associated with stigma because of the following two features: it is both universal and not intended to replace foregone income.

We follow the take-up patterns for three years of two groups of households following a household expansion by one member. The first group consists of five-member households that had expanded to six members for which the information on social benefits have already been relevant prior to the current household expansion. This group of households had the monetary incentive to search for information regarding the program before the current household expansion and is used here as our treated group. The second group, which serves as a control group, is composed of households of four members that expand to five members. The information for the second group was immaterial in the past and became relevant with the household's current expansion.

This information gap is used to test whether households who were potentially exposed to information for a longer period of time react differently in terms of taking-up their social benefits as compared to a control group of households when a household of either type expands by one member. Both groups of households face the same (direct) administrative cost. In addition, all households are entitled to the same social benefits as a result of the current household expansion. The stigma cost in our context does not play an important role, if any.

In the next section we sketch a very simple model of take-up. In section 3 we describe the structure of social benefits in water consumption and the benefit calculation.

Section 4 describes the definition of take-up and Section 5 presents the data. Section 6 presents the estimation methodology, results and robustness analysis. Section 7 includes the time of exposure results and section 8 concludes.

2. A simple model of take-up

Here we sketch a very simple model to guide our discussion on the role of information in shaping take-up of social benefits. Suppose a household derives utility from monetary income only:

$$(1)\ U_1 = U(Y),$$

where U_1 is the utility level in absence of welfare benefits, Y is income from all sources and U follows the standard assumptions (monotonic and quasi-concave). If a household participates in a welfare program then the utility, U_2 is:

$$(2)\ U_2 = U(Y + B - \phi),$$

where B represents the level of social benefits and ϕ stands for participation costs. This formulation assumes that the costs of collecting welfare benefits are monetary costs only, which is more likely in our context of low stigma cost.

The costs of participation in a welfare program may have both a fixed component and a variable component that varies with the benefits level:

$$(3)\ \phi = \phi_0 + \phi_1(B),$$

A household will decide to participate in the program as long as benefits are greater than costs regardless of the level of income. That result is particular to the way the costs affect utility. A household is likely to participate the greater the benefits level is in case where $\phi_0 > 0$ and $\phi_1 = 0$, which is the most simple case. The way we model the role of information here is by assuming that the cost of participation is a negative

function of potential exposure time to the program, $\phi_0 = \phi_0(t)$ where t reflects the time since the entitlement to social benefits. Thus, the decision to participate in a program is positively affected by exposure time.

3. Social benefit in water consumption

The social benefit that is the focus of this paper is provided to all households in Israel in terms of reduced price for one of the most basic goods – water. In this regard it is close to in-kind transfer and is similar in nature to food stamps. The pricing structure of water in Israel consists of three increasing block tariffs (IBT).[8] This structure accounts for both efficiency and equity considerations: the highest marginal price reflects efficiency, where it roughly covers the marginal cost, whereas the low price of the first block aims to allow relatively easy access to water consumption for poor people.[9]

In 2002, the price in the first block, applying to the first 96 cubic meters (hereinafter: m^3), was \$1.2/m^3 including a sewage surcharge. The price in the second block, for additional consumption up to 84 cubic meters, was \$1.5/m^3. The charge for all extra consumption was \$1.9/m^3.

This pricing structure has an additional feature. Households larger than four persons are entitled to an additional 36m^3 per person per year at a low rate.[10] This particular feature has been an integral part of IBT structure for more than 30 years, and is both universal and non-automatic. To receive this social benefit a household must report on household size. Poor families tend to be large, and this feature maintains that consideration in IBT pricing structure.

The social benefit in water consumption is associated with an extremely low administrative cost. To obtain the supplementary quantity of water at a low price, a

[8] Israel was one of the pioneers in using IBT Pricing structure. In the past twenty years there has been a global trend toward the use of IBT (OECD 1999).
[9] Note that IBT may not be an optimal pricing structure even after taking equity consideration into account (Bös, 1994).
[10] Households with irrigated lawns are allowed an additional 0.6m^3 per square meter per year, up to 300m^3, at a low price (excluding sewage surcharge).

household must fill out a very simple form: half a page requesting only the names and ID numbers of all household members, and the attached birth certificate of the newborn household member (see Appendix 1). A family automatically receives a birth certificate immediately after a baby is born. The form may be sent by regular mail (cost of a stamp) or via fax (cost of a phone call). Nevertheless, households may incur additional indirect administrative costs.

A household must report to the water utility provider every time a new member joins the household in order to get the supplementary quantity of water at a low price. This benefit may continue for years until a member leaves the household. According to the law, the benefit starts on the reporting date onward (no retroactive incidence). The social benefit takes effect right after reporting (i.e., the next billing period). There is no uncertainty regarding the outcome of the application process and in practice no rejections occurs.

Reporting the number of household members does not require sharing information regarding the household's economic conditions such as income, wealth or working status with the water utility officials – information that may be associated with psychological or social cost as in the case of income maintenance or unemployment benefits. This additional quantity is given to every household regardless of income. Thus, the universality of that social benefit reduces substantially, if not eliminates, the role of stigma in determining take-up rates.

3.1 Benefit calculation

We define social benefit in this paper as the difference between the current (virtual) water bill in the case of reporting on household size and virtual (current) water bill in the case of non-reporting. Unlike food stamps, the social benefit here depends on the level of (water) consumption. For example, the social benefit could even be zero if water consumption is low enough (equation 1).

