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Preventing deaths and injuries from house fires: an outcome evaluation of a community-based smoke alarm installation programme

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ABSTRACT

Background Few studies have examined the impact of community-based smoke alarm (SA) distribution programmes on the occurrence of house fire-related deaths and injuries (HF-D/I).

Objective To determine whether the rate of HF-D/I differed for programme houses that had a SA installed through a community-based programme called Operation Installation, versus non-programme houses in the same census tracts that had not received such a SA.

Methods Teams of volunteers and firefighters canvassed houses in 36 high-risk target census tracts in Dallas, TX, between April 2001 and April 2011, and installed lithium-powered SAs in houses where residents were present and gave permission. We then followed incidence of HF-D/I among residents of the 8134 programme houses versus the 24 346 non-programme houses.

Results After a mean of 5.2 years of follow-up, the unadjusted HF-D/I rate was 68% lower among residents of programme houses versus non-programme houses (3.1 vs 9.6 per 100 000 population, respectively; rate ratio, 0.32; 95% CI 0.10 to 0.84). Multivariate analysis including several demographic variables showed that the adjusted HF-D/I rate in programme houses was 63% lower than non-programme houses. The programme was most effective in the first 5 years after SA installation, with declining difference in rates after the 6th year, probably due to SAs becoming non-functional during that time.

Conclusions This collaborative, community-based SA installation programme was effective at preventing deaths and injuries from house fires, but the duration of effectiveness was less than 10 years.

INTRODUCTION

House fires continue to be a major cause of deaths and injuries in the USA, accounting for more than 2600 deaths and 13 000 injuries in 2010.¹ Smoke alarms are one of the mainstays of prevention of these deaths, but many houses still do not have functional smoke alarms, especially those at highest risk.^{2–4} One approach to increasing the prevalence of smoke alarms in high-risk houses, is through community-based smoke alarm installation programmes,^{5–8} but there are few studies that have evaluated the impact of such programmes on the rate of fire deaths and injuries.^{7 9 10} One study in the USA showed a significant decrease in injury rates after the programme⁸; however, a critique of

the study raised the issue of possible biases, including regression to the mean, in that before/after evaluation.¹¹

Based on an initial epidemiologic study of house fire-related deaths and injuries in 1990s,¹² we designed a targeted, community-based intervention called Operation Installation (OI) to install smoke alarms in high-risk census tracts in Dallas, TX; high-risk tracts included those tracts that had been previously identified as having high rates of house fire-related deaths and injuries, and that had lowest median income. We evaluated the impact of this programme on house fire-related deaths and injuries using a cohort approach which, to our knowledge, has not been done previously, to evaluate a smoke alarm installation programme.

METHODS

Intervention: operation installation

OI has been a joint project of the Injury Prevention Center of Greater Dallas (IPC), the Dallas Fire Rescue Department (DFRD), and the Dallas chapter of the American Red Cross (ARC). With each OI, volunteers assist DFRD firefighters and prevention staff as they go into homes to install smoke alarms and collect information about each home. OI was patterned after the Oklahoma City programme that was begun in 1990,⁸ the main difference being that in OI, all smoke alarms have been installed by OI personnel, and used only lithium-powered ionisation-type smoke alarms (see online supplementary appendix 1 for more details). Target census tracts were chosen because they were high-risk tracts, which included those with rates in the upper quartile during the 1991–1997 study, and those with income in the lowest quartile.¹²

Setting and participants: definitions of programme population, non-programme population and cohort denominators

For purposes of this study, a house was defined as a single-family or two-family dwelling (eg, a house or a duplex). A programme house was defined as one that received at least one smoke alarm through OI; a non-programme house was any other house in the same census tract that did not receive a smoke alarm through OI. Information about programme households (programme population cohort) was measured directly by surveys that were completed by volunteers with OI, at the time of installation of the smoke alarm. Information collected on the

survey form for each programme house included address, type of residence (house, duplex), whether the residence was rented versus owned, number of residents who lived in house, number of residents who were >64 years old and the number <5 years old, and the race/ethnicity of the head of the household, among other variables. Missing data for the number of persons residing in the house were managed with a multiple imputation technique. For non-programme population cohort, aggregated survey results for programme houses were subtracted from the aggregated totals for that census tract estimate for the house/duplex population for the year that OI was carried out in the tract. See supplementary appendix 2 for more details.

