cross-correlations between nonresponsive neurons were not altered (Fig. 4E, left), they were increased between photostimulated neurons and remained stable the next day (Fig. 4E, right). Thus, optogenetic activation of identified neurons enhanced their local functional connections for at least 1 day (Fig. 4F).

Recalled ensembles shared similar characteristics—such as number of neurons and spatial distribution—with ongoing ensembles (fig. S7), but the mean distance between active neurons was shorter (fig. S7D), which indicates that the effect of the photostimulation is local. Recalled ensembles often had neurons that did not belong to ongoing ensembles (fig. S7, D and E), demonstrating that recalled ensembles are indeed novel and not just dormant preexisting ensembles. However, given that cortical connections are likely not in a tabula rasa state, we expect that imprinted ensembles may recruit segments of physiologically relevant circuit motifs (Fig. 4F).

Previously, electrical or optogenetic stimulation (*25*) has been used to show that coactivation of neuronal groups can produce physiologically relevant behaviors (*13*, *26*). Here, we show the possibility of training individual neurons to build artificial neuronal ensembles (*13*), which then become spontaneously active (Fig. 4D, right). Our results are consistent with the finding that neurons responding to similar visual stimuli have a higher interconnectivity (*27*), as well as with the similarity between visually evoked and spontaneous ensembles (*9*). In both cases, recurrent coactivation of a neuronal group would enhance functional connectivity, imprinting ensembles into the circuit.

More than 60 years ago, Hebb proposed that repeated coactivation of a group of neurons might create a memory trace through enhancement of synaptic connections (*12*). Because of technical limitations, this hypothesis has been difficult to test with single-cell resolution in awake animals. By combining novel imaging and photostimulation techniques (*14*, *15*) and analytical tools (*19*), our work can be interpreted as a confirmation of the Hebbian postulate and as a demonstration that cortical microcircuits can perform pattern completion.

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SUPPLEMENTARY MATERIALS

www.sciencemag.org/content/353/6300/691/suppl/DC1
Materials and Methods
Figs. S1 to S7
References (*28*–*40*)

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ECONOMIC POLICY

The impact of homelessness prevention programs on homelessness

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Despite the prevalence of temporary financial assistance programs for those facing imminent homelessness, there is little evidence of their impact. Using data from Chicago from 2010 to 2012 (n = 4448), we demonstrate that the volatile nature of funding availability leads to good-as-random variation in the allocation of resources to individuals seeking assistance. To estimate impacts, we compare families that call when funds are available with those who call when they are not. We find that those calling when funding is available are 76% less likely to enter a homeless shelter. The per-person cost of averting homelessness through financial assistance is estimated as $10,300 and would be much less with better targeting of benefits to lower-income callers. The estimated benefits, not including many health benefits, exceed $20,000.

Over 2 million people experience homelessness each year in the United States (*1*). Historically, the primary approach to combating homelessness has been to provide emergency shelters or transitional housing services to those who are already homeless. More recently, policy-makers have increased their focus on homelessness prevention efforts. One of the most common prevention strategies is to provide temporary financial assistance to people facing eviction in order to keep them in their residences. In the United States, 93% of households live in an area that has such a program, and these programs receive over 15 million calls a year (*2*). Despite the prevalence of these efforts, there is little evidence about the extent to which they actually prevent homelessness (*3*, *4*).

Here we examine the effectiveness of temporary financial assistance by using data from the Homelessness Prevention Call Center (HPCC) in Chicago, which processes about 75,000 calls annually. Chicago residents at risk of becoming homeless can call 311 to request temporary financial assistance for rent, security deposits, or utility bills. These callers are routed to the HPCC, which is a centralized processing center that screens callers for eligibility and connects eligible callers with local funding agencies.