As can be seen in equation (1), the calculated benefit depends non-linearly on the level of water consumption. This equation applies for a household larger than four persons without a garden.

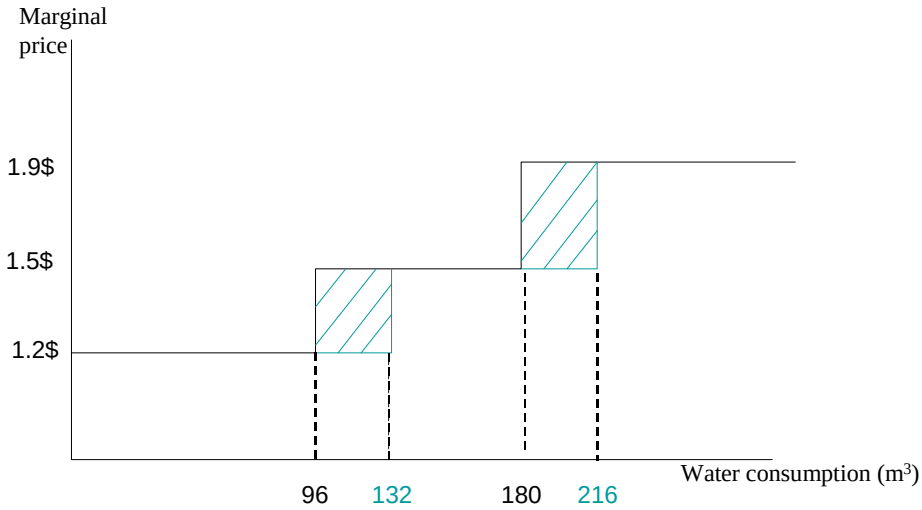
(1)

$$SB = \begin{cases} 0 & \text{if } C \le 96 \\ (C-96)(P_2 - P_1) & \text{if } 96 < C \le 180 \text{ and } X \le 96 \\ 36N(P_2 - P_1) & \text{if } 96 < C \le 180 \text{ and } X > 96 \\ 84(P_2 - P_1) + (C-180)(P_3 - P_1) & \text{if } C > 180 \text{ and } X \le 96 \\ (180 - X)(P_2 - P_1) + (C-180)(P_3 - P_1) & \text{if } C > 180 \text{ and } 96 < X \le 180 \\ 36N(P_3 - P_1) & \text{if } C > 180 \text{ and } X > 180 \end{cases}$$

Where SB denotes the yearly social benefit, C denotes the actual water consumption, N denotes the true number of persons above four and P_i denotes the marginal price of water at block i. X is defined as the difference between the actual water consumption and 36 multiply by N. Recall that every household is entitled to $96m^3$ of water at a low rate and an additional $84m^3$ at an intermediate rate.

The maximum yearly social benefit for an additional household member equals the difference between the highest and lowest price multiplied by the supplementary quantity (Figure 1). The maximum present value of social benefit *per person* is approximately $315. To obtain the maximum present value of social benefits for a household, that amount should be multiplied by the number of household members above four, for each year.

Figure 1: Yearly social benefit for a 5-member household



4. The definition of take-up

There are two alternative ways to define take-up in this paper. One way is to define take-up based on the reporting status of households. An alternative definition of take-up could be based on the presence of a positive social benefit in addition to reporting status. We employ only the first definition for reasons that are discussed below.

According to the first definition, the eligible population is divided into participants and non-participants. Participants are those households that have reported to "Hagihon" the same number of individuals as appears in the official files in the Ministry of the Interior. We assume here that the size of a household in the official files is also the actual size. These data files are used by the Israeli government for many purposes such as determining child allowance eligibility, elections, and drafting the relevant population into the army. Our database does not contain the household age structure and reporting date. Therefore, we do not know the length of participation.

The definition of take-up that we use, which is based on the household's reporting status, may include households that have ex-post zero social benefit due to a low level of water consumption in the current year and seemingly have no incentive to report. The weakness of using the first definition is quantitatively limited. Most households in our working data set are not in the lowest price category. More than 90 percent of five-member households is either at the intermediate or highest marginal price level.

Alternatively, we could have defined take-up in the following way: a reporting household that is also entitled to a positive social benefit is defined as a participant. Those households that are entitled to a positive social benefit and yet do not report would have been defined as non-participants.

However, a rational household should decide to report based on the expected present value of social benefits that could be different from the ex-post calculation of social benefit in the current year. Recall that the calculated social benefit depends on actual water consumption, which is uncertain. The alternative definition would overlook

households who had positive benefits in the past (and may have in the future) but have zero benefits at the current year.

A definition that is based on the calculated social benefit may be exposed to endogeneity. The dependent variable in this case – entitlement to a positive social benefit – is influenced by the household level of water consumption. Actual water consumption is clearly an endogenous variable and is associated with household characteristics. For example, poor households tend to have disproportional zero social benefits because of low level of water consumption while rich households more likely have a positive social benefit due to their high consumption level (Figure 2). Therefore, using take-up as a dependent variable based on this definition would introduce an endogeneity problem when household wealth indicators are used as explanatory variables.

