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How Effective Homelessness Prevention Impacts the Length of Shelter Spells

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Abstract

Homelessness prevention programs intervene with households apparently in imminent danger of becoming homeless, and try to keep them housed. If they are at least partially successful, how do they change the average shelter spell of households actually becoming homeless? We use data from 2003 to 2008 for Homebase, a New York City homelessness prevention program that studies have found to be effective in reducing shelter entries. Homebase made no difference in average shelter spells at the community level. This result, like many results about shelter spell length, is not easy to reconcile with the idea that shelter spell length is a reflection of the seriousness of underlying problems.

Keywords

Homelessness prevention; shelter spells; family homelessness

1. Introduction

Consider a program that takes some families who are about to become homeless, gives them assistance with their immediate problems, and successfully diverts some of them from becoming homeless. Will this program lengthen the average stay of families who do enter homeless shelters, shorten it, or leave it unchanged? This paper is the first to investigate this question. Our answer is that the average stay does not change.

The question is important for two reasons. The more practical reason is the evaluation of homelessness prevention programs. These programs are the subject of rising interest among homeless advocates and service providers (see, e.g., National Alliance to End Homelessness 2013), and received a large appropriation ($1.5 billion over three years) in the American Recovery and Reinvestment Act of 2009 (ARRA). If homelessness prevention programs prevent homelessness only among families who would have stayed in shelters for just a few days, they look quite different than they would if they prevent homelessness among families who would have stayed in shelters many months.

Second, understanding the effect of homelessness prevention programs on shelter stays can yield some insight into what homelessness is like and how it should be modeled. A

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traditional view, for instance, considers homelessness to be intimately linked to a long-lasting pathology of some kind (substance abuse or mental illness, for instance), and so sees spells of homelessness as manifestations of the underlying long-lasting pathology. In this view, which is theoretical, shorter spells are manifestations of the weaker cases of the underlying pathology and longer spells are manifestations of more serious cases. Homelessness prevention programs do not make expensive investments in their participants, and so in that sense, probably cannot eliminate severe pathologies. The traditional view thus implies that if homelessness prevention programs work at all, they work with the weakest cases, and so they should lengthen the average shelter stay for those who enter shelters.

An alternative view is that families are buffeted by stochastic shocks, both positive and negative, and become homeless when they suffer enough negative shocks to be left with a very low level of resources (financial capital, obviously, but also health, human, and social capital). By supplying a favorable shock to a family with severely depleted resources, a homelessness prevention program can therefore sometimes avert or postpone homelessness. (Suppose families whose resources fall to level 0 become homeless, and every period each family receives a shock of (+1) or (−1), with equal probability. Then half of families at resource level 1 will become homeless in the next period. A homelessness prevention program identifies families at level 1, and increases their resource level to 2. As a result, some families who would have become homeless never become homeless (families destined to receive a negative shock next period and then mainly nonnegative shocks after that without long strings of mainly negative shocks), and others will have their homeless spells postponed. See O'Flaherty 2012 for a technical discussion.)

In this view, homeless families are similarly buffeted by stochastic shocks. They leave homelessness when the cumulative effect of those shocks is to raise their resources high enough that they become conventionally housed again. How long families stay homeless then depends on the process that generates the stochastic shocks. An obvious story about the distribution of shocks is that it depends only on the family's resource level (or is totally invariant); that is, the process is Markovian, or "memory- less." If this is the case, then the history of how a family reached homelessness—in particular, whether it received homelessness prevention services or not—would have no influence on the distribution of homeless spells.

But other theories about the shocks that families receive when homeless can lead to alternative predictions about homeless spells. In particular, suppose that the probability of some favorable shocks (getting a job, winning the lottery) depends on calendar time, not the resources a family has or its time in shelter. Then the process is not Markovian. Suppose further that homelessness prevention programs delay shelter entry for some of their participants, but do not affect the timing or probability of these favorable shocks. Then by postponing the start of spells whose ends do not depend on when they start, the homelessness prevention program will shorten spells instead of averting them.

Therefore studying the effect of prevention programs on spell length can give us useful (though obviously not definitive) insight into how homelessness works.

