

## 4.5 Hypnosis and telescoping: some rediscovered facts

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During one of the many discussions of the validity problem at the New Orleans symposium it was suggested that the answer might be found in hypnosis. Although the suggestion was made partially in jest, the more I thought about it, the more it appeared possibly to have some merit.

Since the heart of the validity problem has to do with the fallibility of human memory and since numerous clinical studies have demonstrated the ability of hypnotized subjects to recover lost memories while in the hypnotic trance, it seemed that hypnosis might indeed represent a potentially fruitful area of inquiry.

When I was approached about six months ago and asked whether there was anything I cared to contribute to this year's programme I recalled the hypnosis idea and suggested that it might be instructive to prepare a paper on the subject, provided that I could reserve the right to withdraw should my findings prove to be negative.

The suggestion was forwarded to the Programme Committee, who concluded that they would indeed like to have such a paper, but that the subject matter was sufficiently interesting and provocative that they would like to have it presented regardless of the findings. With some trepidation, I accepted the assignment.

As it turned out, my concern was not unfounded. Now, several months later, I find myself in the awkward position of advising my audience not to do something which most would never have considered doing in the first place. I would like to explain why.

At the time I accepted the assignment, I thought that I might be able to use hypnosis to investigate the *telescoping* phenomenon, which, I believe, explains the reason why the recent reading method produces magazine audience estimates which are systematically greater than those produced by the through-the-book method.

However before proceeding with the experiment I had planned, I first conducted a search of the literature, which convinced me that no study involving hypnosis could ever produce findings that could be unequivocally interpreted. And so, I aborted my plans.

The reason that the results would have been equivocal is that, although it is true that hypnotized subjects can frequently recall events that have occurred and are not remembered in the waking state, hypnotized subjects are also more likely to fabricate such events than are subjects in the non-trance state.

Stalnaker and Riddle, writing in the *Journal of General Psychology*, reached that conclusion over 50 years ago. The conclusion has since been experimentally confirmed as recently as 1983 by Sanders and Simmons, in an article entitled 'Use of Hypnosis to Enhance Eyewitness Accuracy: Does It Work?'

The tendency for hypnotized subjects to fabricate has recently been a source of considerable controversy in the US courts. In 1982, the California Supreme Court ruled that the testimony of eyewitnesses in criminal trials is inadmissible if the witness was previously hypnotized for the purpose of enhancing the recollection of events. The decision was based upon the concern of the court that fabrication in the hypnotic trance might later distort the witness's non-trance recollections in the court room.

Until two weeks ago I had intended to elaborate on these points, to make some closing remarks, to put hypnosis forever behind me as a validating device, and to say nothing more about telescoping beyond what I had already said.

But that was before I happened to hear an address by Barbara Bailar of the US Bureau of the Census delivered to an ARF Workshop on Research Quality and entitled 'Something Is Not Better Than Nothing'.

In the course of the speech, I was startled to hear Ms Bailar's strong criticism of studies which require respondents to estimate the recency with which a particular event has occurred. Let me quote:

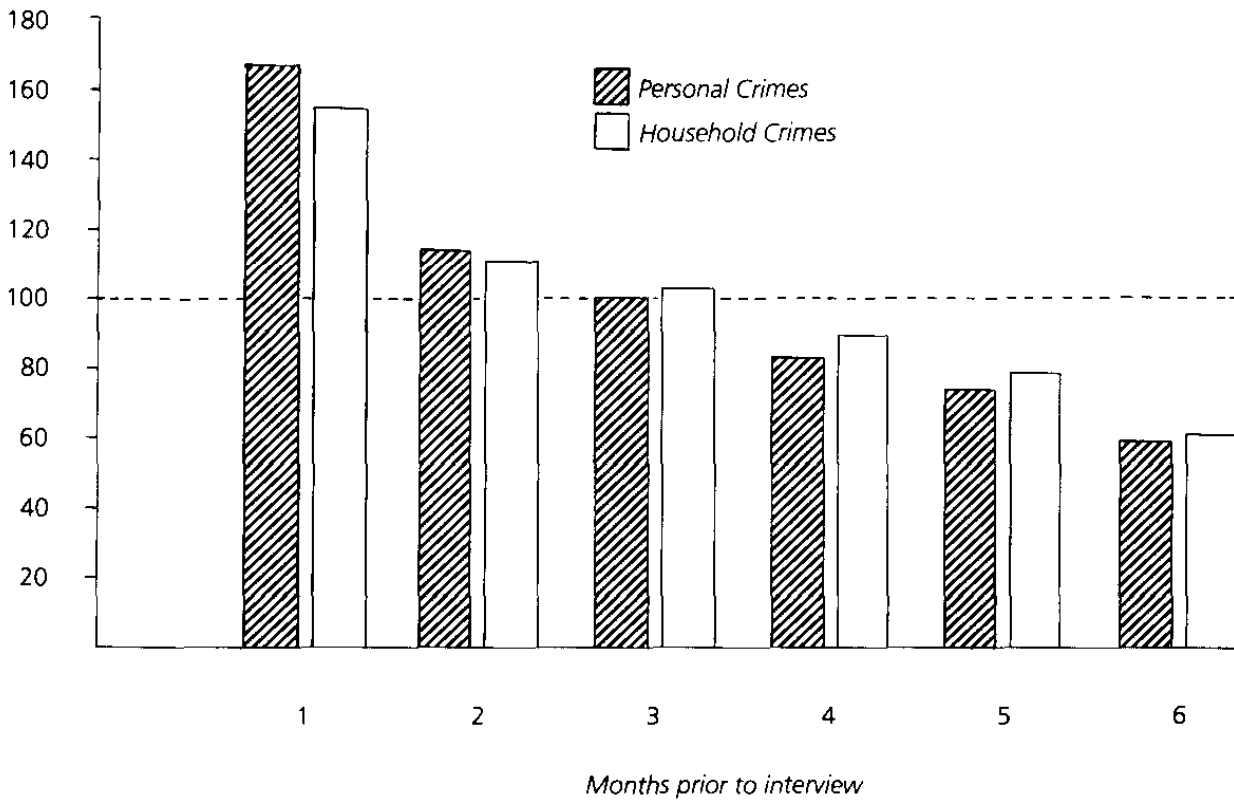
"In every study the Bureau has done where we try to get people to place events in time there is a recall problem. We ask respondents if something occurred within a given time frame and if it did we ask them to place it, often by month, within that time period." (Does that sound like a familiar magazine audience estimating procedure?) "The results show that the placement is poor, and this is true for surveys on crime, on income, on expenditure."

