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Comments of Philip S. Schaenman, President, TriData, for the Fire-Safe Cigarette Technical Study Group Meeting, September 10-11, 1987.

My name is Philip Schaenman. I am president of TriData Corporation, which specializes in a wide range of fire protection studies including fire data analysis and public fire education. Previously I was Associate Administrator of the United States Fire Administration in charge of the National Fire Data Center. I was a technical reviewer of the John Hall/NFPA and Bea Harwood/CPSC technical reports for this study group. I think they both did an excellent job in responding to my comments (though I still have major disagreement with Hall's methodology.)

TriData's clients are in both the public and private sector, and for the record include the US Fire Administration, the Consumer Product Safety Commission, and the tobacco industry. We pride ourselves on the quality and integrity of our work, and I would like our remarks to stand on their technical merit.

1. Overall Comments

The Technical Study Group is to be congratulated for a superb effort and important contribution. I do have a bone to pick, however, with the draft report's summary of fire statistics. I also am concerned that the TSG's final report will be misread or misconstrued in certain critical respects.

Some of the statements in the draft TSG report interpreting the pertinent fire data are incorrect, and the way some other data is presented may give a misleading impression of the fire problem. By a combination of errors and omissions, an exaggerated view is drawn of the careless-smoking fire problem. That problem is serious enough without being overdrawn. Careless smoking remains the leading cause of fire deaths in the United States. Even if it were in second or third place, there would still be plenty of justification for focusing on the problem. By exaggerating the data, there are two potential harms: the report's credibility is weakened, and the results are apt to be used inappropriately in making fire policy.

Now, let's get down to specifics. I'll make my comments by page number in the draft summary report dated August 20.

Page 6 -- and this is where I have most of my comments: The number quoted as "serious injuries" should be just "injuries." Injuries to be reported to NFPA or NFIRS are supposed to be those requiring medical attention. It is not clear exactly where the line is drawn in practice. The injury data are among the softer pieces of information we have. We have no accurate idea of the severity of the injuries reported to NFPA's survey or the National Fire Incident Reporting System. There are only two levels of the severity data element in NFIRS: alive or dead. We do know "deposition of the victim" (how the victim was handled, e.g. whether transported to a hospital), but that may miss some severe injuries and include minor ones. The number of victims taken to hospitals is much smaller than the number shown in the report. It thus is inappropriate to use the word serious to modify "injuries." I would suggest that that modifier be deleted.

Also, you might note that the injuries quoted are mostly firefighter injuries (100,000 of the 130,000), and the deaths mostly civilian deaths (all but 100). I would suggest breaking out the firefighter deaths and injuries separately.

2. Data for the years 1982-1985 do not reflect a stabilized fire situation, in my opinion. They declined through 1984 and then rose in 1985. 1986, just published by NFPA, shows a drop from 1985. There is no basis to assume that there has been a plateauing of fire deaths that will persist into the future. They could go up or down. There have been previous 'plateaus' in the data after which the long-term downward trend was resumed. There have also been plateaus like the mid-1970s after which there was an increase in deaths. Perhaps you should simply include a plot of the last ten years of fire deaths and fire incidents, and let readers judge for themselves. It is not good statistical practice to pass a line through the last few points in a time series and assume that that is where we are heading. John Hall wove a plausible scenario of why deaths and fire incidents might stabilize, but there are other scenarios that are also plausible that could argue they will go up or down. One doesn't simply balance those off and say, "We'll assume it is level."

3. Also in the first paragraph, you note that the United States has a higher fire-death rate than most other nations and cite some of my reports as the source. What the reports also show is that many peoples of the world have been able to achieve half our fire-death rate more by focusing on people's carefulness through public education -- which is not totally believed in this country. They tend to put an onus on people who start fires, even by accident. The fire community should not stop and wait for a

fire safe cigarette to be perfected. I strongly recommend that the TSG call for continued action on smoke detectors, public education and home sprinklers as well as a continuation of the fire safe cigarette research. Surely it is premature to suggest that we have a panacea for the careless smoking problem, and I wouldn't want other prevention strategies to slow down until we do.

Back to the data:

4. In the second paragraph on page 6 it says that cigarette ignition of furniture and mattresses is by far the single leading cause of fire death. That is no longer true. It is still the leading cause, but not by far. And in the two extremes of population (urban and rural) where the fire death rate is highest, careless smoking is not the leading cause. In very rural areas, heating has surpassed smoking, and in the urban old northeastern and north central cities with highest fire death rates, arson has passed careless smoking, according to data from NFIRS.

5. At the end of paragraph 2, page 6 it is stated that cigarettes start a constant percentage of fires and are thus a continuing threat. This statement is not literally true even for "all fires," but more importantly, it is not true for residential fires, where the majority of the deaths occur. There has been a very sharp drop in the percent of residential fires involving cigarettes. Using the NFPA data in the final report to the TSG and NFPA national estimates, the percentage went from 10.1% in 1980 to 7.9% in 1984. Using slightly older CPSC data that had been provided to the IAFC ad hoc committee that studied the same problem, the drop was comparable. Using the US Fire Administration's approach to categorizing causes, the drop from 1980 to 1984 was from 10.5% to 7.4%.

Those are major movements, not at all constant percentages. Further, in terms of absolute numbers, there has been almost a 50% drop over the period 1978 - 1985, and it is even higher on a per capita basis because our population has grown. It is a major omission of the TSG final report to ignore the enormous drop in smoking fires when characterizing the smoking fire problem.