Our data come from New York City, and concern families, not single individuals unaccompanied by children[1]. The New York state constitution (as interpreted in consent decrees that apply only to New York City) guarantees a right-to-shelter, and the New York City Department of Homeless Services (DHS) operates a large shelter system for families. Because of the consent decrees and local law, New York City is required to provide single

[1]However, pregnant women count as families.

units with private bath and kitchen facilities for families with housing emergencies. The cost is about $100 per family per day (New York City Independent Budget Office, 2012). On September 24, 2013 DHS housed 10,465 families with children and 1,830 families without children.

In November 2004, DHS inaugurated a homelessness prevention program called Homebase. We study that program from its inception through November 2008. DHS did not start Homebase in the entire city at the same time. Instead, Homebase started in 2004 in only a few neighborhoods, and then expanded citywide in two waves in 2007 and 2008. The phased start-up provides a quasi-natural experiment that allowed Messeri, O'Flaherty and Goodman (2012) to estimate the effect of Homebase on shelter entries. Their estimate is that it reduced shelter entries by between 10 and 20 for every hundred families it served officially. Rolston et al. (2013) in a small controlled experiment of a later version of Homebase also concluded that it reduced shelter entries among participants, and that it reduced the unconditional average of days in shelter among participants (where this average includes both families who entered shelters and those who did not).

The phased start-up also allows us to estimate the effect on shelter exits in this paper.

For purposes of this paper, "homelessness" means "sheltered homelessness." We cannot observe street homelessness. But it is extremely unlikely that the shelter entries that Homebase averted among families were converted to street homelessness. Street homelessness among families in New York City during this period was extremely rare, and there is no reason to believe it was correlated with Homebase activities. Homebase participants were free to enter shelters, retained their right-to-shelter, and were never encouraged to become street homeless. The goal of Homebase was to make being housed more attractive and feasible, not to make entering shelters more difficult.

2. Theory

Homebase could affect exits in three different ways. All of these ways are indirect, because Homebase provides no services designed to help individual families leave shelters sooner or to encourage them to stay longer; it is entirely devoted to keeping them out of shelters. Two of these ways would make spells longer, and one would make spells shorter.

Selection in the traditional view of homelessness would make spells longer. If Homebase were more successful in averting homelessness for families with less serious problems than for families with more serious problems, and if homeless spells were longer for families with more serious problems, then Homebase would be more successful in averting short than long spells. The average spell that starts when Homebase is operating would therefore be longer than the average spell that starts when Homebase is not operating.

Spillovers might also make spells longer. For instance, if Homebase participants stay in apartments they would otherwise vacate, fewer apartments will be available for shelter residents to move into. When Homebase is operating, then, families may take longer to exit from shelter stays.

Postponement could make spells shorter. Suppose some families leave shelters when an exogenous favorable event occurs—winning the lottery, finding a good job, getting married—and the hazard of the good event does not depend on whether the family is in a shelter or not, and rises over time. Then if Homebase delays shelter entry, it reduces the expected interval between shelter entry and the favorable event. The average shelter spell for families who entered after Homebase started would be shorter.

Postponement would not have an effect, of course, if homelessness were part of a Markovian process, since fixed dates would not matter.

Notice that each of these three effects tells us to look at a somewhat different set of exit hazards. Postponement tells us to look at all days in spells that began after Homebase had been operating a few months. Selection tells us to look at those days, as well as all days in all the other spells that began when Homebase was operating, too. The spillover story, by contrast, tells us to look at days when Homebase was operating, not the spells in which they are embedded.

To keep the analysis simple, we will concentrate on selection and postponement, and not test directly for spillovers. To the extent that spillovers are geographically diffuse, moreover, our dataset may not let us test for them at all (we have no way of knowing whether a family that originally came from Brooklyn would not have left the shelter system and moved to the Bronx if Homebase had not been operating).

3. Literature review

A voluminous literature indicates that men, minorities, and younger adults are more likely to enter shelters (US Department of Housing and Urban Development 2010). However, when it comes to length of spell given homelessness, the evidence is generally cloudy on correlates of spell length. If lengths of homeless spells were linked to the problems that made people homeless, then these groups would have longer homeless spells. The literature does not support this implication.