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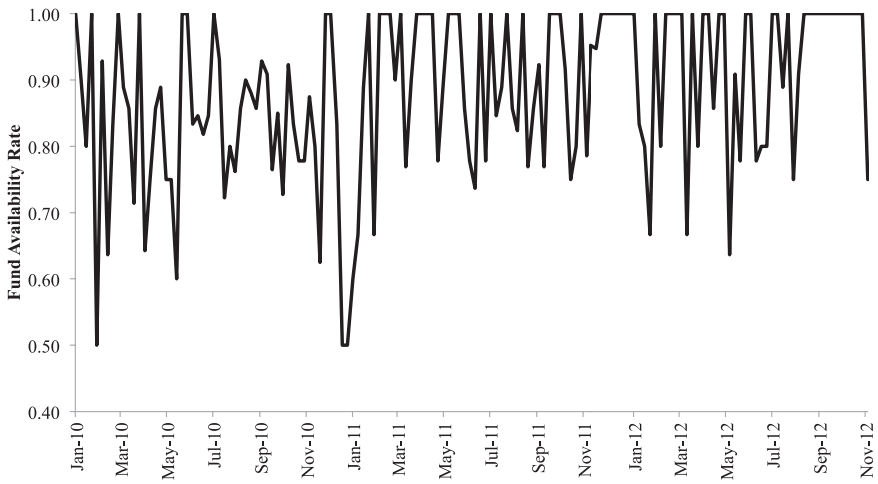

Fig. 1. Fund availability rate by week. Shown is the fraction of eligible callers in our homogenous sample that were referred to financial assistance each week from 2010 to 2012. The homogeneous sample includes all eligible callers who were seeking rent assistance with need amounts between $301 and $900, were nonveterans, were not receiving housing subsidies, were not requesting more than 1 month's rent, had a social security number, had a family income below twice the poverty line, and were not homeowners (n = 1431).

At the beginning of each call to the HPCC, information and referral specialists collect detailed information to determine whether the client is eligible for financial assistance. The specialist refers those who are not eligible for financial assistance to nonfinancial support services. General eligibility is based on four criteria: (i) The client must be able to demonstrate self-sufficiency after they receive assistance; (ii) the client must have an eligible crisis (e.g., job loss or medical emergency) that has led to the need for assistance; (iii) the client must face imminent risk of homelessness or utility shutoff; and (iv) the current crisis must be solvable by the financial assistance.

For eligible callers, the specialist then looks to see whether there is a delegate agency that is offering funds at that time. These agencies have additional fund-specific restrictions beyond those imposed by the general eligibility rules, such as the request type (rent, mortgage, security deposits, or utility bills), the need amount, veteran status (a few funds are restricted to veterans), receipt of housing subsidies (some funds will not assist those who receive Section 8 vouchers), the number of months of unpaid rent (some funds will pay for only 1 month), whether the caller's income is more than twice the poverty line, whether the caller has a valid social security number, and whether the caller is a homeowner.

Not all eligible callers are referred to funds. Funding for financial assistance varies unpredictably over time. The availability of funding on any given day depends on many factors. For example, some delegate agencies require that callers meet with a financial counselor before funds are dispersed, and the caller will not be referred if an interview slot is not available at the time of the call. For some agencies, there are only a fixed number of appointments available each week or month, but new interview slots might become available through cancellations. Variation in funding can also occur as a result of inconsistent and unpredictable funding streams from local or state programs.

The variation in the availability of funding is evident in Fig. 1, which shows the fund availability rate (the fraction of eligible callers that are referred for funding) at the HPCC by week from 2010 through 2012. To ensure that this variation is not due to changes over time in caller characteristics, we focused on a subset of callers, which we call the "homogeneous" subsample, whose characteristics affecting access to specific funds were the same. For example, all callers in this subsample were requesting rent assistance, and all had need amounts between $301 and $900. As Fig. 1 shows, there was considerable variation in the likelihood that such callers were referred for assistance. For some weeks, all eligible callers with these characteristics were referred to funds. But for most weeks, only a subset of these callers was referred, and for two of these weeks only half were referred. We used this variation in fund availability to determine the impact of financial assistance on homelessness. One concern could be that this variation may be correlated with caller characteristics that directly influence homelessness because certain types of callers may have information about fund availability. However, from the perspective of the eligible client, the availability of funds is difficult to predict (supplementary materials, section 1.3, and tables S1 and S2).