Case definition/outcome measures

For the outcome measure for this study, we defined a 'case' as a house fire-related death or injury (HF-D/I) in one of the 36 target census tracts. A death was defined as a person who the Dallas County Medical Examiner determined to have died from a house fire that occurred in a target census tract, and a non-fatal injury was defined as a person who was injured but did not die from a house fire that occurred in a target census tract, and who was identified through the Emergency Medical Services database as having been transported by ambulance to a hospital (see online supplementary appendix 3 for more details).

Study period

OI first took place in October of 1999, but we did not begin systematically collecting data with a standardised survey form until 28 April 2001, which is the date we used as the beginning of this study. For each census tract, the study period began on the date of OI in the census tract, and ended on 4/27/2011 (10 years after the first OI of this study) or when a 2nd OI was done (which occurred in three census tracts), whichever was first. The follow-up period varied for each tract depending on the date of OI. Follow-up duration ranged from 10 years in the earliest tract, to 2.5 months in the latest OI tract.

Smoke alarm follow-up surveys

Eight hundred randomly sampled houses that had received smoke alarms through OI approximately 2, 4, 6, 8 and 10 years previously, were revisited by DFRD personnel to assess presence and functioning of the OI smoke alarms that had previously been installed. These methods are detailed in a companion paper.¹³

Statistical analysis

Rates were calculated as cases per 100 000 person-years of follow-up. Comparisons of unadjusted rates were done as rate ratios (RRs), with a conditional maximum likelihood estimate of the RR with exact confidence limits.¹⁴ The preventive fraction in the exposed population (the programme population), which is a measure of the efficacy of the intervention, was calculated as 1 minus the rate ratio.

To calculate adjusted rates for comparison of programme and non-programme house population, we performed a multivariate analysis that treated the programme and non-programme population rates as collected from two independent samples. Each rate was weighted for the size of the denominator (ie, the number of person-years for programme and non-programme populations in the tract). The analysis fit a multiple regression model, with programme and non-programme case rates for each census tract as the dependent variable, an indicator variable for programme and non-programme populations, and the demographic variables listed in table 1 for the programme and the

Table 1 Baseline characteristics for programme and non-programme populations*

	Programme houses	Non-programme houses†	Total
Number of homes	8134	24 346	32 480
Population	28 570	79 135	107 705
Mean (number of persons/house)	3.5	3.3	3.3
Number of smoke alarms installed	20 127	–	
Mean (#SD/house)	2.5	–	
Households (% with characteristic)			
Owned residence	71.1	63.7	65.6
Black HH	47.6	52.7	51.4
Hispanic HH	43.5	35.5	37.5
HH >64 years	31.3	21.0	23.6
Owned and HH >64 years	26.8	16.9	19.4
Rented and HH >64 years	4.5	4.1	4.2

p < 0.05 for all comparisons of household characteristics between programme and non-programme house populations, except rented and HH >64 years, which was non-significant.

*36 Target census tracts.

†See text for methods of calculating non-programme population.

HH, Head of household.

non-programme populations as covariates. Thus, case rates for programme and non-programme populations were adjusted for the size of the populations and the follow-up duration and the percentages of rental versus owned houses, head of household being African-American or Hispanic, or being elderly (>64 years), as well as elderly and rented or elderly and owned houses.

A secondary analysis used the difference between case rates for programme versus non-programme populations in a paired comparisons methodology to assess case rate difference for each of the 36 census tracts. Online supplementary appendix 4 contains more details of the analyses.

Data were analysed with Epi Info (TM) 3.5.3 (CDC, Atlanta, Georgia, USA) and STATA V.11.2 (STATA, College Station, Texas, USA). For the cumulative case rate graph, rates over time for the programme and non-programme populations were compared by the Kolmogorov–Smirnov equality of distributions test.¹⁵

RESULTS

Programme and non-programme populations

During the study period, OI was performed in 36 census tracts, containing 32 480 houses with a population of 107 705 persons. A total of 20 127 smoke alarms were installed in 8134 houses (mean, 2.5), occupied by a population of 28 570; these comprised the programme population. The non-programme population consisted of 24 346 houses, with a population of 79 135 (table 1).

Study period case rates

For the study period in the 36 target census tracts, there were 42 cases (26 deaths/16 injuries). Mean age was 48.6 years (range, 0–90 years, with age unknown for one case) and was similar for deaths and injuries. Twenty-three cases (54.8%) were female. African-Americans accounted for 26 (61.9%) cases, 9 (21.4%) were white, 4 (9.5%) Hispanic, and 3 cases (7.1%) were of unknown race/ethnicity. Of the 26 deaths, 17 died at the scene of the house fire, and 9 died after transport to a

hospital. Of the 16 non-fatal injuries, 8 were admitted to a hospital, and 8 were seen in an emergency department (ED) and not admitted to a hospital.