For example, no clear pattern emerges for mental illness. Piliavin et al. (1993) found that pre- homeless psychiatric hospitalization was associated with shorter spells; McBride et al. (1998) found that mentally ill women stayed in shelters longer; while Culhane and Kuhn (1998) and Allgood and Warren (2003) found no significant association between mental illness and shelter stay length.

No clear pattern has emerged on race, ethnicity, or gender. Wong et al. (1997) and Poulin (2007) found that whites exit shelters sooner than other racial and ethnic groups, but Piliavin et al. (1993), Culhane and Kuhn (1998), and Allgood and Warren (2003) found that race and ethnicity have no significant effect on shelter stay. Some studies (Piliavin et al. 1996, Culhane and Kuhn 1998 for New York, Allgood and Warren 2003) found that men stay in shelters longer; other studies (Culhane and Kuhn 1998 for Philadelphia and Poulin 2007 for New York) found that women stay longer; and Piliavin et al. (1993) found that gender did not make a significant difference.

The one result that has been found in several papers and not counter-indicated by any other paper is that older people stay in shelters longer (Piliavin et al. 1993, Culhane and Kuhn 1998 for New York City, Wong et al. 1997, Allgood and Warren 2003, Poulin 2007). One study found that larger families stay longer (Wong et al. 1997), and no study has found otherwise.

The literature on homelessness prevention is more meager. Most of the studies lack plausible identification strategies and look only at shelter entries (for reviews, see Apicello 2010 and Apicello et al. 2012). They do not look at the length of shelter stays. Messeri, O'Flaherty, and Goodman (2012) studied the community impact of Homebase and concluded that it reduced the number of families entering shelters from the CDs in which it operated. Since they were concerned with the net impact on shelter entries, they could not say whether the families who did not enter because of Homebase were Homebase

participants or not. Rolston et al. (2013), as we have noted, also looked at Homebase with a clear identification strategy for finding the *unconditional* average stay..

4. Homebase and Family Shelters—The Institutional Setting

DHS established Homebase in order to divert families with treatable emergencies from entering shelters. DHS enters into contracts with nonprofit organizations to operate Homebase centers in different neighborhoods. People who think they are in danger of becoming homeless self-select and apply for assistance at these centers.[2] During the period we study, eligibility was restricted to families who met an income criterion and who were experiencing "treatable emergencies" in the view of program staff. Ineligible families often received counseling and referrals, however. Staff at these centers used their judgment to provide a wide array of services to eligible families in order to keep those families housed. Services included family and landlord mediation, legal assistance, short-term financial assistance, mental health and substance abuse services, child care, and job search assistance. We do not have information on the distribution of these services. Families stay in the Homebase caseload an average of six months, and few repeat.

DHS began Homebase in 2004 in six community districts (CDs) [3]. These six community districts were spread throughout the city geographically (of the five boroughs, only Staten Island was unrepresented). They were not randomly selected, but were concentrated among the CDs from which most shelter entrants came. For most of the next three years, Homebase operated only in these six CDs. In July 2007, Homebase expanded to encompass 31 more CDs, and in January 2008 it added the remaining 22 CDs. Since January 2008, Homebase has operated citywide.

Homebase could alter the identities of families entering shelters in many ways. It could, for instance, substitute non-participants for participants if it diverted scarce resources to participants. Or Homebase could reduce entries by non-participants if participants shared helpful information with their friends and neighbors, or if their examples of non-entry in the face of crises changed the motivations of friends and neighbors, or increased the stigma of shelter entry.

Homebase could also change when families enter shelters. Service in a particular month might delay entries that otherwise would have happened in that month. But it might also avert or delay entries that would have happened in later months. Messeri, O'Flaherty, and Goodman (2012) tested for these effects in several ways, and concluded that the net postponement effect was small. (For instance, if Homebase mainly postponed entries, then experienced CDs would appear to be much more ineffective than inexperienced ones, and they do not.)