The empirical analysis for this study relies on administrative data about callers seeking temporary financial assistance that are routed to the HPCC (supplementary materials, materials and methods). The main sample used for this study was drawn from all calls to the HPCC from 20 January 2010 to 4 December 2012. We restricted our sample to requests for rent or security deposits because those who call the HPCC seeking assistance with utility bills are highly unlikely to enter a homeless shelter (supplementary materials, section 1.2), which is our primary outcome of interest. We also restricted the sample to the first call to the HPCC from an individual. It is common for some callers to contact the HPCC multiple times. Our concern was that subsequent calls may not be exogenous: The characteristics associated with these calls may be correlated with both the availability of funds and the likelihood of entering a homeless shelter. Our main sample for analysis thus included 4448 calls—3574 for rent and 874 for security deposits—and 58.2% of these callers were referred to funds (table S3).

If the availability of funds were purely random, one could determine the impact of financial assistance by comparing outcomes for eligible individuals who call the HPCC when funds are available with outcomes for those who call when funds are not available. In practice, however, not all eligible callers have the same likelihood of being referred to funds because of the fund-specific restrictions mentioned above. For example, callers who requested between $301 and $900 of assistance were more likely to call when funds were available because more funds cater to these need amounts (table S4). Therefore, for our balancing tests of baseline characteristics, we controlled for the factors that are known to affect fund availability by estimating regressions of individual characteristics on a dummy variable for fund availability and these factors. To account for patterns in call volume, we also included seasonal controls (supplementary materials, section 1.4).

The results from our balancing tests are reported in Table 1 for both the homogeneous subsample and our main sample of eligible callers seeking assistance with rent or security deposits. In the first and third columns, we present the means for observable characteristics for our control group—callers who are not referred for funding. In the second and fourth columns, we report the difference in means between the treatment and control groups, as measured by the coefficient on the availability-of-funds indicator. For the homogeneous subsample, no additional controls were included in the regression, so this estimate is the actual raw difference in means. For the main sample, this estimate is from a regression that included the other controls mentioned above. The results indicate that those who call for rent or security deposit assistance when funding is not available are very similar to those who call when funding is available. In nearly all cases, we failed to reject the null hypothesis that the characteristics are the same at $P < 0.05$ (*5*). For the characteristics where we did reject the null hypothesis,

the differences in means were small. As an example, for the main sample, those calling when funds were available made $52 less in monthly income than those calling when funds were not available, a 4% difference in income. The statistically significant differences indicate that those calling when funds are available are more likely to enter a shelter, independent of the availability of funds, which would bias against our main finding that funding reduces homelessness. Additional evidence of the balance in characteristics is available in table S5.

To assess homelessness outcomes, we linked the HPCC data to administrative data from the Homeless Management Information System (HMIS) about entries into and exits from housing facilities for the homeless in Chicago. These data cover roughly two-thirds of homeless beds in Chicago (supplementary materials, section 1.1, and table S6). In the results presented below, we focus on these key measures of homelessness: whether a caller enters a homeless shelter within 3 or 6 months after the call and the number of days spent in a shelter during the first 6 months after the call.

We estimated the intention-to-treat (ITT) effect of being referred to financial assistance on the likelihood that an individual enters a homeless shelter (supplementary materials, materials and methods). These ITT estimates capture the difference in homelessness outcomes between those who call when funds are available and those who call when funds are unavailable. They measure the impact of fund availability, not actual receipt of financial assistance (*6*, *7*).

The results of our analysis of the impact of fund availability on homelessness are presented in Table 2 for our main sample, the homogeneous subsample, and other subsamples (estimates of the coefficients on other covariates are given in table S8, and alternative specifications that demonstrate robustness are given in tables S7 and S9). For our main sample, fund availability led to a 1.4-percentage-point decrease in the probability of entering a shelter within 3 months of the call and a 1.6-percentage point decrease in the probability of entering a shelter within 6 months (*8*). Both of these estimates are statistically significant at $P < 0.05$. These effects represent an 88% decline in the likelihood of becoming homeless after 3 months and a 76% decline in the likelihood after 6 months. The results for days spent in a shelter are consistent with those for whether a caller enters a shelter. Calling when funds were available reduced the time spent in a shelter over the next 6 months by 2.6 days (or 84%), and nearly all of the effect on days spent in a shelter can be accounted for by whether a caller entered a shelter rather than by the length of the stay. This suggests that fund availability has only a small effect on the length of time one spends in a shelter, conditional on entry. That the impact of fund availability falls predominantly on whether or not one enters a shelter is not surprising, given that the intervention is designed to keep people in their own residences.