Target tracts were followed for a mean of 5.2 years (range, 0.2 to 10 years) after OI, and resulted in 523 305 person-years of follow-up, for a crude rate of 8.0 cases per 100 000 person-years. Four cases (two deaths and two injuries) occurred in the programme population for a crude rate of 3.1 cases per 100 000 person-years, and 38 cases (24 deaths and 14 injuries) occurred in the non-programme population, for a crude rate of 9.6 cases per 100 000 person-years (RR, 0.32, 95% CI, 0.10 to 0.84). The rate difference was 6.5 per 100 000 population (95% CI, 2.1 to 10.8), with an attributable preventive fraction of 67.6% (95% CI, 9.2% to 88.4%) for the programme population (table 2). For fatal cases, the preventive fraction in the programme population was 74.3% (rate: 1.6 vs 6.1 per 100 000 population, respectively; RR, 0.26, 95% CI 0.04 to 0.93). For non-fatal cases the preventive fraction was 56% (rate: 1.6 vs 3.5 per 100 000 population, respectively; RR, 0.44, 95% CI 0.07 to 1.7).

For the first 5 years of follow-up, the case rate in the programme population was significantly lower than in the non-programme population (1.0 vs 9.8 per 100 000 population, respectively; RR=0.1, 95% CI 0.01 to 0.74; attributable preventive fraction=90%), but it was not for the second 5 years of follow-up (11.1 vs 9.0 per 100 000 population, respectively; RR=1.2, 95% CI 0.3 to 4.4).

Of the four cases that occurred in programme houses, one occurred during the first 5 years after OI, and the smoke alarm functioned. Three cases occurred during the second 5 years, and none of the smoke alarms functioned (in two, the smoke alarm was present but not working, and in one, the alarm was not present). Each of the latter three cases had received only one smoke alarm thru OI.

Cumulative incidence

The cumulative incidence for the programme and non-programme populations (figure 1) showed a significant difference between the two groups ($p < 0.02$, by Kolmogorov–Smirnov equality of distributions test). Additionally, the graph

appears to have a changing slope of incidence for the programme population in the later years of the study, with a declining difference between the two groups starting at about the seventh year of the study.

Multivariate analysis

From the multivariate analysis, the adjusted case rate in the programme population was lower than the non-programme population (3.5 vs 9.5, respectively, rate difference 6.0; 95% CI 0.8 to 11.1; RR=0.37, 95% CI 0.00 to 0.86), as listed in table 3. The rates for programme versus non-programme populations were significantly different for the first 5 years of follow-up ($p < 0.01$), but not for the second 5 years.

Results of the paired comparisons analyses for the full 10-year period (adjusted rate difference 5.1, 95% CI 2.7 to 7.5) were similar to results of the overall adjusted analysis, although somewhat more pronounced and with wider CIs, and also was significant for the first 5 years ($p < 0.001$) but not for the second 5 years.

Smoke alarm prevalence

Among the 800 houses that were revisited in the random sample, the proportion of houses that had at least one working OI smoke alarm was substantially lower in years 6, 8 and 10 than in the 2-year and 4-year strata (figure 2).

DISCUSSION

To our knowledge, this is the first study to look at the impact of a smoke alarm installation programme using population-based rates of house fire-related deaths and injuries in houses that received a smoke alarm (programme houses) versus houses in the same neighbourhoods that did not receive one (non-programme houses). The unadjusted case rate in programme homes was 68% lower than non-programme homes, and the multivariate-adjusted rates showed a similarly lower rate in programme homes, translating to a programme efficacy of >60%. We found significantly lower case rates in programme homes in all four analyses that were done.

One previous randomised study in the UK showed no difference in injuries or deaths for neighbourhoods that had a smoke alarm distribution programme.¹⁶ However, that study did not install the alarms, and few of the alarms handed out were ever installed, which probably accounted for the lack of effect of the programme. Our study documented the presence of a high proportion of functioning alarms in a sample survey 2 years after the programme.

The efficacy found in our study was comparable with that found by Mallonee *et al*, in a smoke alarm giveaway programme in Oklahoma City in 1990.⁸ That study has been criticised because its design could not exclude regression to the mean as an explanation for the drop in death and injury rates.¹¹ Our design sought to eliminate the issue of regression to the mean, by following two cohorts (programme and non-programme homes) through the same time periods. Even so, we found no evidence of regression to the mean for the case rates for study census tracts, between the 1990s and the 2000s.