Throughout this period, DHS operated a variety of programs intended to speed exits by subsidizing post-shelter housing. Before December 2004, DHS operated the Emergency Assistance Rehousing Program (EARP), under which homeless families received Section 8 vouchers and landlords renting to them received bonuses. Homeless families also received priority referrals to public housing. Both programs ended in December 2004. They were replaced by Housing Stability Plus (HSP), which in turn was replaced in 2007 by the Advantage Rental Assistance program (New York City Independent Budget Office 2012). (The Advantage program ended in April 2011.)

[2]At: various time, Homebase has engaged in outreach efforts, including occasionally knocking on doors in neighborhoods from which many shelter entries have emanated.

[3]For planning and administrative purposes, New York City is divided into 59 community districts, with an average population of about 135,000 each.

These programs differed in eligibility, in priorities within the shelter system, and in the structure and value of the post-shelter subsidy. None, however, made any distinctions based on the CD of a family's last address. Hence we will simply distinguish between exits under these post-shelter subsidy programs—called "placements"—and all other exits.

5. Data

DHS gave us de-identified information on every spell of family homelessness that began between January 2003 and November 2008. For each spell we know the composition of the family (number of adults and number of children), the CD of the family's last address before shelter entry, and the date of entry. For completed spells, we know whether they are placements or not. Since our data are de- identified, we do not know how many families experience multiple spells of homelessness. In general, our analysis would be sharper if we had additional information about families like age, race, ethnicity, an prior homeless experience, but we do not.

We study spells of family homelessness that began between January 2003 and November 2008 inclusive. There were 45,111 such spells. We censor spells at December 31, 2008. Of all the spells, 37,711 were completed before December 31, 2008 with an average length of 274 days.

Of the uncensored spells (that is, the spells for which we observe exits from the shelter system), about three quarters---28,012---ended in placements. Uncensored spells that ended in placements averaged 307 days; other uncensored spells averaged 181 days. The difference in length, though large, is not statistically significant.

6. Methods

We use Cox proportional hazard methods. Because we want to consider placement and non-placement exits separately, we estimate separate hazard rates for each mode of shelter exit. The goal is to find whether families who entered the system when Homebase was operating had a smaller or larger hazard of non-placement exit than other families.

Knowing the CDs in which Homebase was supposed to be operating in any month is not sufficient to know whether a family was exposed to Homebase services. Before Homebase went citywide in January 1, 2008 about 5 percent of Homebase cases we observe were from CDs that were not supposed to be eligible for services in the month that these cases were opened. In our model, following Messeri, O'Flaherty, and Goodman (2012), we will accommodate this non-compliance by specifying two operational variables. One is a CD-level indicator for "informal operations" set equal to one in the first month in which a family from that CD receives Homebase services, and in all subsequent months. The other, measuring "official" operations takes a value of one in the month that DHS opens Homebase there, and in all subsequent months. (When a CD is in full operation, it is thus both informally and officially operating.)

In addition, we say that a CD is "experienced" after it has been in official operation for more than two months. Experienced Homebase offices may be more proficient at their jobs, and the communities may know their methods better. Experienced CDs may also have different experiences with entries that were delayed rather than averted. Our choice of two months is somewhat arbitrary, but we do not know how long either of these processes should take, and two months gives a decent opportunity to see whether either process is operating on net.

Since Homebase operation is not a simple concept, we assign each entering family values for these three dummy variables. First, P_{fc}= 1 if and only if Homebase was operating

informally when family f from CD c entered the shelter system. Second, H_{fc}= 1 if and only if Homebase was operating officially when family f entered the shelter system. Finally, R_{fc}= 1 if and only if Homebase was officially operating and experienced when family f entered the shelter system.