The impact of calling when funds are available is comparable for the homogeneous subsample (Table 2). The magnitudes of the effect are larger (in absolute value), but in percentage terms, the impact is slightly smaller, and these estimates are not statistically significant at $P < 0.05$. We found a similar impact for the subsamples of callers seeking rent or security deposit assistance; in nearly all of these cases, the effect of fund availability is statistically significant at $P < 0.05$ (Table 2). For example, for those seeking rent assistance, fund availability led to a 1.5-percentage-point decrease in the probability of entering a shelter within 6 months, a decline of 71%.

To examine how the impact changes over time since the call, we re-estimated the ITT effect with the dependent variable in the model being whether the caller checked into a shelter within τ months after the call, where τ ranges from 1 to 12. Figure 2 reports the main point estimates from these specifications along with the 95% confidence interval for our main sample. These results show that calling when funds are available has an immediate impact on homelessness, which is not surprising given that most eligible callers face imminent risk of homelessness: Most have already been served an eviction notice. The effect within 2 months is slightly larger than the effect within 1 month, but the magnitude of the effect changes little at longer follow-up periods. The estimated impact within 12 months (a decline of 1.7 percentage points) is similar to the estimated impact within 2 months (a decline of 1.5 percentage points). In all cases, the estimates are statistically significant, and the results are similar for the subsamples of callers seeking assistance with rent (fig. S1) or security deposits (fig. S2).

Analyses for other subgroups reveal considerable heterogeneity in the effects of financial assistance. As shown in Table 2, the effects differ noticeably by income. For those seeking rent assistance whose incomes were below or equal to the median income for the sample (below ~90% of the federal poverty line), calling when funds were available reduced the likelihood of entering a shelter within 6 months by 2.2 percentage points (88%). There is little evidence of an effect of financial assistance on homelessness for those with above-median income among eligible callers. Other subgroups that appear more likely to benefit from financial assistance include individuals (single adults), males, callers younger than 30, and those calling in the winter (table S10) (*9*).

A common criticism of programs that aim to prevent homelessness through financial assistance is that emergency funds tend to be poorly targeted; resources go to those who would not end up homeless even in absence of the assistance (*4*, *10*, *11*). Thus, the temporary financial assistance may crowd out other resources for avoiding homelessness. The evidence from our sample of callers to the HPCC is consistent with this argument. Many HPCC callers who were not referred to financial assistance found a way to avoid homelessness even though they were facing eviction from their residence at the time of the call (*12*). Despite this evidence of considerable crowd-out, given the high cost of homelessness to individuals and society, even a small overall reduction in homelessness may be cost-beneficial. Thus, it is important to know whether the value to individuals and society of the unmeasured