The Oklahoma City programme followed homes for about 4 years, used alkaline battery-powered smoke alarms, and documented the presence of functioning alarms in a majority of programme houses after the distribution effort.⁸ That study found a 77% lower rate in the target zip codes 4 years after the programme. We found a similar lower rate in programme homes for the first 5 years of the study (9.8 vs 1.0, an 89% lower rate in programme homes).

We were hopeful that by using lithium-powered smoke alarms, our programme would sustain its efficacy longer.

Table 2 Incidence rates of cases, for programme and non-programme populations

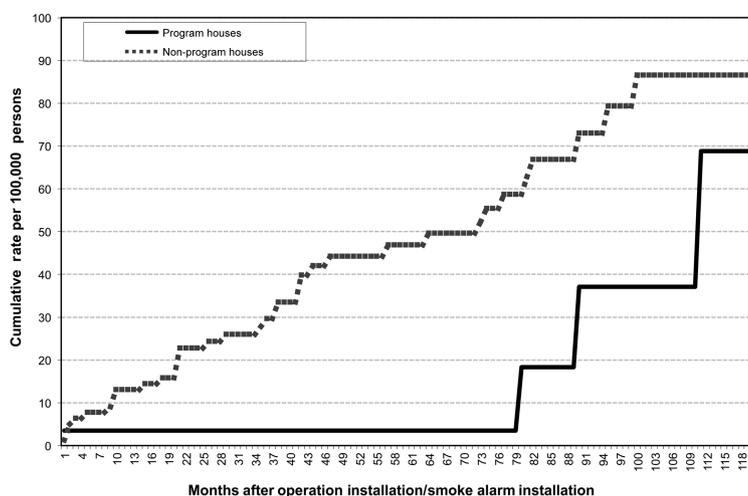
	Programme houses	Non-programme houses*	Total
Number of homes	8134	24 346	32 480
Population	28 570	79 135	107 705
Mean (#persons/house)	3.5	3.3	3.3
Number of smoke alarms installed	20 127	–	
Mean (#SD/house)	2.5	–	
Person-years of follow-up	128 333	394 972	523 305
Cases			
Number of deaths	2	24	26
Number of injuries	2	14	16
Total	4	38	42
Rate† (95% CI)	3.1 (0.8 to 8.0)	9.6 (6.8 to 13.3)	8.0 (5.8 to 10.9)

Includes 36 Target census tracts.

*See text for methods of calculating non-programme population.

†Per 100 000 person-years. For comparison of programme versus non-programme houses: Rate ratio=0.32 (95% CI 0.10 to 0.84); Preventive fraction in programme population: 68% (95% CI 9.2 to 88.4%).

Figure 1 Cumulative incidence rate of house fire-related cases, According to study group, for houses in target census tracts. Cases were defined as a death or non-fatal injury related to a house fire. Programme houses had received a smoke alarm through Operation Installation, between 2001 and 2011. Non-programme houses were all other houses in the same census tracts, that did not receive a smoke alarm through Operation Installation.



No. Persons at Risk	0 years	2 years	4 years	6 years	8 years	10 years
Program	28570	25729	14757	9108	5097	156
Non-Program	79135	71808	45322	36446	15661	1535

However, the efficacy of OI seems to have dropped during the second 5 years of follow-up. There was a marked difference in incidence case rates between the first 5 years and the second 5 years in the programme population, with an apparent change in the cumulative incidence curve at about 6–7 years. This may have been due to non-functioning of smoke alarms over the years. Although lithium-powered alarms are supposed to function for 10 years, it was apparent from our follow-up testing that they do not. Although >90% of the programme houses had at least one working smoke alarm at the 2-year follow-up sample, that proportion was down to 20% for the 10-year sample.¹³ This is similar to the findings of Jackson *et al*,¹⁷ that only about one-third of lithium-powered smoke alarms were still functioning after 10 years.

In our study, there were four ‘failures’ with deaths or injuries in programme houses; three occurred late, in years 7–10 of the study; for all three, the original OI smoke alarm was non-functional (two cases) or had been removed (one case), and there was no other functional smoke alarm in the residence at the time. Each had received only one smoke alarm through OI. Installing more than one alarm likely would have increased the odds that at least one alarm would be working after several years. The results support the recommendation for regular monitoring of smoke alarms to assure that they are functioning,³ even when lithium powered. This also has important implications for planning of repeat smoke alarm installation programmes in communities that implement such programmes.