The alternative definition may be exposed to an additional source of endogeneity. The endogeneity results from the negative relationship between reporting status and water price. Those households who do not report on their size face higher price compared to households who do report, holding everything else constant. The actual level of water consumption of participants may be higher. The calculated benefits for participants are biased to the extent that the elasticity of water consumption is negative (Dahan and Nisan, 2005). Thus, it generates a spurious positive correlation between the level of social benefits and the likelihood of reporting.

5. Data

The original data set we have covers all households in Jerusalem for the years 1999-2002.[11] Our data set comes from three main sources: "Hagihon," the only water utility in Jerusalem; the Municipality of Jerusalem; and the Israel Ministry of the Interior. Most of the data originate with the Municipality of Jerusalem and were merged with household water consumption data from "Hagihon" and household size at the end of each year from the Ministry of the Interior.

[11] We excluded observations for several reasons (commercial consumers, shared meters consumers, households larger than twelve individuals, households metered during part of the year and identification mismatch at different sources).

In the merged data set we have information on household size from two different sources: household size as reported to "Hagihon" and household size as documented in the Ministry of the Interior. This allows us to identify those households that are entitled to the social benefit but do not collect it. Thus, the use of these two data sources enables us to define eligibility and take-up in a relatively precise way.

As discussed previously, the eligible population is composed of households larger than four persons. The main focus is to analyze take-up rate patterns following a family expansion by one member of those households that are four persons or larger.[12] As a result, all households of four persons or less were excluded.

Our main working population consists of households that had expanded between 1999 and 2002 according to the official files (i.e., the Ministry of Interior). We constructed three different pools (A, B and C) that differ in time distance between the date of household expansion and the timing we examine the reporting status.

The first pool is composed of three panels – 1999-2000, 2000-2001 and 2001-2002 – where in the first pool we have information on the same household for two consecutive years.[13] This allows us to identify those households that had expanded *during* the second year and their reporting status at the *end* of the same year, a half year after the expansion on average. For the sake of conciseness, throughout the paper we use one year, two years and three years instead of a half year, a year and a half and two years and half, respectively.

The second pool includes two panels – 1999-2001 and 2000-2002. We follow the same household for three consecutive years. This covers those households that had expanded during the second year together with their reporting status at the end of the third year, a year and half after the expansion on average. The third pool, which is in fact a panel, is composed of households with information for four consecutive years (1999-2002). This covers those households that had expanded during the second year

[12] Dahan and Nisan (2006b) examine the take-up patterns of those who had expanded by two members.
[13] The data available to us consists of households who stay in the same apartment for the entire period in each panel.

together with their reporting status at the end of the fourth year, two years and half after the expansion on average. In both pools B and C those households that were expanded more than once were excluded because those households are entitled to double (or even triple) social benefits.

Table 1: Data by household type

Pool	**Type of household**					
	Total	Did not expand and other*	4 that became 5	5 that became 6 (All)	5 that became 6 and reported on the fifth member	5 that became 6 but did not report on the fifth member
Pool A (1 year)	**89,168**	**84,292**	**2,656**	**2,220**	**1,381**	**839**
1999-2000	28,085	26,473	853	759	514	245
2000-2001	29,828	28,240	893	695	427	268
2001-2002	31,255	29,579	910	766	440	326
Pool B (2 years)	**54,411**	**51,854**	**1,377**	**1,180**	**772**	**408**
1999-2001	26,305	25,017	678	610	420	190
2000-2002	28,106	26,837	699	570	352	218
Pool C (3 years) (1999-2002)	**24,992**	**23,062**	**421**	**377**	**263**	**114**

* Other includes expanded households of 6 or more and households that expanded by more than one member and households with an official size that is different compared to "Hagihon" (except for households of five members that are listed in "Hagihon" as four).

In all three pools, we focus on two household groups. In pool A the first group (hereinafter: the treated group) covers 2,220 households of five members that become six (Table 1). The second group of pool A consists of 2,656 four-member households that expanded by one member. These households are our control group.

The benefits level can be up to 8 percent of annual water expenditures but for most households it is around 5-6 percent (Figure 2). Table 2 presents take-up rates using the definition outlined above. The take-up rate among households of five members that become six is around 52 percent two years after the expansion which is well within the range of take-up rates in social programs in OECD countries (Hernanz et al, 2004).

The take-up rate of five-member-households (that become six) is higher compared to the take-up rate among households of four members that become five (41 percent). The differences are similar regardless of the pool we use.

Time distance matters as is evident in Table 2. The take-up rate after two years is almost twice as much as the rate after one year. Clearly, it takes time to collect the social benefit associated with reporting. The take-up after three years is just slightly higher compared to two years. For example, the take-up rate among households of five members that become six is 28 percent after one year, 52 percent after two years and 55 percent three years after the expansion. Notice that these take-up rates are different from the take-up rates of the same household for various time distances, as shown in Table 7. We will return to this issue later.