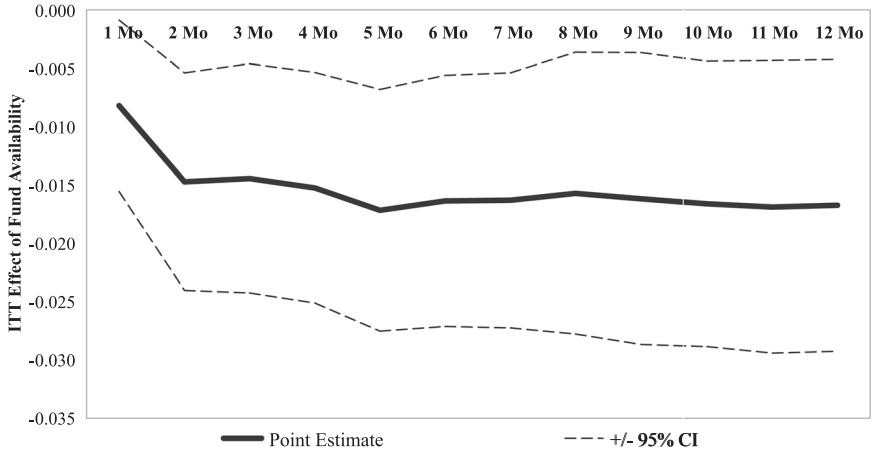

Fig. 2. ITT effects of fund availability on shelter admittance 1 to 12 months after the call. We estimated 12 separate regressions of shelter entry 1 to 12 months (mo) after the call on an availability-of-funds indicator, demographic characteristics of the caller, ZIP code–level characteristics, fund-specific restriction variables, and seasonal control variables. The solid line plots the coefficients on the availability-of-funds indicator in these regressions (e.g., −0.015 denotes a 1.5-percentage-point decline in shelter entry); estimates of the coefficients on other covariates are given in table S8. The dashed lines denote the 95% confidence interval (CI) calculated using standard errors clustered at the ZIP code level. The estimates for the effect 1 to 6 months after the call are based on the main sample (n = 4448). Beyond 6 months, we lose some information because we do not have data about shelter entry beyond 6 months for all callers. Thus, the sample size decreases with each month beyond 6 months, with the 12-month shelter entry estimates based on a sample of 3892 callers.

benefits of homelessness reduction is large enough to justify the documented costs.

The cost of reducing homelessness through rent assistance includes the operating costs of the call center and delegate agencies, as well as the cost of the financial assistance. Altogether, we estimate these costs to be about $720 per caller referred. Given our main estimates of the effect of calling the HPCC when funding is available, we calculate the cost per homeless spell averted to be about $10,300 (supplementary materials, section 2.4). This cost per homeless spell averted would be lower if the program were better targeted toward those for whom financial assistance is more effective at reducing homelessness. Our results indicate that the per-person cost of averting a new case of homelessness among very low-income families would be $6800, 35% less than the per-person cost among all eligible callers (*13*–*15*).

The benefits of this intervention result from avoiding a number of costs associated with a person becoming homeless, including the cost of providing shelter and other housing services, the cost to society of addressing other needs that may arise as a result of homelessness, and other private costs to the individual. One study estimates the average per-person cost of providing shelter for individuals who become homeless for the first time at about $2400 in 2012 dollars (*16*), and another study estimates the per-person public costs (including health care, police and incarceration, and welfare programs such as food stamps) of the homeless to be $5148 annually (*17*, *18*).

Perhaps the most substantial benefits of homelessness prevention stem from reduced

Table 1. Mean characteristics and differences in means between treatment and control groups. Shown are the mean characteristics for the control groups and the regression-adjusted difference between groups. To test balance, each characteristic was regressed on an availability-of-funds indicator, fund-specific restriction variables, and seasonal control variables, and standard errors were clustered at the ZIP code level. Caller characteristics come from HPCC records from 20 January 2010 to 4 December 2012 for eligible, first-time callers; ZIP code–level characteristics come from the 2010–2012 American Community Surveys and are standardized to have a mean of 0 and a variance of 1. Shelter inhabitancy in the past 18 months comes from HMIS data for homeless shelters in Chicago.

Dependent variable	Homogeneous subsample		Main sample	
	Control group mean	Coefficient on availability of funds	Control group mean	Coefficient on availability of funds
Female	0.874	−0.100**	0.794	−0.032*
White, non-Hispanic	0.066	0.022	0.062	0.011
Black, non-Hispanic	0.906	−0.030	0.908	−0.017
Other, non-Hispanic	0.033	0.002	0.032	0.005
Hispanic	0.083	0.001	0.069	−0.001
Age	39.258	−1.078	39.129	−0.838*
Number of adults in caller's household	1.341	0.038	1.353	−0.019
Number of minors in caller's household	1.357	−0.012	1.389	−0.083
Standardized percentage in ZIP code with high school degree	−0.061	0.001	0.014	−0.034
Standardized labor force participation rate in ZIP code	−0.060	0.032	−0.015	−0.003
Standardized unemployment rate in ZIP code	0.017	0.005	0.014	0.005
Standardized median age in ZIP code	−0.163	0.150**	−0.006	0.018
Standardized monthly housing cost in ZIP code (in thousands)	0.034	−0.080	0.009	−0.040
Standardized median household income in ZIP code (in thousands)	−0.071	0.026	−0.004	−0.021
Standardized fraction black in ZIP code	0.046	−0.048	0.024	−0.012
Standardized fraction white in ZIP code	−0.042	0.027	−0.014	−0.010
Standardized fraction other races in ZIP code	−0.040	0.076	−0.037	0.051
Applying because of benefit loss	0.132	0.023	0.115	−0.010
Applying because of inability to pay bills	0.022	−0.015	0.013	−0.008**
Applying because exiting shared housing	0.044	0.013	0.089	0.012
Applying to flee abuse	0.016	−0.012	0.016	−0.002
Applying because of job loss	0.407	−0.030	0.301	0.012
Monthly income (in thousands)	1.175	0.080**	1.210	−0.052**
Receiving Supplemental Nutrition Assistance Program benefits	0.637	−0.057	0.633	−0.019
Receiving child support	0.061	−0.010	0.060	−0.017**
Receiving earned income	0.682	0.008	0.650	−0.004
Receiving disability payments	0.084	−0.026	0.096	−0.013
Receiving social security income	0.106	−0.003	0.137	0.000
Receiving income from Temporary Assistance for Needy Families	0.039	−0.001	0.061	0.003
Receiving unemployment payments	0.201	0.029	0.151	0.023
Receiving other income sources	0.067	−0.023	0.073	−0.005
Living situation: rent housing	0.907	−0.010	0.818	−0.015
Living situation: shared housing	0.093	0.010	0.174	0.015
Shelter inhabitancy in the past 18 months	0.027	0.003	0.029	0.008
n	182	1431	1858	4448