There are several limitations of our study. The study was not randomised, and although programme and non-programme houses were on same blocks and in the same census tracts, they may have differed in important ways that we did not measure, such as the prevalence of smoking, poverty, or pre-existing smoke

alarms. It is possible that persons in programme houses were inherently at lower risk of fire-related death or injury, and self-selected into the programme. However, the multivariate analyses, which attempted to control for factors such as race, ownership status, elderly household member and so on, confirmed the significance of the finding from the crude rate analysis, implying that the programme and non-programme populations were not dissimilar despite not being randomised. Additionally, the case rate in the programme population for the second 5 years of follow-up was as high as the non-programme population, coinciding with a time when many of the smoke alarms had ceased to function. Another potential limitation is that the population of non-programme homes was calculated using census data, rather than by direct measurement. Inaccuracies in the census data could have overestimated or underestimated the population, leading to errors in calculations of rates for this comparison group. We attempted to minimise the differences in census counts by making interpolated estimates of the population for the year of OI. The number of residents per home was similar for the programme and non-programme populations, and case rate for the non-programme population (9.6 per 100 000 population) was similar to the pre-study case rate (9.3 per 100 000), prior to the intervention. Additionally, it is possible that our surveillance may have missed some cases of non-fatal injury, thus underestimating case rates; however, this would likely have been similar for the programme or non-programme populations, and when we analysed only fatal cases, the effectiveness remained significant. And finally, it is possible that some of the impact of the intervention resulted from the brief on-site education that was done at the time of smoke alarm installation. We could not separate the impact of the educational component of the programme and the resulting awareness, from the actual installation of the smoke alarms.

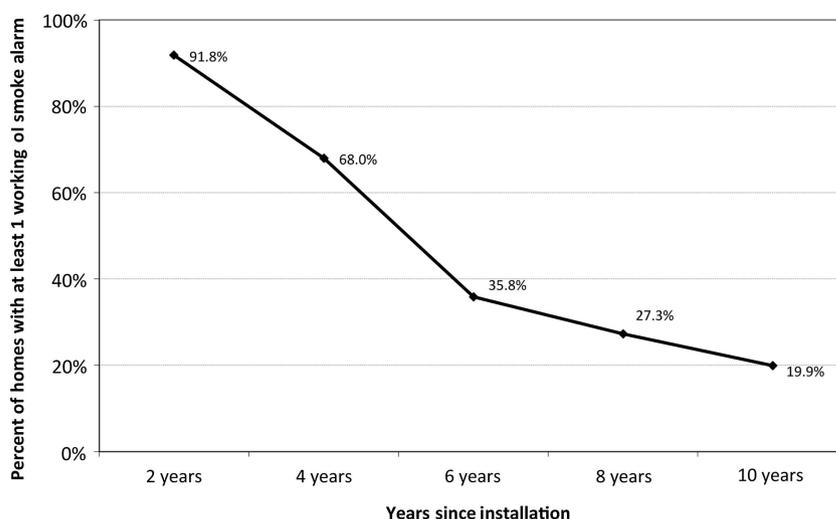
Table 3 Comparison of case rates for programme and non-programme populations, including unadjusted and adjusted rates, rate differences and rate ratios

	Rate* in programme houses	Rate* in non-programme houses	Rate difference	Rate ratio	Preventive fraction
Unadjusted (95% CI)	3.1 (0.8 to 8.0)	9.6 (6.8 to 13.3)	6.5 (2.2 to 10.8)	0.32 (0.10 to 0.84)	68% (9.2 to 88.4%)
Adjusted† (95% CI)	3.5 (0 to 8.2)	9.5 (7.3 to 11.7)	6.0 (0.8 to 11.1)	0.37 (0.00 to 0.86)	63% (16 to 100%)

*Per 100 000 person-years.

†Multivariate regression analysis using independent samples approach, controlling for size of the populations and the follow-up duration and the percentages of rental versus owned houses, head of household (HH) being African-American or Hispanic, HH being elderly (>64 years), as well as elderly HH and rented or elderly HH and owned houses (see text).

Figure 2 Proportion of houses that still had at least one working smoke alarm, by years, since installation, for programme houses that received at least one smoke alarm through Operation Installation, Dallas, TX, n=800. Adapted from McCoy *et al*¹³; companion paper.



