# INCREASING THE EFFECTIVENESS OF HOME ENERGY AUDITORS: A FIELD STUDY

Marti Hope Gonzales, Elliot Aronson, and Mark Costanzo
University of California at Santa Cruz

## ABSTRACT

We have argued that the R.C.S. Home Energy Audit program could be made more effective if auditors were trained to be better communicators. Specifically, if auditors could communicate their findings to homeowners in a manner consistent with recent social psychological findings, homeowners would be motivated to follow their recommendations and would take greater action to use energy more efficiently.

A quasi-experiment was conducted in which nine experienced home energy auditors attended two one-day workshops in which they were trained to: (1) communicate vividly; (2) personalize their recommendations; (3) induce commitment; and (4) frame their recommendations in terms of "loss" rather than "gain."

The effectiveness of these auditors was compared with the effectiveness of a control group of experienced auditors who did not receive specific training in these communication skills. Telephone interviews conducted after the home energy audits revealed that twice as many customers visited by trained auditors reported that they had already applied for a zero-interest loan (ZIPLOAN) retrofit finance program. These results were statistically significant. In addition, customers in the experimental condition reported a greater likelihood of acting on the auditors' recommendations than did customers in the control condition. These results, too, were statistically significant. Finally, utility records assembled four months after the last telephone interview showed that a significantly greater proportion of customers served by trained auditors actually applied for participation in retrofit finance programs.

# INCREASING THE EFFECTIVENESS OF HOME ENERGY AUDITORS: A FIELD STUDY[1]

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While the development of alternative sources of energy (e.g., solar, wind, and nuclear power) represents a potential alternative to fossil fuels, many call for continued efforts to promote long-range conservation. One such effort was the establishment of the Residential Conservation Service (R.C.S.) Program when the National Energy Conservation Policy Act (NECPA) was enacted by congress in 1978. Among the services mandated by the act was local utilities' provision of free or low cost home energy audits to customers.

As of today, however, the R.C.S. Home Audit Program, designed to encourage homeowners to make capital investments to conserve energy, has not been notably successful. In 1981 and 1982, less than 6% of eligible households nationwide took advantage of offers of home energy audits. In California, the audit request rate was smaller still (U.S. Department of Energy, 1984). Other research has shown that of the small percentage of homeowners who request audits, a minority actually take the actions recommended by the auditors (Hirst, Berry & Soderstrom, 1981). Even after considerable investment of resources on the part of utility companies, those consumers who request an audit are reluctant to take action on the auditors' recommendations.

This consumer inertia is particularly puzzling, for improving the energy efficiency of the home is in the homeowners' economic self-interest and is consistent with national interest. It is our contention that part of the reason is psychological--the consumer is in a state of inertia caused by a mixture of habit, insufficient understanding of and insufficient commitment to energy conservation (see Stern & Aronson, 1984).

The auditor is a pivotal figure in energy conservation because he or she can be a vehicle for effective face-to-face communication about the benefits of conservation activities. Previous studies have shown that certain auditor attributes (e.g., gender, degree of extroversion, length of time on the job, etc.) can influence the effectiveness of the audit (Brown, Berry, White & Zeidler, 1986). We believe that a significant improvement in the

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effectiveness of the Home Audit Program could be brought about if auditors were trained to capture homeowners' attention, penetrate psychological barriers caused by adherence to old habits, and increase their sense of personal involvement.

The specific relevance of social psychological research on face-to-face communication to energy conservation has been explored in depth in recent years (Aronson et al., 1985; Aronson & Yates, 1985; Costanzo, Archer, Aronson & Pettigrew, 1986; Stern & Aronson, 1984; Yates & Aronson, 1983), but there has been little effort to experimentally test the relevance of this research in a field setting. Our plan was to conduct a field study in which we would train a small sample of home energy auditors to communicate their recommendations in a manner consistent with the social psychological principles of communication described below. We would then compare their effectiveness with that of a control group of equally competent auditors who were not trained in these social psychological aspects of communication and persuasion. The following is a description of the communication principles upon which the training was based.

Vivid Information. Data summaries and statistics are often difficult for the average homeowner to assimilate. Although less representative than aggregate information, vivid examples and personalized case studies can be more successful in capturing and holding individuals' attention. For example, someone planning to purchase a new television set is less influenced by product rating summaries in Consumer Reports than by the story Uncle Harry tells about the money he lost trying to keep his Sony television working, despite the fact that Uncle Harry's account increases the data base by only one report.

Numerous researchers have documented the persuasive impact of case histories over statistical information (Dickson, 1982; Hamill, Wilson & Nisbett, 1980; Nisbett & Borgida, 1975). This is because they're concrete, personal, and more relevant than summaries. To extrapolate to energy conservation, there is reason to believe that providing statistical summaries of potential savings will be ineffective at inducing conservation behavior in homeowners. An alternative would be to use vivid case histories of neighbors or other local "superconservers" who have saved more energy and money than average. These portraits should include concrete examples of measures taken and the benefits that followed. "Superconservers" are low probability cases, but they provide vivid and dramatic exceptions to the dry statistical rule, and have a disproportionately large impact on the behavior of energy consumers.

Similarly, language which conjures up a vivid and memorable image has been shown to capture and hold the individual's attention, and to influence behavior (Nisbett & Ross, 1980). For example, an auditor might point to the loss of warm air from around and beneath the doors of a home, and recommend weatherstripping as a cost effective remedy. Some homeowners would follow the recommendations but far more would act in response to a vivid presentation such as the following: "If you were to add up all the cracks around and under the doors of your home, you'd have the equivalent of a hole the size of a large grapefruit in your living room wall. Just think about all the heat that would escape from a hole that size... And your attic doesn't have any insulation. We call that a 'naked' attic. It's as if your home is facing winter

not just without an overcoat, but without any clothing, at all." The fact that the cracks and a grapefruit encompass the same area is of interest to an engineer; but in the mind of the average homeowner, the grapefruit will loom larger than the cracks around the door. Similarly, insulation is something with which most people lack experience. But the idea of being naked in the winter is something that forces attention and increases the probability of action.