*$P < 0.10$; **$P < 0.05$; two-tailed *t* test of the difference between treatment and control groups.

Table 2. ITT effects of fund availability on shelter spells. We performed regressions of shelter entry (3 or 6 months after calling the HPCC) or number of days spent in a shelter (6 months after calling the HPCC) on an availability-of-funds indicator, demographic characteristics of the caller, ZIP code–level characteristics, fund-specific restriction variables, and seasonal control variables. The standard errors were clustered at the ZIP code level. Shown here are estimates of the coefficients on the availability-of-funds indicator. Estimates of the coefficients on other covariates are given in table S8.

Dependent variable	Shelter admittance		Days spent in shelter
	3 months after calling	6 months after calling	6 months after calling
Main sample			
Funds are available	−0.014**	−0.016**	−2.620**
(Standard error)	(0.005)	(0.005)	(0.878)
n	4448	4448	4448
Mean of dependent variable for control group	0.016	0.021	3.132
Homogeneous subsample			
Funds are available	−0.019	−0.021	−2.987
(Standard error)	(0.013)	(0.013)	(1.911)
n	1431	1431	1431
Mean of dependent variable for control group	0.027	0.033	4.714
Rent callers			
Funds are available	−0.014**	−0.015**	−2.278**
(Standard error)	(0.006)	(0.006)	(1.025)
n	3574	3574	3574
Mean of dependent variable for control group	0.018	0.021	3.169
Security deposit callers			
Funds are available	−0.014*	−0.026**	−4.571**
(Standard error)	(0.008)	(0.010)	(1.538)
n	874	874	874
Mean of dependent variable for control group	0.012	0.020	3.062
Rent callers, below median income			
Funds are available	−0.023**	−0.022**	−3.389**
(Standard error)	(0.008)	(0.009)	(1.486)
n	1781	1781	1781
Mean of dependent variable for control group	0.023	0.025	3.565
Rent callers, above median income			
Funds are available	−0.004	−0.007	−0.942
(Standard error)	(0.008)	(0.009)	(1.352)
n	1790	1790	1790
Mean of dependent variable for control group	0.013	0.018	2.783

*$P < 0.10$; **$P < 0.05$; two-tailed *t* test of the difference between treatment and control groups.

private costs. For example, homeless adults face higher mortality rates (*19*); one-fifth of homeless children have been separated from their families, and they are twice as likely to have a learning disability and to repeat a grade (*20*). These private costs of homelessness can be very high, so even a small reduction in homelessness can generate substantial cost savings. For example, the benefits associated with declined mortality alone can offset much of the cost of reducing homelessness through financial assistance. Estimates suggest that age-adjusted mortality rates for the homeless population in New York City are four times as great as for the U.S. population as a whole and two to three times as great as for the population of New York City (*19*, *21*). Taking the lower bound of this mortality effect as causal, U.S. Environmental Protection Agency estimates of the value of a statistical life suggest that the mortality-reducing benefit per person that avoids homelessness comes to about $13,000 (supplementary materials, section 2.4). Based on this estimate, the benefits stemming from reduced mortality alone exceed the costs.