These results were the culmination of a typical public health approach to prevention: identifying a problem and the populations at risk, designing and implementing an intervention, and then monitoring outcomes to evaluate its impact. Even so, it required more than 10 years of implementation and follow-up to document its efficacy, a difficulty that is inherent in using relatively rare events such as house fire deaths and injuries as outcome measures. Nevertheless, the results reinforce the value of a community-based smoke alarm installation programme to decrease deaths and injuries from house fires, and point out the importance of long-term support for their implementation.^{7 18}

What is already known on the subject

- ▶ Smoke alarms are well documented to reduce the risk of death from residential fires, but there are conflicting data regarding the impact of community-based smoke alarm installation programmes.
- ▶ There are no outcome studies of the efficacy of such programmes that used lithium-powered smoke alarms
- ▶ The duration of efficacy of community-based installation programmes is not known.

What this study adds

- ▶ This community-based smoke alarm installation programme was highly effective for residents of houses that received a smoke alarm, for the first 5–6 years after installation, but its efficacy declined substantially after that time.
- ▶ Most cases of house fire-related deaths and injuries in programme houses occurred in houses where the programme smoke alarm either did not work, or had been removed.
- ▶ Based on this data, smoke alarm installation programmes may not be able to plan on efficacy remaining high for the full 10 years that lithium powered alarms are advertised to work.

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Contributors GRI, MAM, CR, SS-S, DKC, MS and RJA contributed to the concept and design of the project. MAM, CR and JJB contributed to the collection the data. GRI, MAM and BJM collaborated on the analyses, interpreting the results and writing the article. SS-S, DKC, MS and RJA provided administrative support for the project. GRI and MAM had access to all data and are responsible for the overall content. All authors reviewed and approved the final version.

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Competing interests None.

Ethics approval This study was deemed to have exempt status by the University of Texas Southwestern Medical Center Institutional Review Board.

Provenance and peer review Not commissioned; externally peer reviewed.

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7000 US children injured or killed by guns annually

Yale University researchers, in a paper in *Paediatrics*, estimated that firearm injuries sent 7391 children to the emergency room in 2009 and 6% of them died. The damage caused by gun-related injuries rarely gets the same attention as fatalities, with the most common firearm injuries being open wounds, fractures and internal injuries of the thorax, abdomen or pelvis. (noted by IBP).

Alberta researchers aim to reduce youth sports injuries by 20% in 5 years

Top injury prevention researchers in Alberta, along with community partners (Hockey Canada and WinSport) aim to reduce youth sport injuries by 20% by 2020. The initiative is led by Carolyn Emery and Brent Hagel and will focus on ice hockey, skiing and snowboarding, and junior high school phys-ed programmes. The initiative will build capacity by creating an interdisciplinary group of researchers including 20 international experts. What is perhaps most significant about this initiative is the inclusion of a fixed goal that may actually be achievable. (noted by IBP).

Child car seats in the USA may be required for protection in side-impact crashes

The National Highway Traffic Safety Administration seeks to improve child car seats by including a test that simulates a side crash. The agency estimates the new standards would prevent the deaths of about five children, and injuries to 64 others, every year. A safety advocate, applauding the proposal, called the estimate 'very, very conservative'. Safety advocates have long sought tougher side-impact standards for child car seats. (*The Associated Press*) (noted by IBP).

Supplementary Appendix 1

Operation Installation

In a previous study of house fire-related deaths and injuries in Dallas, TX, we had performed calculations of rates of these deaths and injuries for every census tract in Dallas for 1991-1997.¹ That study and others showed that house fire-related deaths and injuries were highest in the census tracts with lowest median income,¹⁻⁵ and in houses without functioning smoke alarms.^{6,7} To address this problem, we began a joint effort along with the Dallas Fire Rescue Department, to install smoke alarms in houses in high-risk census tracts in Dallas through a program called “Operation Installation” (OI). Ongoing surveillance allowed us to evaluate the impact of OI on rates of house fire-related deaths and injuries for residents of the houses that received smoke alarms through OI, compared to the rate in residents of houses in the same census tracts that did not receive a smoke alarm through OI.

