

Household Gun Ownership

Ludwig et al. recently showed that in telephone surveys wives report the presence of a gun in the household less often than do husbands.¹ Since male and female spouses represent the same population of households, this "gender gap" should not exist. Apparently, wives are less likely to know about household guns than are husbands. The authors concluded that surveys should ask about personal, not household, gun ownership. This strategy would produce "more accurate estimates of America's gun stock than do reports about household guns."

A study by the California Firearms Injury Surveillance Program replicates Ludwig's principal finding. We included gun ownership questions in California Behavioral Risk Factor Surveillance Surveys for 1994 through 1996 and found that only 30% of California households report guns, compared with about 41% in national surveys cited by Ludwig. Even though California has a lower level of gun ownership, there was still a significant gender gap. Thirty-nine percent of male spouses reported having a gun in the household, compared with 30% of female spouses ($n=11\,861$, $P<.01$). Thus our finding supports Ludwig's conclusion that wives underreport household guns.

Ludwig argues that when we use household surveys to determine the presence of guns, we must take into account that some respondents are more knowledgeable than others. This is a valid methodological issue for survey researchers, but there is a substantive public health issue aside from this measurement problem.

Our data and Ludwig's show that many women living with men are unwittingly exposed to firearms in their homes. The presence of guns in households, not just personal gun ownership, is associated with an increased risk of homicide² and suicide.³ Gun homicides in the home kill household members or acquaintances much more often than strangers.^{4,5} Guns kept in homes are more likely to be involved in a fatal or non-fatal accidental shooting, criminal assault, or suicide attempt than to be used to injure or kill in self-defense.⁶ Women are more likely than men to report that handguns are stored safely.⁷

Public health research needs to illuminate this problem. When a couple's household contains a gun, who controls it? Who is responsible for its safe storage? Who is likely to use it to threaten or shoot someone? We believe that Ludwig's findings and ours raise an important issue for the well-being of families, particularly women and children who

live with a deadly hazard whose very existence is unknown to them. □

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The Firearm Injury Surveillance Program is funded by a grant from The California Wellness Foundation.

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The findings of Trent, Van Court, and Kim provide additional support for the view that survey estimates for household gun ownership rates may be understated because wives are less likely to report on guns in the home than are husbands. We would, however, urge caution in implementing specific public health interventions on the basis of these findings.

While Trent et al. interpret these findings as evidence that some husbands keep guns in the home without the knowledge of their partners, as we note in our original article this is only one possible explanation for differences in husbands' and wives' reports on household gun ownership. For example, another explanation is that because women are more likely to view guns as socially undesirable than men,^{1,2} social-

desirability bias may be more pronounced with the reports of wives than husbands.

While we do not share Trent and colleagues' view that these findings unambiguously demonstrate that many wives are unaware of their husbands' guns, we wholeheartedly agree with their proposed research agenda. Additional information about the role of guns in the home should be an important part of the public health community's efforts to reduce the toll of gun violence in the United States each year. □

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Parental Attitudes and Childhood Immunization

In their December 1998 article, Prislín and colleagues concluded that sociodemographic characteristics influence children's immunization primarily through their parents' beliefs, attitudes, and perception of control over immunization.¹ This conclusion is inconsistent with the findings of previous immunization research. Moreover, we are concerned that the findings may be an artifact of the way the analysis was performed.

Prislín et al. provide little discussion of previous literature concerning predictors of childhood immunization and how this study's findings fit into the larger set of scientific studies. Several previous studies, including our own, found that sociodemographic characteristics are strongly associated with immunization status, but they also showed little relationship between parents' attitudes and children's immunization status when adjustments were made for sociodemographic factors.²⁻⁵ Prislín et al. concluded from their analysis that children's immunization coverage can be improved

by educating parents. Although we agree that parents should be informed about the vaccines, their benefits and contraindications, and correct immunization schedules, consistent with the *Pediatric Standards on Immunization Practices*,⁶ there is little evidence that education of parents plays a substantial role in improving immunization coverage.⁷ Educational strategies assume that children are not appropriately immunized because their parents are not well informed or have poor attitudes about vaccines. To the contrary, the empirical evidence to date demonstrates that low immunization levels reflect our failure to achieve a primary care system that ensures early, timely, and continuous services for very young children, particularly those from disadvantaged populations, rather than parents' attitudes about immunizations.^{3,4}

We also suggest that because of the study's methodological limitations, its results be viewed with caution. The most important methodological questions concern the estimation procedures used in the mediating model and the immunization variable. First, the mediating model is confusing and not well described, and, depending on how it was estimated, it may have masked important results. In particular, it is unclear whether the authors estimated a full regression model that included the sociodemographic variables with the variables concerning parents' attitudes and beliefs. As a result, the reader cannot assess what happens to the effect of the various attitudes and beliefs on immunization when the sociodemographic variables are included in the model. The mention of an estimated R^2 for the regression results implies that standard linear regression was used, but immunization is a binary variable. Thus, the analysis may not have used the appropriate regression model—logistic regression—for a binary variable.

A second methodological limitation is the authors' up-to-date immunization variable, which is unconventional. It is a cross-sectional measure, assessing up-to-date status for age, that combines different ages of children; age, however, is not included in the regression model. Parents behave differently with regard to seeking immunizations for their children at different ages; accordingly, predictors of immunizations differ by a child's age.²⁻⁴ In addition, *Haemophilus influenzae* type b was a recommended vaccination for children born in 1992. Why was it not included in the authors' study?

More research is needed on the mechanisms by which sociodemographic characteristics work to influence immunization status. Whenever possible, future studies of immunization status should include both parental and health care provider variables in the same model. We believe that because of method-

ological limitations and inattention to relevant research on immunization, the study of Prislín et al. does not justify the broad conclusions that sociodemographic characteristics influence immunization status primarily through parental attitudes and beliefs about immunization and that educational strategies can substantially improve immunization coverage. □

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Our data indicate a reliable, although by no means perfect, relationship between parental attitudes and childhood immunization even after adjustment for sociodemographic factors, whereas Strobino et al. found differently. This difference does not invalidate either conclusion. Rather, it indicates a need for further examination of factors responsible for these differences.

We do not disagree with the position that systemic factors contribute significantly to

poor immunization rates in this country. Infant mortality rates, immunization rates, and a host of other indices of the health of our population relative to international populations clearly paint a picture of a failed health care delivery system. However, to state that systemic factors are consequential and therefore parental factors are not consequential is a non sequitur. Moreover, if immunization rates are explained solely by systemic factors, how can we account for variations within the same ethnic^{1,2} or socioeconomic groups? We agree that both systemic and parental factors should be examined in the same model in future studies, as it is reasonable to expect that they combine to affect immunization rates.

Turning to the methodological issues raised by Strobino et al., we did use appropriate analytic techniques. In our mediating model, we included a full regression model that estimated the effects of attitudes and beliefs, adjusted for sociodemographic variables. R^2 was estimated in regressions in which continuous variables (beliefs, attitudes, and control) were predicted. We used logit regressions only in the model in which a dichotomous up-to-date variable was predicted. All this was noted in the text (p 1823).

The argument that children's age may be a moderator because it affects parental behavior "with regard to seeking immunizations for their children" is worthy of further investigation. However, this suggests that parents do play a role in immunization, which is contrary to the point raised by Strobino et al. It is possible that parental beliefs, attitudes, and perception of control change with children's age; however, these factors still mediate immunizations. □

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