Table 2: Take-up rates for different time distances

Pool	**Take-up rates**			
	4 that become 5 (Control Group)	5 that become 6 (all) Treated Group	5 that become 6 and reported on the fifth member	5 that become 6 but did not report on the fifth member
Pool A (1 year)	**0.20**	**0.28**	**0.36**	**0.13**
1999-2000	0.21	0.31	0.39	0.15
2000-2001	0.22	0.29	0.37	0.15
2001-2002	0.18	0.23	0.33	0.11
Pool B (2 years)	**0.41**	**0.52**	**0.63**	**0.32**
1999-2001	0.42	0.53	0.64	0.29
2000-2002	0.41	0.51	0.61	0.34
Pool C (3 years) (1999-2002)	**0.45**	**0.55**	**0.64**	**0.35**

6. Estimation

6.1 Methodology

In order to examine the role of information we distinguish between two separate groups of households. These two groups are identical in the following sense: they

both expanded by one member at the beginning of the respective period. The first group consists of five members who had expanded to six and for which the information regarding social benefits is valuable prior to the current household expansion. Those households had the monetary incentive to search for that information.

The second type is a four-member household that expanded to five members. Those households did not have the incentive to report according to the rules of the program. It should be recalled that the price structure of water is the same for every household up to four members regardless of household size. Therefore, there is no social benefit associated with additional members as long as the household is four members or less.

The first type of households had the incentive to search for information regarding the social benefits associated with reporting before the current household expansion, while for the second type the information was immaterial in the past and became relevant with the current expansion of the household.[14] We use the information gap between these two groups and the current expansion of a household by one member as a quasi-natural experiment.

The treated group is composed of two sub-groups of households: households of five members who expanded to six and had reported in the past on the fifth member and households of five members who expanded to six but had not reported on the fifth member. Those households that had reported on the fifth member apparently were in possession of the information on social benefits in water consumption before the current household expansion.

Those households are presumably informed but we cannot rule out that this group may be a selection of households that had faced lower administrative cost in the previous household expansion. Likewise the behavior of households that did not report on the fifth member may be consistent with two conflicting hypotheses: those households were truly uninformed and consequently they had not collected their

[14] A rational household may have the incentive to search for information even before becoming eligible (i.e., before the anticipated fifth member joins the household) but it is true for both the treated and control groups.

social benefits following the previous household expansion or they were fully informed but decided not to report because of cost-benefit considerations.

Thus, using households that had reported on the fifth member as an alternative treated group may be exposed to a self-selection problem. The risk of self-selection problem is important to extent that the cost-benefits considerations that dictate the decision to report in the past on the fifth member is correlated with the conditions following the current household expansion. We use instead all five-member households who became six as our treated group.

Our main goal is to test whether the treated group reacts differently in terms of taking-up their social benefits compared to the control group, when a household of either type is expanded by one member. Based on the model sketched in Section 2 we hypothesize that those households which for a longer period of time had the incentive to collect information would tend to have higher take-up rates

The treated and control households all face the same administrative process to report on household expansion. Thus, the direct administrative cost is the same for all households. This direct cost is relatively low: a household needs to complete a half-page form and send it via mail or fax to the water utility. Yet, households may face additional indirect administrative costs.

A potential additional factor that may affect a household's decision to collect social benefits is stigma cost. In our context, stigma cost should not play an important role due to the universal nature of this social benefit. Every household, regardless of its income or wealth, is entitled to this social benefit (i.e., it is not means-tested).

A household above four members is entitled to an additional $36m^3$ of water at a low rate for each additional member regardless of its size. Therefore, the treated and control groups are entitled to the same social benefits (for a given price) as a result of the current expansion.

However, a five-member household that did not report on the fifth member is entitled to exactly double quantity of water at a low rate compared to the control group.

Seemingly, the coefficient might be overestimated because part of the treated group is entitled to higher social benefits. The actual bias should not be quantitatively important because the take-up rates of those households are even lower than the control group (Table 2).

Table 3: Descriptive statistics - a comparison of treated and control groups*

				The share of households			
			Apartment Size	**Orthodox Jews**	**Arabs**	**Below poverty line**	**at (actual) price B or C**
One year	5 turn 6	Obs	1,668	2,220	2,220	2,220	2,220
		Mean	76.8	0.49	0.08	0.02	0.95
		Std	23.7	0.5	0.27	0.14	0.22
	4 turn 5	Obs	1,894	2,656	2,656	2,656	2,656
		Mean	73.2	0.36	0.06	0.01	0.97
		Std	21.6	0.48	0.23	0.12	0.18
		Means' Difference	3.56	0.13	0.02	0.01	-0.02
		T statistic	**4.65**	**9.23**	**2.79**	1.27	**-3.03**
Two Years	5 turn 6	Obs	938	1,180	1,180	1,180	1,180
		Mean	78.1	0.48	0.08	0.02	0.95
		Std	23.7	0.5	0.27	0.13	0.22
	4 turn 5	Obs	1,083	1,377	1,377	1,377	1,377
		Mean	74.6	0.35	0.06	0.014	0.96
		Std	22.7	0.48	0.23	0.12	0.19
		Means' Difference	3.50	0.12	0.03	0.004	0.01
		T statistic	**3.38**	**6.44**	**2.52**	0.81	1.71
Three Years	5 turn 6	Obs	308	377	377	377	377
		Mean	81.89	0.31	0.11	0.03	0.95
		Std	25.18	0.46	0.32	0.16	0.21
	4 turn 5	Obs	350	421	421	421	421
		Mean	77.14	0.21	0.08	0.02	0.97
		Std	23.84	0.41	0.27	0.15	0.17
		Means' Difference	4.75	0.1	0.04	0.01	-0.01
		T statistic	**2.48**	**3.26**	1.7	0.25	-1.43

*The data in this table refer to the year before household expansion.