Operation Installation has been a joint project of the Injury Prevention Center of Greater Dallas (IPC), the Dallas Fire Rescue Department (DFRD), and the Dallas chapter of the American Red Cross (ARC). With each OI, volunteers assist DFRD firefighters and prevention staff as they go into homes to install smoke alarms and collect information about each home. OI was patterned after the Oklahoma City program that was begun in 1990;⁸ the main difference from that program is that in OI, all smoke alarms have been installed by OI personnel, and only lithium-powered ionization-type smoke alarms were installed. The process of OI involves canvassing the target census tract, going door-to-door to offer installation of smoke alarms in the home, along with brochures about how to decrease the risk of fires and to develop and practice an escape plan. OI typically takes place 8-10 times per year, on a Saturday morning, with 16 teams (consisting of a fireman, a fire prevention officer, and a volunteer) canvassing the census tract along with 4 firefighting vehicles (fire trucks, ladder trucks, etc.) in a pre-arranged manner, for 3-4 hours. Approximately 200-400 houses are typically canvassed in each session. Each census tract takes ~2-3 sessions to complete, depending on the size of the tract. For houses where no one was home at the time of the canvassing, teams leave door hangers with information about how to request a smoke alarm to be installed by the DFRD. Only houses and duplexes were included in OI, because Dallas city has had a code requiring apartments to have functioning smoke alarms, and as a result, DFRD has prioritized houses and duplexes for this intervention. For purposes of this study, a house was defined as a single-family or two-family dwelling (essentially, a house or a duplex). Smoke alarms were provided from donations by various groups and persons between 2001 and 2006, and by a grant from CDC from 2007-2011.

Target census tracts were chosen because they were high risk tracts, which included those with high rates during the 1991-1997 study,¹ and those with lowest income. OI first took place in October of 1999, but we did not begin systematically collecting data with a standardized survey form until April 28, 2001. There were a total of 32,480 homes in the 36 target census tracts. OI installed smoke alarms in 25% of the homes; in 57%, no one

answered the door; in 10% the resident refused to allow the OI team to enter; in 8% the resident had adequate smoke alarm coverage and did not want more installed.

References:

1. Istre G, McCoy MA, Osborn L, et al. Deaths and injuries from house fires. *N Engl J Med* 2001;344:1911-1916.
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Supplementary Appendix 2

Information Collected for Program Houses

Information collected on the survey form for each program house included address, type of residence (house, duplex), rented vs. owned, number of residents who lived in house, number of residents who were >64 years old and the number < 5 years old, the race/ethnicity of the head of the household, the presence of a telephone, and the number of smokers in the residence, among other variables. Information for total number of residents was missing from the surveys for 109 (1.3%) program houses. We used a multiple imputation technique to impute values for these missing fields, with the “impute” function for STATA 11, which utilizes the approach of Little and Rubin¹. The following variables were used for the imputation: age and race of head of household, and owned/rented status. Through STATA we conducted 20 imputations for each of the missing values and combined the results into a single multiple-imputation result. Using these imputed values changed the denominator by about 1%, which made minimal difference in the calculation of rates for the study, either with or without the imputation, and did not affect the results. For example, the following were the rates per 100,000 population for the Program and Non-program populations, the CMLE Rate Ratios, and the calculated Preventive Fractions, respectively: **with** imputed data: Program rate, 3.12; Non-program rate, 9.62; CMLE RR, 0.324; Preventive Fraction, 67.6%; **without** the imputed data: Program rate, 3.15; Non-program rate, 9.59; CMLE RR, 0.329; Preventive Fraction, 67.1%. Complete addresses were missing for 111 (1.4%) program houses; these were excluded from the analysis.

Definitions of Program Population, Non-program population, and Cohort denominators

A program house was defined as one that received at least 1 smoke alarm through OI; a non-program house was any other house in the census tract. Program households were measured directly by the surveys outlined above, at the time of installation of the smoke alarm. For non-program households, aggregated survey results for program houses were subtracted from the totals for that census tract estimate for the house population for the year that OI was carried out in the tract. Because the study interval covered the years 2001-2011, census tract data for the year of OI were estimated by linear interpolation of 2000 and 2010 census data for houses and duplexes in the tract. For example, if OI took place in 2004 in a census tract, demographic data for the houses and duplexes in the census tract were estimated to reflect their expected levels in 2004 given the known 2000 and 2010 census counts, using simple linear interpolation. Thus, for each census tract, we identified the population in the cohort of houses that received a smoke alarm through OI (program population), and the population in the cohort of houses that did not receive a smoke alarm through OI (non-program population), for the year of OI. Because the house/duplex population of the target census tracts changed by < 10% between 2000 and 2010, whether we used this interpolated census data, or the 2000 census data, or the 2010

census data made little difference in the calculations of rates, and did not change the significance of the findings.

Reference

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Supplementary Appendix 3

Case Definition

For the outcome measure for this study, we defined a Case as a house fire-related death or injury (HF-D/I) in one of the 36 target census tracts. A death was defined as person who the Dallas County Medical Examiner determined to have died from a house fire that occurred in a target census tract, and a non-fatal injury was defined as a person who was injured but did not die from a house fire that occurred in a target census tract, and who was identified through the Emergency Medical Services database as having been transported by ambulance to a hospital. This approach is similar to the definition we had used in our earlier study.¹ We then identified the address of each fire that resulted in a Case, and determined whether the address was a program home or a non-program home. To do this, we linked each death and injury by the date, time, DFR incident number and address from the DFR fire incident file, and compared the address of the incident to the list of homes that had received smoke alarms through OI, to determine whether home was a program home or a non-program home.