Let me restate, because I am concerned about this being taken out of context, that the large drop in the careless smoking problem in no way makes it an unimportant problem. There is plenty of good reason to justify trying to solve the cigarette fire problem. There are a variety of forces and approaches to reducing the problem that have apparently been succeeding over the past decade. Making the product safer certainly merits consideration, too. But the policy decision regarding its use should take into account the previous history and trend. The other factors having an effect on the problem should not be ignored. Neither should the magnitude of the problem be exaggerated.

6. The third paragraph on page 6 says that the risk of death from fire is increasing. What is known is that the number of fire deaths per thousand reported fires is increasing. What is not known is whether the risk per ignition is increasing. It may well be that some or most of the increase in deaths per fire comes from the reductions in the denominator of minor fires where smoke detectors were present and the fire got detected early. (Residential fires have gone down much more sharply than residential fire deaths over the last decade.)

In light of the uncertainty that clearly exists on this point, I would suggest that the statement concerning per fire risk be deleted from the final report.

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Attached is a rewritten copy of pg. 6 as I believe it should read in light of the points I made.

7. On page 19, I would shorten the title to "Commercial Feasibility." The section really does not "analyze" the feasibility of altering pertinent characteristics, but rather lists potential problem areas. The title is parallel to the other sections, but the content is not parallel because the technical study did not include a detailed analysis of manufacturing feasibility or the many other factors listed in this section.

8. Also on page nineteen, Section B is titled "Monitoring Success." Perhaps a different title such as "Monitoring Ignition Propensity" might be clearer and more accurate.

9. Page 20--I was puzzled by why the discussion of the field test feasibility was the main discussion under the topic sentence in bold that deals with "tracking incidence of cigarette-initiated fires." Why wouldn't the prime monitoring of cigarette fires be done as it has in the past, with a combination of the NFPA annual survey and the USFA NFIRS data on causes of fires? If all cigarettes on the market were to be changed toward lower ignition propensity, the overall statistics on careless-smoking fires should reflect the change if it is significant. Of course, other real-world changes could affect the overall fire picture, too, such as the number of cigarettes smoked, influences on carefulness of smokers, and performance of smoke detectors. But still, the overall statistic would be the prime measure, with statistical analyses to separate out other impacts.

It would take a much larger sample than is contemplated for the CPSC field work to detect changes among cigarettes with confidence, after taking out varying sales by brand and area. The CPSC feasibility study showed that you can indeed collect many details on fires involving cigarettes, including the brand and socioeconomic characteristics of the smokers. It also showed that you could reach wrong conclusions from small non-random samples. The point is, if the impact is large, the current statistics will suffice. If the impact is small, a large, very carefully drawn field sample will be needed to make statistical inferences. I do believe the field study can be useful, but not as the main monitoring tool.

10. The discussion on page 20, paragraph 4, brings up the fact that half of the smoking fires anticipated from the nine cities were not reported during the course of the study. It will be important in the future to know whether this half-reporting was a statistical fluke (i.e., many fewer fires occurred than normally occur during the period), whether the need to report details held back reporting coverage (always a possibility), or whether when asked to follow up on fires called "cigarette fires," many that might have been thrown in that cause bin without a firm foundation were now being called something else. It would be desirable for any follow-up study to pay attention to resolving that issue. It would be useful to know whether most of the fires called "smoking fires" were indeed that, or whether the numbers were inflated. I might add that the same type of problem exists for electrical fires, "children playing" fires, and some other cause categories. Helping to resolve questions concerning the validity of the data would seem an important part of a recommended follow-on.

11. On page 21, I think Table 7 should be deleted as unnecessary and possibly misleading. The potential reduction in cigarette losses is the entire baseline, not an arbitrary portion of it. The table shows the kinds of numbers that would result if one of the "best" experimental cigarettes were commercially feasible. But if cigarette number 106 is not commercially feasible, or is otherwise shown to be inappropriate -- because, for example it cannot be smoked -- then the potential reduction is something less. Having the table there may well imply to the average reader, or to the reader who skims, that this is the expected reduction, when that is not what is meant to be described. As I read the report and backup materials, and listen to the discussion at these meetings, it is clear that the TSG is far from being in a position to say that a particular modified cigarette should be introduced. "Technical feasibility" has been demonstrated in one sense, but "technical feasibility" of a commercially feasible cigarette has not. In light of the above comments, Table 7 does not seem to serve a useful purpose and is likely to mislead many readers. I also am concerned that many readers of the second paragraph of page 4 of the draft report will be misread for the same reasons. I would recommend that that paragraph be deleted, so that readers can focus on the "particular conclusions" of the study that follow, which seem well stated.

Also, incidentally, I might also note that Table 7 assumes that deaths from careless smoking will continue at exactly the same level as at present for the next 10 years (1,500 times 10). As I noted earlier, there is no support for that assumption other than the sheer assertion that the latest years' 1983-1985 data stop the long term trend.



12. There is an omission from the report of the important research done by Rhyne and Spears on the use of logistics curves to analyze propensity of upholstered materials to ignite. The concepts were left out of the discussion in Hall's report, which is probably why they did not surface in the summary. But they could be important if commercial feasibility limits or precludes potential improvements in ignition propensity. I don't understand why this research was ignored, and suggest it be considered.

Thank you for considering my comments. I hope they are useful.