By its construction, the treated group is larger by one member than the control group, and that may generate differences in household characteristics. Table 3 presents household characteristics for both groups. It shows that the treated group has larger apartment size but it is smaller in terms of apartment size per capita. It is unclear which group is wealthier and it depends on the assumed economics of scale in housing. The share of Orthodox Jews is higher (statistically significant) among the treated group because they tend to have more children. For the other three

characteristics: Arab population, below poverty line indicator and price level, the difference is not always significant. In the estimation part we will control for these characteristics.

6.2 The estimated models

We estimate two alternative models: the first model does not control for any household characteristics while the other model addresses a potential effect of the differences in household characteristics and a year effect.

First model:

(2) $y_i = a + bD_i + \varepsilon_i$ **,**

and second model:

(3) $y_i = \alpha + \beta \mathbf{x}_i + \gamma D_i + \delta_j t_j + \varepsilon_i$,

where, y_i is a dummy variable that is equal to one for a household that had reported and zero otherwise. $\mathbf{x}_i$ denotes a vector of household characteristics in the respective period and D_i represents the dummy variable for the treatment effect. D_i is a binary variable that equals 1 for a treated household and zero for a control household. We also control for a year effect, t_j where there are two separate year effects in the case of three panels, one year effect in the case of two panels and no year effect in the case of three years panel.

The vector **x** includes an array of household characteristics that may affect reporting behavior. There are three types of variables: wealth indicators (apartment size, garden size and poverty indicator), social network indicators (Orthodox Jews and Arabs), language barrier (Arabs) and the virtual marginal price of water faced by a household which represents the level of social benefits.

The net effect of wealth on take-up is uncertain. According to Moffitt (1983), take-up rates should be falling as wealth rises due to lower marginal utility. In contrast, take-up rates might not be affected directly by wealth level as long as the cost of

participation is modeled the way presented in Section 2. In addition, wealth may affect take-up rates indirectly (through the connection between education and wealth) because we do not control for education in our regressions. A wealthier household may incur lower cost of collecting and processing information given the positive relations between wealth and education.

According to the recent literature, we hypothesize that a household that belongs to a social network is more likely to be informed and as a result would have higher probability of reporting (Bertrand et al, 2000).[15] In Jerusalem there are two large distinct ethnic groups that may be classified as social networks: Orthodox Jews and Arabs. An Orthodox Jewish household is defined as such if it is located in an Orthodox neighborhood as classified in the Jerusalem master plan. Similarly, a household is defined as Arab if it lives in an Arab neighborhood as classified in the Jerusalem master plan. Each one of these two groups maintains close personal relations internally and has little social connection with the rest of the population. In fact, the Arab population has almost no social connection with the Jewish population.

In addition, an Arab household may face a language barrier. Although the criteria for an additional quantity of water at a low rate are outlined both in Hebrew and Arabic on the back page of every water bill (though the font size is extremely small), the application form is available in Hebrew only. Part of the Arab population in East Jerusalem who affiliate themselves with the Palestinian Authority tend to minimize the frequency of contact with Israeli official authorities. Therefore, they may be less exposed to information regarding their entitlement to social benefits. The Arab population may face lower information cost due to the social network but at the same time incurs higher information cost due to language barrier. Thus, the net effect must be examined empirically.

As noted before, both the treated and control groups are entitled to the same additional quantity of water at a low rate following the current expansion by one member. Yet, the actual level of social benefits may still differ depending on the

[15] See also Aizer and Currie (2004), Duflo and Saez (2003), and Borjas and Hilton (1996).

actual level of water consumption that determines the marginal price paid by a household.

We use the virtual marginal price as a proxy to estimate the effect of the level of social benefits on take-up rates. A marginal virtual price is defined as the marginal price that would have been faced by a household given its actual water consumption in the event of no reporting on household size.

In general, households that reported on the current expansion face a lower (actual) marginal price as compared to households that did not report. This may affect their actual water consumption to the extent that price elasticity of water demand is negative. The virtual price might be higher for a reporting household and as a result the coefficient might be biased downward. The estimated price elasticity of water demand is relatively low which subdues that bias (Dahan and Nisan, 2005).

6.3 Results

We run an OLS regression with and without control variables for household characteristics. The outcomes of interest in our case are limited dependent variables. However, as noted in Angrist (2001), the problem of causal inference for these variables is not fundamentally different from continuous outcomes. If there are no covariates or the covariates are sparse and discrete, linear models are no less appropriate than other types of dependent variables. The OLS coefficients of dummy variables are easy to interpret: the units are percentage points of take-up rates.[16]

To estimate the role of information as captured by our treatment dummy variable we use three different pools, according to the time gap between the year of household expansion and the date the reporting on that expansion is checked. In the first pool the dependent variable is defined based on the reporting status of a household a year after the expansion. The second and third pools are based on reporting status two and three years after the household expansion, respectively.

[16] Table 5 in the appendix presents Logit regressions and the general picture is the same.

Table 4 reports the coefficients and t statistics for the two estimated models. In a regression without any control variable, the coefficient of the treated group is around 7 percentage points even when the reporting status is detected at the end of the expansion year. The magnitude of the treatment effect is higher when the reporting status is checked at the end of the second or third year since the household expansion. The estimated treatment effect ranges between 8 to 10 percentage points. Those households for which the information was relevant for a longer period of time indeed have higher take-up rate.