Pre-Study Period

We designated a pre-Study period as beginning Jan 1, 1998, which was after the initial study that identified risk factors and high rate census tracts,¹ and ending on the date of first study OI in the census tract. During the pre-study period, the population of the study tracts was followed for a mean of 8 years (range, 3.3-13.1 years), for a total of 906,247 person years. During this time period, there were 84 Cases (35 deaths and 49 non-fatal injuries), which was a rate 9.3 cases per 100,000 person-years. By comparison, the case rate during the original study that covered the time period 1991-1997, was 9.9 cases per 100,000 population (data not shown).

Study Period

For each census tract, the Study Period began on the date of OI in the census tract, and ended on 4/27/2011 (10 years after the first OI of this study) or when a 2nd OI was done, whichever was first. The follow-up period varied for each tract depending on the date of OI. Follow-up duration ranged from 10 years in the earliest tract, to 2.5 months in the latest OI tract. For these first 10 years of the study, 36 census tracts were canvassed (“target tracts”); 20,127 smoke alarms were installed in 8134 houses that had 28,570 population. Since OI was an ongoing process, the dates of OI ranged from April 28, 2001 in the first census tract, until February 12, 2011 in the 36th census tract. As a result of the staggered dates of OI, the follow-up time for the census tracts ranged from 2 months to 10 years (mean, 5.2 years; median, 4.6 years). For three census tracts, OI was repeated in the tract before the full 10 years of follow-up (after 9.0 years, 8.6 years, and 6.6 years, respectively), and these were counted as program houses/target tracts only until the date of the second OI.

Smoke Alarm Follow-up

A random sample of houses that received smoke alarms approximately 2, 4, 6, 8, and 10 years previously were re-visited by DFR personnel to assess presence and functioning of the OI smoke alarms that had previously been installed. Detailed description of methods and results of this survey can be found in a companion paper.²

References

1. Istre G, McCoy MA, Osborn L, et al. Deaths and injuries from house fires. *N Engl J Med* 2001;344:1911-1916.
2. McCoy MA, et al. (*Injury Prevention* submitted companion paper)

Supplementary Appendix 4

Statistical Analysis

Data were analyzed with Epi Info (TM) 3.5.3 (CDC, Atlanta, GA) and STATA version 11.2 (STATA Corp, College Station, TX). Rates were calculated as cases per 100,000 person years of follow-up. Comparison of two rates done with a conditional maximum likelihood estimate of the Rate Ratio with exact confidence limits.¹

Two multivariate analytic approaches were taken to evaluate the effect of OI. The first, taking a two-independent samples approach, treated the program population rates and non-program population rates as collected from two independent samples. Each rate was weighted for the size of the denominator (i.e., the number of person years for program and non-program populations in the tract). The analysis fit a multiple regression model, with program and non-program case rates for each census tract as the dependent variable, an indicator variable for program and non-program populations, and demographic variables for the program and the non-program populations as covariates, including type of residence, rented vs. owned, number of residents who lived in houses, whether there were persons >64 years old, the race/ethnicity of the head of the household, among other variables. A test of the coefficient for the indicator variable provided a test for significance of intervention effect. The analysis further allowed the calculation of adjusted case rates for program and non-program populations, adjusting for the covariates mentioned.

A second analysis used the difference between house fire-related case rates for the program vs. non-program household populations in a paired comparisons methodology for assessing case rate difference for each of the 36 census tracts. Specifically, the program population case rate was subtracted from the non-program population case rate within each census tract, producing a difference that was assumed free of extraneous factor effects within a tract and a function of intervention effect. Using these resulting rate differences as outcomes, a multiple regression model was fit where case rate difference within a tract served as the dependent variable, and census variables were covariates. The objective of this model was to reduce the error variance for testing the hypothesis that mean case rate difference equals zero. The covariates included census tract data from each target census tract (but not separately for the program and non-program populations): age > 64 years, race and gender of head of household, household median income, percent of households that had paternal presence, and percent of dwellings that were owned.

For the cumulative mortality rate graph, case rates over time for the program and non-program populations were compared by the Kolmogorov-Smirnov equality of distributions test.²

Reference

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