It is important to qualify that none of the estimates of the impact of homelessness on mortality are causal. In fact, there is virtually no evidence of the causal relationship between homelessness and other private costs. This makes it difficult to calculate reliable estimates of the private benefits stemming from homelessness prevention. Nevertheless, taking the estimates of potential benefits discussed above at face value, they amount to $20,548 per homeless spell averted ($2400 + $5148 + $13,000), which far exceeds the estimated $10,300 price tag for reducing homelessness through emergency financial assistance. The implied net benefits could be even greater if one were to include the value of other potential benefits of homelessness prevention, such as improved health, better academic outcomes for children, and others. Moreover, because the cost per homeless spell averted is much lower for the lowest-income callers, the net benefits of emergency assistance are considerably larger for this population. In addition, this discussion only addresses the impact of financial assistance on homelessness. Such assistance may also reduce the likelihood of moving involuntarily to another residence or moving in with family or friends. If temporary financial assistance reduces the likelihood of having to move, then the value of the benefits of this intervention would be even greater.

This study provides quasi-experimental evidence of the efficacy of a homelessness prevention approach that is widespread but understudied. The results show that temporary financial assistance does, in fact, prevent some individuals from having to enter a shelter. Although the cost of reducing homelessness through this prevention approach is relatively high, the cost savings of the program are likely to far exceed these costs because homelessness exacts a considerable toll on society and the homeless themselves. In addition, the evidence indicates that these programs would be more cost-effective if they were to target groups that benefit more from financial assistance, such as very low-income individuals and families.

A substantial expansion of temporary financial assistance programs may adversely affect the behavior of those who are potentially eligible—for example, by encouraging more individuals to seek assistance or by discouraging individuals from self-insuring through personal savings. Policymakers should take into account the potential for these sorts of moral-hazard responses when deciding whether to expand homelessness prevention programs.

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5. To conduct a joint test of the significance of these characteristics, we regressed availability of funds on all of the exogenous characteristics in Table 1, as well as on the fund-specific restrictions and seasonal controls. The *F* statistic for the null hypothesis that all coefficients on the exogenous variables are jointly equal to zero is $F_{(31,\ 4091)} = 1.36$, with $P = 0.09$.
6. This distinction is important because of noncompliance: Some callers who are referred to an agency for assistance do not receive funds. Furthermore, some callers seeking assistance when funds are not available may receive funds by calling back when funds are available. Unfortunately, our data sources do not include information on actual receipt of financial assistance. However, receipt information for a subset of HPCC callers suggests that just under three-quarters of those

referred to funds actually receive financial assistance (*7*). Section 1.5 of the supplementary materials includes further discussion.
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8. These estimates reflect the effect of fund availability on the likelihood of checking into a shelter that reports data to the HMIS system (our source for shelter data). As explained in the supplementary materials, section 1.1, our shelter data do not cover all shelter entries. In addition, our data do not capture the effect for the unsheltered homeless or for those who move in with others.
9. Because these subgroup analyses entail multiple hypotheses, there is greater risk of a type I error. If we make a simple Bonferroni adjustment to the *P* values, treating each sample split in isolation (i.e., the number of subgroups is two), then two of the main effects for the subgroups in table S10 are significant at $P < 0.05$: the effect for those below median income and the effect for those calling in the winter. If, however, one is interested in controlling for the familywise error rate for all six regressions in table S10 simultaneously, then none of the main effects are significant at $P < 0.05$, but the effect for those below median income is significant at $P < 0.1$ ($P = 0.06$).
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12. Only about 2% of those in our main sample who called when no funds were available entered a shelter within the next 6 months. We estimate that our data capture just over half of all homeless spells (supplementary materials, section 1.1), suggesting that about 4% of these callers become homeless. However, financial assistance may also reduce the risk of other bad outcomes such as involuntarily having to move to another residence or having to move in with family or friends.
13. Other studies have noted that homelessness prevention programs could be more effective if they were to do a better job of targeting resources to high-risk families and individuals (*14*, *15*).
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18. These are very rough estimates. The estimate of public costs is for Los Angeles only and is for the absolute per-person cost for the homeless, rather than the cost relative to an otherwise similar person who is not homeless. But this estimate might be a reasonable upper bound of the impact of homelessness on the costs for health care, police and incarceration, and welfare programs.
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21. The estimates from this study for the effect of homelessness on mortality are for a cross section of single adults in New York City. The effect of homelessness on mortality for this group may not accurately reflect that for the typical caller to the HPCC. Because there is very limited evidence on the direct effect of homelessness on mortality, these are the best estimates available.