We saw earlier that the treated and control groups are not completely identical in their characteristics. The second estimated model appearing in Table 4 shows that the effect of treatment is only slightly lower, even after controlling for various explanatory variables and a year effect. The magnitude of the treatment effect is closer to the first estimated model when reporting status is examined at the end of the second or third year since the household expansion.

Given the nature of the pool, we control for a year effect in the second estimated model. In the one-year pool (which is composed of three panels of two years) we found that the 2001-2002 dummy is negative and significant while the other dummy is insignificant. The year effect is not significant in the two-year pool and by construction there is no year effect in the three-year pool.

As discussed above, the Arab household dummy variable reflects two conflicting forces in terms of information cost. The negative sign of this coefficient and its magnitude is consistent with the hypothesis that language barrier has much more influence than social network. The Arab coefficient is -0.12 when reporting status is detected at the end of the household expansion year. This coefficient is twice as large when reporting status is checked at the end of the second or third year since household expansion.

The Orthodox Jewish dummy variable represents a lower information cost due to better social network. As expected, the coefficient is positive but it is statistically significant in two of the three cases.

As expected, the level of social benefits positively affects take-up rates as implied by the virtual price coefficient. A higher virtual price implies higher social benefits that induce a household to collect information and report on household size. In general, the coefficient of virtual price B (the intermediate price) is significantly positive compared to the lowest price, and it is lower than the coefficient of virtual price C (the highest price), although this is not always the case. This result is in line with one of the most robust findings in the literature on the reasons for low take-up rates.

In general, all wealth indicators (apartment size by quintiles, garden size and poverty indicator[17]) are insignificant. This finding is consistent with the theoretical prediction of the model presented in Section 2. However it can be also in line with the notion that take-up rates are influenced by wealth level due to lower marginal utility (Moffitt, 1983) but it is canceled out by the effect of lower information cost associated with higher level wealth, to the extent that wealth and education are correlated.

Table 6 presents a robustness test where we exclude the two ethnic groups from the pools. As can be seen, the treatment effect of a potentially informed household is now even larger. The take-up rate of households of five who had expanded to six is 13 percentage points higher when reporting status is checked at the end of the second or third year since household expansion. This estimated effect is not sensitive to the inclusion of control variables.

The coefficients estimated in both models imply a quantitatively large effect. The estimated take-up rate of the treated group is around 25 percent higher as compared to the control group. This supports the idea that information plays a major role in shaping take-up rates.

7. Time of exposure

In this section we indirectly explore the role of information. Previously, we used reporting status after one year, two years and three years for all households. In

[17] In this paper, a household is considered below the poverty line if it is entitled to a municipal tax deduction. This tax deduction is means-tested and is indirectly related to the formal poverty line in Israel.

contrast, here we exploit the panel nature of our data to follow reporting status for the same household over time.

Reporting status after three years should not be different from that after two years or after one year, unless there has been a change in the cost or benefits of applying for social benefits. The actual benefits can be changed as a result of changes in water consumption patterns (which in itself may change as a result of the household's economic conditions). The way we capture changes in benefits is by controlling for the virtual price.

As explained earlier, stigma cost should not play a role in our case. Thus, changes in take-up rates could be the result of changes in information and administrative costs. This test is based on households who stay in the same apartment for the entire period. Thus, we in fact control for the distance from the applications center, which is a natural proxy for administrative cost (Warlick, 1982). The direct administrative cost is the same regardless of the application timing. Yet, the administrative cost may still change as a result of changes in variables that are not observable.

The fourth possible explanation for changes in take-up rates of the same household over time is information. Since information regarding social benefits is continuously floating around, the time of exposure since household expansion may play a role. To estimate the information effect we run OLS regressions with fixed effects for households and time effects. In addition we control for the virtual price.

Interestingly, the quantitative effect of information using this exercise is similar to some of the results found in the previous Section. Column 1 in Table 8 shows that the take-up rate is 8 percentage points higher after three years compared to the take-up rate after two years. That result is not sensitive to the inclusion of an interaction variable between time and treatment effects (column 2 in Table 8). As expected, the take-up rate in the first year, which is also the expansion year, is much smaller in comparison to that after two years. The coefficient of the first year is artificially large due to the fact that those households who had expanded near the end of the first year collect their social benefits only during the second year.

We interpret the magnitude of the time coefficient as another strong indication of the importance of information in determining take-up patterns. We cannot completely rule out an upward bias of this coefficient due to the potential omission of administrative factors. However, it is unlikely to relate the time trend in take-up rates we found to changes in the administrative cost.

To conclude, we show the importance of the length of exposure to relevant information, which complements the evidence we have in Section 6 on the importance of information in determining take-up patterns.

8. Conclusions

This paper addresses two challenges faced in the previous literature. First, the two reliable sources on eligibility and take-up allow us to estimate the role of information based on a precise definition of take-up. Second, the negligible role of stigma cost in our context further helps to isolate the effect of information. This paper provides estimates on the importance of information in determining take-up patterns of social benefits using a quasi-natural experiment strategy.

We show that the take-up rate of the treated households is substantially higher compared to a control group of households. The estimated information effect in terms of take-up rates is in the range of 8 and 13 percentage points. This result is robust for different time distances and the inclusion of various household characteristics. In addition, we found that variables that are directly associated with information cost such as social network indicators (Orthodox Jews and Arabs) and language barrier (Arabs) have significant influence on take-up rates.

