

DISCUSSION

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I would first like to commend the authors for their fine presentations and interesting papers. It was a great pleasure to read them.

The paper by White, Berk, and Mathiowetz discusses a methodological experiment, the results of which will be used in the planning of a major survey. There are a few issues which I would like to address relevant to this paper.

-The first issue relates to the need for oversampling in the design of major health surveys. We are long past the point where it is useful for NCHS (or other agencies) simply to provide estimates of health characteristics or utilization of services for the U.S. as a whole or even for major regions. Issues concerning access to and quality of health care, incidence and prevalence of morbidity and disability among such subgroups as Blacks, Latinos, and the elderly are crucial to health policy decisions. In order for these issues to be addressed, valid and reliable estimates will be needed for these groups, and this will require oversampling which, in turn, will call for major changes in the design of some of the major sample surveys.

-Use of the NHIS as the "master" sampling frame for the other NCHS population based surveys seems to me to be a rational move which would avoid replication of efforts in sample design. It would also permit a relatively efficient means of oversampling. This seems commendable, although it may have the potential of increasing the response burden of NHIS participant.

-A field trial designed as a factorial experiment is an appropriate use of resources that would test the effects of sample design and operational procedures on such outcome variables as costs and response rates. This particular experiment seems well thought out and should provide valuable data upon which the design and operations of NMES can be "fine tuned". It seems that in this experiment, the size of the sample is large enough to evaluate main effects but probably not large enough to discern more subtle effects and interactions. Considering, however, the large costs of such experiments, this is what we must live with.

In the paper by Gonzales, Ezzati, Lago and Waksberg, we have another example of the need for subnational estimates. In this instance, NCHS planned, I believe for the first time, a major survey to obtain information about a single ethnic group - Latinos.

Mr. Gonzales and his colleagues explore systematically the design and estimation procedures used in the Southwestern portion of HHANES. Although the Mexican origin population in these 5 States is high compared to other States, it is relatively low in absolute terms, and the necessary screening to locate such individuals would ordinarily be quite expensive. To reduce these expenses PSU's with low Hispanic population and segments with few Hispanics were eliminated from coverage. Although the estimation process attempts to adjust post hoc

for this by use of the income variable, what it really does is to substitute high income persons of Mexican origin living in places having relatively high Mexican origin density for high income persons of Mexican origin living in places having relatively low proportion of Mexican origin persons. In an examination survey such as HHANES, this might result in some biases. I refer particularly to the work of Dr. Michael Stern, a cardiovascular epidemiologist at the University of Texas at San Antonio, who has shown that the prevalence of certain markers for diabetes decreases with "acculturation" as measured by the proportion of persons of Mexican origin in a defined geographical area.

Finally, it should be emphasized that persons of Mexican origin living in the Southwest might differ considerably in health characteristics and pattern of nutritional intake from those living outside of the Southwest. For example, there is a very large population of Mexican Americans in Chicago and in other Northern cities, and their lifestyles may differ considerably from those considered in HHANES. This may limit the generalizability of the findings from this study.

The paper by Snowden is concerned with the potential use of the NHIS as a sampling frame for other NCHS population based surveys. The major focus of this paper lies in the demonstration of 1) the feasibility of linked telephone survey (LTS) methodology in obtaining data about critical health issues that affect individuals in a population subgroup, and 2) in obtaining data for a group such as a family or household.

Implicit in this paper seems to be a new NCHS interest in "quick turnaround" surveys on critical or timely issues. This type of survey has not in the past been part of the NCHS repertoire. It would be hoped that traditional NCHS standards with respect to control of sampling and measurement errors would be maintained in these new ventures.

The availability of preliminary information could appear to give LTS a considerable advantage over random digit dialing (RDD) in many situations.

The paper by Brogan and Porter demonstrates that "much more" is not necessarily "much better". The major issue seems to be that the 6 CHIP counties represent only slightly more than 1% of the total population of Georgia. Although the additional 960 sampling units taken in these 6 counties resulted in a one-third increase in the total sample, the precision for estimating the 99% of Georgia outside of these 6 counties remained essentially the same. Also, although these 6 counties had a proportion of Blacks twice that of the remainder of Georgia, they still accounted for only a small fraction of the total Black population of Georgia. Thus, the findings of no great increase in precision are not surprising.

The findings of this survey with respect to

Black-White differences in the prevalence of hypertension should be of interest since there is some recent evidence that the prevalence rates are converging.

Although the dual design did not cause an increase in precision, it probably did enable the technology required for planning and conducting a large survey to filter to the local health department level, and this, in itself, seems to be worthwhile.

The paper by Lam, Wong, and Lee is concerned with the very important problem of screening a large population for a disease (in this case, the disease is psychiatric disorders) with an inexpensive screening test which has, however, less than perfect sensitivity and specificity. The authors propose a modified two stage procedure in which a sample of the screenees is flagged in advance to receive the more definitive diagnostic test in the same session as their screening test no matter what the outcome of their screening test. Those not flagged are given the more definitive diagnostic test only if their screening test is positive by some preassigned cutoff point.

This type of problem arises frequently in both population epidemiology and clinical epidemiology. At this moment, there is a major NHLBI supported trial now in the field in which lung scanning, a relatively inexpensive and non-invasive procedure used to detect the presence of pulmonary embolism is being evaluated against pulmonary angiography, a more accurate yet more expensive and invasive "gold standard". The issues discussed here are almost identical to those being considered in this trial.

The authors, in this presentation, have derived the optimal fraction, r , of subjects to receive the diagnostic test no matter what their screening tests show. This optimal fraction maximizes the information per unit cost with

respect to the overall estimate of disease prevalence. They then go to show that this modified procedure often sacrifices little in the way of efficiency over the usual two stage screening in which only positives are given the more definitive diagnostic test.

It would appear to me that this modified procedure has considerable logistic advantages over the usual two stage procedure. It would appear, however, that if the specificity of the screening procedure is high and the prevalence of positive screens is low, then the testing of a large number of negative screenees as would be required in the modified procedure would be relatively uninformative and would result in low efficiency. Perhaps the authors should discuss in more detail when the modified procedure is especially good or especially bad vis a vis the usual two stage screening procedure.

The authors have written an excellent and very publishable paper on this important topic. It could, however, be improved if the usual language and notation of epidemiologic screening (e.g., p_v+ , p_v- , sensitivity, specificity) were used.

The paper by Chakrabarty gives an excellent overview of the major features of the new NHIS design and their potential impact. Particularly exciting are: the use of two PSU's per stratum which will improve variance estimation; the use of area sample lists and the use of 4 independent sets of PSU's which will permit NHIS to serve as a frame for other population based NCHS surveys; and oversampling of the black population.

It was extremely disconcerting to learn of the 25% sample reduction. I hope that this will eventually be restored, and it would be interesting to learn what the prospects are for such a restoration.