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SUPPLEMENTARY MATERIALS

www.sciencemag.org/content/353/6300/694/suppl/DC1
Materials and Methods
Supplementary Text
Figs. S1 to S7
Tables S1 to S10
References (*22*–*28*)

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ATMOSPHERIC CHEMISTRY

Atmospheric photochemistry at a fatty acid–coated air-water interface

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Although fatty acids are believed to be photochemically inert in the actinic region, complex volatile organic compounds are produced during illumination of an air-water interface coated solely with a monolayer of carboxylic acid. When aqueous solutions containing nonanoic acid (NA) at bulk concentrations that give rise to just over a monolayer of NA coverage are illuminated with actinic radiation, saturated and unsaturated aldehydes are seen in the gas phase, and more highly oxygenated products appear in the aqueous phase. This chemistry is probably initiated by triplet-state NA molecules excited by direct absorption of actinic light at the water surface. Because fatty acids–covered interfaces are ubiquitous in the environment, such photochemical processing will have a substantial impact on local ozone and particle formation.

Over the past 20 years, interfacial processes have become increasingly of interest in the field of atmospheric chemistry (*1*), with many studies showing that environmental surfaces display specific chemistry and photochemistry, enhancing certain reactions and acting as reactive sinks or sources for various atmospherically relevant species (*2*–*5*). Many molecules display a free-energy minimum at the air-water interface (*6*–*9*), making it a favored venue for compound accumulation and reaction. Indeed, surface-active molecules have been shown to undergo specific photochemistry at the air-water interface; for example, dimers of 2-oxooctanoic acid are formed there in addition to the expected fragmentation pathways (*10*). Of importance in the environment, the sea-surface microlayer (SML) is mainly composed of surface-active, biogenically derived organics. Recent measurements of the gas phase above irradiated SML surfaces, or simple organic-coated aqueous samples containing natural photosensitizers, have revealed the photochemical formation of a wide variety of functionalized volatile organic compounds (VOCs) (*11*, *12*). Such previously unknown surface-photosensitized chemistry could constitute a substantial abiotic source of VOCs to the marine boundary layer; it is therefore important to fully understand the production mechanisms. Here, we show that complex VOCs may also be produced without an added photosensitizer from an illuminated simple organic acid, which does not absorb actinic light in dilute solutions but exhibits a totally different behavior once present as coating at the air-water interface.

Nonanoic acid (NA) is a highly surface-active simple organic fatty acid, representative of many biogenic compounds present in the SML. In the gas phase and at its solubility limit in aqueous solution, it does not absorb ultraviolet (UV)–visible radiation in the actinic region (that is, at wavelengths longer than ~280 nm). Surprisingly, when a 15-mL quartz reactor was filled halfway with a 2 mM aqueous solution of NA, a concentration giving rise to just over a monolayer, and irradiated for 1 hour with a Xenon arc lamp, prompt formation of gas-phase saturated and unsaturated C_9 and C_8 aldehydes was observed. This result is displayed in Fig. 1. In addition to the aldehydes, a wide variety of (mostly oxygenated) photoproducts was detected in both gas and condensed phases; these are listed in table S1.

The influence of oxygen on the product distribution is displayed for the gas and solution

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The impact of homelessness prevention programs on homelessness

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Programs that buffer a financial shock work

For people without a safety net of social and financial resources, a shock, such as medical expenses not covered by insurance, can be the first step in a downward spiral toward homelessness and morbidity. Evans *et al.* evaluate the effectiveness and cost of a program in Chicago that provides temporary financial assistance with the aim of enabling individuals to stay in their homes and out of homeless shelters. They find that one-time payments of up to $1500 greatly reduce the likelihood of homelessness. The estimated economic benefits exceed the estimated costs, with immeasurable psychic and physical benefits.

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