We also employ the panel nature of our dataset to examine the take-up rates of the same household over time following the eligibility for social benefits. We found that the take-up rate three years after eligibility is 8 percentage points higher as compared to two years after entitlement and 30 percentage points higher relative to the end of the entitlement year.

Low take-up rate should be a cause of concern for policy makers because it undermines the policy goal. A natural policy implication of our findings is that lowering of information barriers is likely to have a significant effect on take-up of social benefits, especially for certain population groups.

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Table 4: OLS estimates of information effect on take-up rates
(The dependent variable: reporting status in year t)

	After One Year		**After Two Years**		**After Three Years**	
Intercept	**0.20** (24.56)	**0.11** (3.79)	**0.41** (30.97)	**0.19** (3.91)	**0.45** (18.59)	**0.22** (2.44)
5 turn 6 (All)	**0.07** (6.14)	**0.05** (4.13)	**0.10** (5.36)	**0.08** (4.39)	**0.10** (2.84)	**0.09** (2.70)
00-01		-0.01 (-0.38)				
01-02		**-0.05** (-3.46)				
00-02				-0.02 (-1.39)		
Arabs		**-0.12** (-4.94)		**-0.27** (-6.96)		**-0.23** (-3.80)
Orthodox Jews		**0.11** (8.63)		**0.16** (7.90)		0.07 (1.80)
Virtual Price B		**0.07** (3.05)		**0.17** (3.97)		**0.21** (2.46)
Virtual Price C		**0.12** (4.82)		**0.19** (4.57)		**0.18** (2.25)
Apartment Size by Quintiles*:						
Second Quintile		-0.02 (-1.13)		0.00 (0.22)		0.02 (0.42)
Third Quintile		0.01 (0.72)		0.03 (1.14)		0.03 (0.62)
Fourth Quintile		0.01 (0.29)		0.02 (0.71)		0.07 (1.27)
Top Quintile		-0.01 (-0.48)		0.01 (0.28)		0.10 (1.69)
Apartment Size Unknown		**-0.07** (-3.95)		-0.03 (-0.99)		-0.08 (-1.37)
Poverty Indicator		-0.00 (-0.01)		-0.04 (-0.71)		0.00 (0.00)
Garden Owners		0.04 (1.62)		0.04 (0.92)		0.06 (0.87)
Garden Size		0.00 (0.73)		0.00 (1.65)		0.00 (0.56)
Observations	**4,876**	**4,876**	**2,557**	**2,557**	**798**	**798**

t statistics are in parentheses
* The bottom quintile is omitted.

Table 5: Logit estimates of information effect on take-up rates
(Dependent variable: reporting status in year t)

	After One Year		**After Two Years**		**After Three Years**	
Intercept	**-1.38** (810.51)	**-2.10** (100.62)	**-0.35** (40.38)	**-1.45** (36.14)	**-0.20** (3.98)	-1.38 (8.76)
Treated (5 turn 6 all)	**0.41** (37.24)	**0.28** (16.03)	**0.43** (28.37)	**0.37** (18.79)	**0.40** (7.99)	**0.41** (7.29)
00-01		-0.03 (0.14)				
01-02		**-0.30** (12.05)				
00-02				-0.11 (1.89)		
Arabs		**-1.34** (28.66)		**-1.62** (43.28)		**-1.15** (14.09)
Orthodox Jews		**0.62** (68.74)		**0.68** (58.06)		0.31 (3.05)
Virtual Price B		**0.65** (12.03)		**0.90** (16.93)		**1.15** (6.59)
Virtual Price C		**0.91** (23.61)		**1.00** (21.53)		**1.04** (5.60)
Apartment Size by Quintiles*:						
Second Quintile		-0.15 (1.43)		0.03 (0.05)		0.10 (0.16)
Third Quintile		0.06 (0.25)		0.17 (1.31)		0.15 (0.31)
Fourth Quintile		0.01 (0.01)		0.10 (0.43)		0.31 (1.39)
Top Quintile		-0.08 (0.39)		0.04 (0.06)		0.43 (2.50)
Apartment Size Unknown		**-0.50** (18.09)		-0.15 (1.04)		-0.39 (2.13)
Poverty Indicator		0.01 (0.00)		-0.21 (0.46)		0.01 (0.00)
Garden Owners		0.23 (1.90)		0.15 (0.57)		0.24 (0.59)
Garden Size		0.00 (1.56)		0.00 (3.82)		0.00 (0.54)
Observations	**4,876**	**4,876**	**2,557**	**2,557**	**798**	**798**

Wald Chi-Square statistics are in parentheses
Bottom quintile is omitted.