Specifically, let $\lambda_j(d, f, m, c)$ denote the hazard that in calendar month *m*, family *f*, which came from community district *c*, will leave the shelter system after *d* days, and the exit will be type *j* (placement or non-placement). Our basic equation is

$$\lambda_j(d,f,m,c)=\lambda_0^j(d)\exp\{[\beta X_{fc}+\gamma_c+\delta_m]+L\left[\tilde{\beta}X_{fc}+\tilde{\gamma_c}+\tilde{\delta_m}\right]\} \quad (1)$$

Here $\lambda_0^j(d)$ is the baseline hazard for type *j* exits; the Cox method does not estimate this directly. The vector X_{fc} is a vector of characteristics of family *f* and community district *c*. In particular

$$\beta X_{fc}=\beta_1 A_f+\beta_2 K_f+\beta_3 H_{fc}+\beta_4 P_{fc}+\beta_5 R_{fc}.$$

Here A_f is the (demeaned) number of adults in family *f*, and K_f is the (demeaned) number of children in family *f*.

Continuing with (1), γ_c and δ_m are dummies for the CD and the current month respectively, and *L* is a dummy variable equal to one if and only if the exit is a placement. Thus the independent variables are fully interacted with placement type.

We estimate this equation stratified by placement type, following Bakoyannis and Touloumi (2013).

The coefficients we are most interested in are β_3, β_4, β_5. These coefficients indicate how the non-placement hazard changes for families who entered the shelter system when and where Homebase was operating. We are also interested in $(\beta_3+\tilde{\beta_3})$, $(\beta_4+\tilde{\beta_4})$, and $(\beta_5+\tilde{\beta_5})$. These sums indicate how the placement hazard changes for families who entered the shelter system when and where Homebase was operating.

7. Summary statistics

Table 1 provides summary statistics. This table includes censored as well as uncensored spells. Spells that started during Homebase CD months appear to be shorter than spells that did not. This applies no matter how Homebase operation is defined. There also does not appear to be much difference between official operation and experienced operation. But no differences are statistically significant.

We also show how Homebase was ramped up. For most of the period, Homebase operated in only six community districts; only in 2007 and 2008 do the community district-months and affected entries expand. Families who entered when Homebase was operating were slightly less likely to be placed, but the difference is small in any year.

8. Results

Homebase appears to make little difference, especially for placements.

Table 2 provides the results from estimating equation (1). It confirms the general picture that Homebase makes no significant difference to exits. Families whose last place of residence was in CDs receiving Homebase services seem to leave slightly faster, but the difference is tiny and not statistically significant. Consistent with Wong et al. (1997) family size has large and significant effects: For non-placement exits, larger families stay longer. For placement exits, families with more children are placed sooner than families with fewer children; the effect is small but significant.

Selection and postponement may both be operating, but if they are, they are cancelling each other out. There is reason to suspect, however, that neither is operating. If operating in the hypothesized direction, postponement would increase the non-placement hazard rate for CDs that have been operating several months, relative to CDs that have been operating less time. So postponement implies that the effect of experience on non-placement exits should be positive and large. Selection has no implication for a change in effect after several months. So if both postponement and selection are strong and offsetting, the coefficient on experience for non-placements should be positive and significant. But it is not: the point estimate is .004(95% C.I.=−.083, .091). Moreover, recall that Messeri, O'Flaherty, and Goodman (2012) found net postponement was unlikely to be large.

Figure 1 shows the hazard functions for families that entered when Homebase was officially operating and when it was not, and for spells that ended in placements and those that did not. This is the first picture of which we are aware of hazard functions for shelter exits, with many variables held constant. The most obvious reaction to figure 1 is that the hazard functions for Homebase and the hazard functions for families who entered when Homebase was not operating are almost identical. This is the confirmation of the result in table 2. The major difference is for non-placement exits, where the hazard rate for Homebase families starts rising sooner than that for non-Homebase families. The empirical work leads us to suspect that this difference is not significant. All of the exit hazards are upward-sloping; the longer a family stays in the shelter system the more likely it is to leave. This is especially true for placements, which probably reflects administrative policies, directly or indirectly. Whether the hazard function would be upward-sloping for non-placement exits if the hazard function for placements were not is an important question which we cannot study.

9. Conclusion

We have tried to answer two questions, one practical and one theoretical.

The practical answer is fairly clear: a homelessness prevention program operated at the same intensity as Homebase had been will not noticeably alter shelter spell lengths. For purposes of cost- benefit analysis, expenditure forecasting, and shelter planning, a city starting a program like Homebase need not worry much about longer or shorter shelter stays. Focusing on entries is sufficient.