To illustrate the point, she reported the results of a crime victimization study which the Bureau had conducted. Respondents were asked about crimes that may have been committed against them in the previous six months. Then when a crime incident was reported, the interviewer asked for the month in which it took place.

Ms Bailar then showed a table (see **Figure 1**) showing the reported rates of victimization for three classes of personal crime (crimes of violence, assault and

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**FIGURE 1**  
**Victimization rates for each of six months prior to interview**  
*(Indexed to six month average)*



Source: Barbara A. Bailar  
US Bureau of Census

personal theft) and three classes of household crime (burglary, household larceny, and motor vehicle theft). I have taken the data from the paper, simplified it for purposes of this paper, and displayed it in the form of the bar chart above. The data are presented only for those respondents claiming to have been victimized in the past six months. The left-hand bars show the reported rates of personal crime for each of the six monthly periods comprising the recall interval, and the right-hand bars represent the reported rates of household crime. In every instance the height of the bar signifies the crime rate as reported by the respondents for each of the six months preceding the interview.

The point of the chart is this. Since the design of the study was such that the reported monthly crime rate should have been the same regardless of the time lapse from the month of the interview, any significant deviation from the straight line relationship shown by the dotted line on the chart must necessarily be attributable to the inability of the respondent to estimate the recency of the event.

In sharp contrast to what one would expect were the respondents able to estimate the recency of these events in an unbiased fashion, what one sees is a heaping up of the reported crimes in the months closer to the month of interview. And in the most recent

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month we find a 55 to 65 percent higher rate of reported crime than would be expected had the respondents' recency estimates been unbiased.

This is precisely the result one would expect to find under conditions of telescoping if respondents were overestimating the recency of the crime, believing that it occurred more recently than it actually did.

I feel obliged to call attention to the fact that there is another interpretation that could be offered to explain this relationship. Perhaps the decline in reported crimes, which occurs as a function of the time lapse between the reported date of the crime and the date of the interview, is caused simply by a failure of some of the victims to pass the six month screen, imagining that the crime actually occurred seven or more months ago, or having forgotten the incident completely.

Such failure would be most common among respondents whose victimization had occurred six months prior to the interview and least common among those who had been victimized only a month prior.

The implication of this interpretation so far as magazine audience measurement is concerned is that the already incredibly high recent reading audience estimates may actually be understated.

I do not consider this interpretation to be a plausible one, however, because to accept it one would also have to accept the conclusion that about a third of those who had been victimized two months prior to the interview mistakenly believed the incident to have occurred at least seven months prior, or not at all.

The day after the speech I telephoned Ms Bailar at the Census Bureau in Washington and asked whether she had any other examples she could share with me. She said that she had and then went on to volunteer that the Bureau had been aware of what she called this telescoping problem for over 20 years. I was surprised to hear this since she professed neither to be aware of the recent reading controversy nor of the work of the broadcast measurement researchers whom I thought had originally coined the term.

A few days later I received in the mail a report of some experimental work that was completed by the Bureau in 1964 which investigated the response errors associated with recency estimation of past month

residential alterations and repairs. The report concluded that volume estimates based on reports of past month behaviour produced serious overestimates.

At the New Orleans symposium I demonstrated that the recency method produces overestimates of broadcast ratings. And I have just shared my discovery that the US Bureau of the Census has reported the same phenomenon in subjects as diverse as crime and home repair. In the two years that have passed since New Orleans I have also been told of other proprietary studies, involving clothing, toiletries, cosmetics, and other package goods which have reached the same conclusion.

With the weight of evidence demonstrating that the recency method produces serious overestimates for a wide variety of human activities, is it not presumptuous to believe that magazine audience estimation is an exception?

Methinks the Recent Reading Emperor has no clothes, and it is time that someone blew the whistle on him!

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