Table 6: OLS estimates of information effect on take-up rates – non-Orthodox Jews only
(Dependent variable: reporting status in year t)

	After One Year		**After Two Years**		**After Three Years**	
Intercept	**0.16** (15.91)	0.04 (1.04)	**0.36** (21.25)	**0.15** (2.11)	**0.45** (15.72)	0.19 (1.50)
Treated (5 turn 6 all)	**0.09** (5.73)	**0.08** (5.14)	**0.13** (4.59)	**0.13** (4.53)	**0.13** (2.97)	**0.13** (2.93)
00-01		-0.01 (-0.48)				
01-02		**-0.04** (-2.31)				
00-02				-0.05 (-2.01)		
Virtual Price B		**0.13** (3.72)		**0.23** (3.55)		**0.30** (2.49)
Virtual Price C		**0.15** (4.42)		**0.21** (3.41)		**0.23** (1.96)
Apartment Size by Quintiles*:						
Second Quintile		0.00 (0.14)		0.07 (1.28)		0.03 (0.33)
Third Quintile		0.03 (1.01)		0.08 (1.60)		0.03 (0.30)
Fourth Quintile		0.01 (0.03)		-0.01 (-0.19)		0.02 (0.23)
Top Quintile		-0.01 (-0.38)		0.03 (0.65)		0.08 (1.01)
Apartment Size Unknown		-0.04 (-1.51)		-0.01 (-0.28)		-0.12 (-1.49)
Poverty Indicator		0.00 (0.09)		-0.08 (-0.96)		-0.03 (-0.21)
Garden Owners		**0.09** (2.58)		0.04 (0.66)		0.02 (0.28)
Garden Size		-0.00 (-0.10)		0.00 (0.99)		0.00 (0.68)
Observations	**2,492**	**2,492**	**1,342**	**1,342**	**515**	**515**

t statistics are in parentheses
* The bottom quintile is omitted.

Table 7: Take-up rates for different time distances for the same household

Time Length since eligibility date	**Take-up rates**			
	4 that become 5 (Control Group)	5 that become 6 and reported on the fifth member (Treated Group)	5 that become 6 but did not report on the fifth member	All 5 that become 6
One year	**0.16**	**0.32**	**0.11**	**0.26**
Two years	**0.37**	**0.56**	**0.26**	**0.47**
Three years	**0.45**	**0.64**	**0.35**	**0.55**

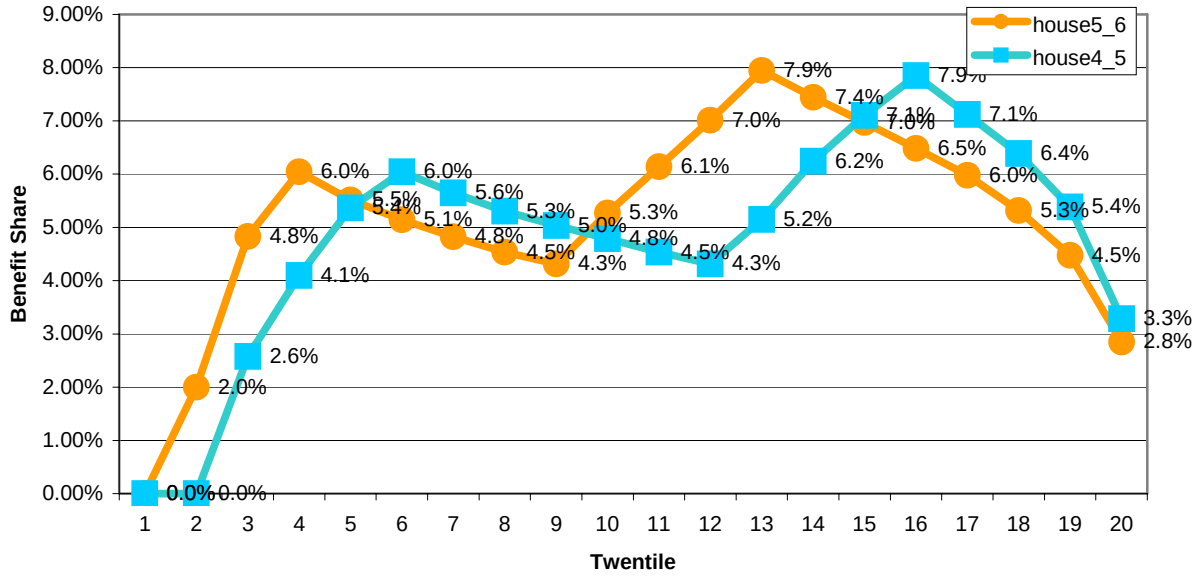
Table 8: OLS estimates of time exposure effect on take-up
(Dependent variable: reporting status in year t)

	Coefficient (t values)	**Coefficient (t values)**
1 Year	-0.21 (-15.35)	-0.20 (-11.79)
3 Years	0.08 (5.82)	0.08 (4.92)
1 year*Treated		-0.04 (-1.36)
3 years*Treated		-0.01 (-0.27)
Price B	-0.01 (-0.26)	-0.01 (-0.22)
Price C	0.04 (0.74)	0.04 (0.78)
Number of Observations	**798**	**798**

t statistic are in parentheses

Figure 2: The Level of Benefits by Twentiles

(as a share of annual water expenditure before household expansion)



Appendix 1: Application Form

"Hagihon" – Jerusalem Water Company

Declaration – Number of household members*

First Name_____ Surname_____

I.D. Number_____

I declare that residing in my apartment, located on _____ Street, number_____

there are _____ members. (Less than age 18)

For children under age 18, please complete the following:

Name_____ I.D. _____

Name_____ I.D. _____

Name_____ I.D. _____

Name_____ I.D. _____

Total number of persons residing in the apartment:_____

Below is my signature. I confirm that all details are true, and will provide notification of any changes in these details.

Signature_____ Date_____

**Translated to English by the authors*

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