The theoretical answer is less clear. We cannot reject the null hypothesis that Homebase had no effect on shelter spells, but non-rejection, of course, is not the same as acceptance. Even though our sample is large and our confidence intervals are wrapped tightly around zero, we cannot be sure that the shelter spells that Homebase averted were not considerably different from the spells it did not avert. The problem (aside from the counterfactual difficulty of asking how long a spell that did not happen would have been if it had happened) is that Homebase operated on a small scale relative to the flow of families entering the shelter system. During CD-months of full operation, the Messeri, O'Flaherty, and Goodman (2012) estimates imply that about 9–16 percent of shelter entries that would have otherwise occurred were averted. Thus even if the exit hazard rate for averted spells were, say, 50

percent higher or lower than the rate for non-averted spells, fully operating Homebase CD-months would still come within our confidence intervals.

A much larger and more intense Homebase program, if it still averted entries at the rate that Messeri. O'Flaherty, and Goodman (2012) observed, could thus provide a stronger test for the Markovian view of shelter spells. But we do not know how well a larger and more intense program would avert entries. No one knows how large the population at any moment is that could be diverted from shelter entry by Homebase-style interventions, or how much of that population could be attracted and identified. Given the size and effectiveness of existing homelessness prevention programs, the difference in length between spells averted and spells not averted, if it exists at all, is not big enough to make any practical difference at the community level.

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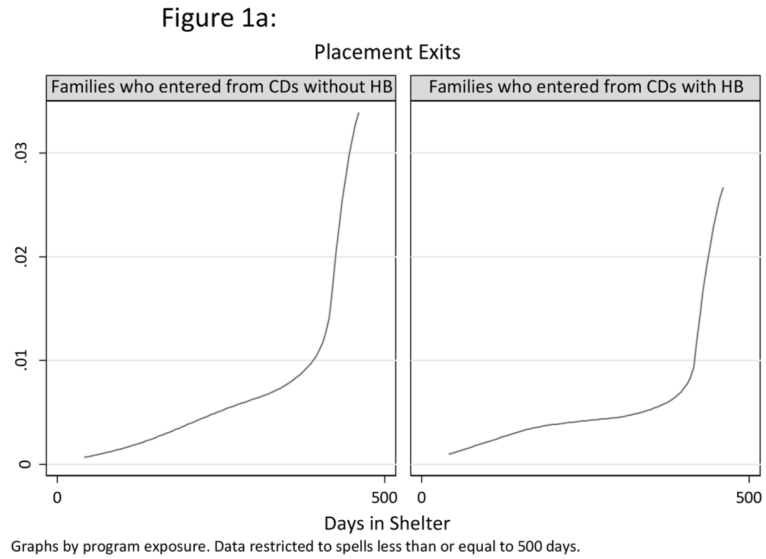

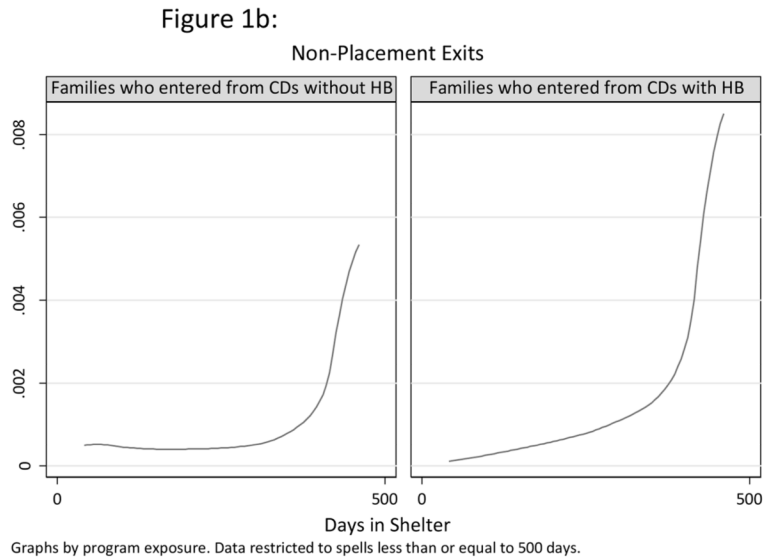

Figure 1.
Figure 1a: Smoothed Hazard Estimates
Figure 1b: Smoothed Hazard Estimates

Table 1

Summary Statistics Entries between January 2003 to November 2008

	Sample Period			
	All CD-Months	**Unofficial Homebase Operations**	**Official Homebase Operations**	**Experienced Homebase Operations**
Total CD-Months	4,189	2,223	1,063	945
Average Months per CD	71 --	37.7 (11.9)	18.0 (10.9)	16.0 (10.9)
Average Shelter Entries per CD-Month	10.8 (11.6)	13.4 (12.9)	15.9 (14.7)	16.5 (14.9)
Total Spells (Total Observations)				
Overall	45,111	29,654	16,849	15,596
Non-Placement Exits, Only	16,807	14,018	10,291	9,707
Placement Exits, Only	28,304	15,636	6,558	5,889
Average Spell Length				
Overall	273.7 (237.8)	236.8 (205.7)	190.1 (175.3)	185.9 (174.9)
Non-Placement Exits, Only	217.6 (249.4)	201.1 (215.1)	160.6 (165.1)	155.4 (162.6)
Placement Exits, Only	306.9 (224.0)	268.7 (191.4)	236.3 (180.8)	236.2 (182.5)
Family Size Variables				
Average Number of Adults	1.3 (0.5)	1.4 (0.5)	1.4 (0.5)	1.4 (0.5)
Average Number of Children	1.7 (1.2)	1.7 (1.3)	1.6 (1.3)	1.6 (1.2)

Homebase Rollout over Time and Space

	2003	**2004**	**2005**	**2006**	**2007**	**2008**
CD-months with Homebase	0	12	72	72	258	649
CD-months without Homebase	708	696	636	636	450	0
Entries	6481	6726	5951	8116	8190	9647
in Homebase-active CD-months	0	199	1396	1915	3692	9647
in non-Homebase CD-months	6481	6527	4555	6201	4498	0
Percent of entries ending in placement						
in Homebase-active CD-months	--	81%	79%	72%	55%	19%
in non-Homebase CD-months	85%	82%	79%	73%	61%	--
Percent of entries censored						
in Homebase-active CD-months	--	0.0%	1.8%	4.0%	16.3%	52.5%

Homebase Rollout over Time and Space

	2003	2004	2005	2006	2007	2008
in non-Homebase CD-months	0.3%	0.5%	1.0%	4.4%	13.0%	--

Notes: There are 59 community districts (CDs) in the sample. CD-months with zero entries are included in calculations. Spells with no observed exit are censored at December 31, 2008. We exclude spells shorter than 1 day. Average statistics presented across entries unless otherwise noted. Standard deviations in parentheses. Annual data for 2008 excludes December.

Table 2

Effects of Homebase Coverage on Exit Rates, Cox Proportional Hazard Model

	Non-placement effect β	$\tilde{\beta}$	Placement effect $(\beta+\tilde{\beta})$
Coverage Indicators			
Official operation	0.024 (−0.067, 0.116)	−0.057 (−0.202, 0.088)	−0.033 (−0.134, 0.069)
Unofficial operation	−0.005 (−0.093, 0.083)	0.024 (−0.076, 0.123)	0.019 (−0.037, 0.075)
Experienced	0.004 (−0.084, 0.091)	0.018 (−0.124, 0.160)	0.022 (−0.080, 0.123)
Sum	0.023 (−0.073, 0.119)	−0.015 (−0.131, 0.100)	0.008 (−0.063, 0.079)
Number of Adults	−0.356 (−0.380, −0.332)	0.078 (0.045, 0.111)	−0.278 (−0.299, −0.256)
Number of Children	−0.139 (−0.150, −0.127)	0.204 (0.189, 0.220)	0.066 (0.057, 0.075)
Log pseudolikelihood			−426,316.65
N			*90,222*

Note: Family size variables adjusted for mean zero. 95% confidence intervals in parentheses.