Also, presentation of statistical data often fails to induce conservation behavior, in part because individuals tend to see themselves as exceptions to statistical norms and averages. The impact of statistical data can be enhanced by making them personally relevant. We believe that little is gained when auditors simply leave the homeowner with a computer summary of potential savings associted with various conservation strategies. Rather, auditors will be more effective if trained to make home audit presentations personally relevant to homeowners (e.g., by using the customer's own utility bills to illustrate current losses or potential savings).

Commitment. Research has demonstrated that people who choose a course of action, commit themselves publicly, and/or invest time, effort or money in that course of action come to see themselves as "believers," as people committed to the content and substance of that action. Commitment increases the probability that interest will translate into action (Pallak, Cook & Sullivan, 1980). Further, small investments often pave the way to larger investments; individuals who fulfill small commitments are more likely than others to fulfill larger, ongoing commitments later on (Freedman & Frasier, 1966; Schwartz, 1970).

The audit itself is an especially critical point in the influence process. Homeowners who schedule energy audits are taking the first step on the path to further energy conservation behaviors because scheduling an audit is, in itself, a small commitment. Through an understanding of the psychology of commitment, further steps can be taken during the audit to ensure that homeowners continue to move forward in the direction of conservation.

A promising strategy of commitment is to get homeowners more involved in the process of the home audit. Homeowners might accompany auditors on their rounds, and assist in taking measurements, reading meters, and so on. They might even be induced to climb a ladder and peer into an uninsulated attic, or bend down and measure cracks underneath the doors. Also, auditors might try to obtain a verbal commitment from homeowners to make the recommended changes. Procedures designed to increase commitment may seem inconsequential. However, they have been shown in other contexts to create a self-perception of concern, and to increase the probability of subsequent action.

Framing. Because the human mind is not a computer, the particular way in which a statement is "framed" will have a major effect on the behavior of the perceiver. For example, Kahneman, Slovic and Tversky (1982) have shown that people experience much stronger reactions at the prospect of losing a certain sum of money than at the prospect of gaining the same sum of money--even though the logical status of the two events is identical. And individuals are more willing to take a risk to minimize or avoid a loss than they are if the goal of the gamble is to secure a positive outcome (Kahneman & Tversky, 1970).

We would expect that people who fear a loss would be more open to innovation, while those who are concerned with enhancing savings would be more inclined to eschew risk or change. The typical conservation campaign strategy with its greater emphasis on savings may inadvertently discourage people from changing their energy-related behaviors. Thus, auditors might be more effective when they present their recommendations in terms of energy and money lost via inaction than in terms of energy or money gained or saved via action.

We hypothesized that auditors trained to make use of the above principles during home energy audits would be more effective at inducing homeowners to take energy conserving actions than would the control group of equally experienced auditors. Our intent was to measure three dependent variables: (1) meter readings of audited dwellings before and after the home energy audits; (2) applications from audited homeowners for participation in two retrofit finance programs--a zero-interest loan (ZIPLOAN) and a rebate finance program (CASHBACK); and (3) telephone surveys designed to provide demographic data, homeowner ratings of auditor effectiveness, the number and type of actions taken by the homeowner since the audit, and whether homeowners had applied for participation in the ZIPLOAN and/or CASHBACK finance programs. As of this writing, analyses of consumption data are not complete, and discussion will be limited to customer self-report data obtained via telephone interviews, and to utility records of actual requests for program participation.

## METHOD

### Participants

A total of eighteen home energy auditors employed by a California utility comprised the initial participant pool. Nine auditors from four districts served by the utility were assigned to a group which received training. Nine auditors from four districts were chosen to serve as a control group. No consideration was given to past performance of the auditors on assigning them to either the experimental or control condition.

### Design and Procedure

Although home energy auditors typically work independently, the districts from which auditors were selected varied greatly in the opportunities afforded auditors to interact with one another. Frequent contact increased the probability that control auditors would acquire information from the experimental auditors, especially if the intervention involved interesting or potentially useful information. For this reason, all auditors in each of three service districts were assigned to either the experimental or control condition. Because strict random assignment of individual auditors was not used, this study is not a controlled experiment, but a quasi-experiment employing a nonequivalent control group design.

The Workshop Training. The training itself consisted of two workshops in which social psychological principles were presented, discussed, modified through consultation with the auditors, tried out in a role-playing format, and refined. Our aim was to use the most appropriate tools available consistent with the prior experience of the auditors--we wanted the auditors to feel comfortable with the new procedures, and to modify the most powerful

strategies in order to make them reasonable in the context of their daily encounters with homeowners.

All nine experimental auditors were in attendance at the first workshop held in May 1985. It became clear that a few of our specific recommendations were extensions of practices that some of the auditors were already using. For example, many of the auditors engaged in some form of inducing commitment and accountability during audit "closings." It was also clear that the training provided them with a useful conceptual foundation for their actions, as well as some concrete suggestions that helped to improve their procedures. A week following the first workshop, participants were sent a summary of the basic principles covered in the training.

The second workshop was conducted four months later. Only five of the original nine auditors were in attendance. The attrition was caused by normal turnover and life events: Between May and September, one auditor was promoted, one had resigned his job, one was no longer conducting audits on a regular basis, and one missed the workshop for medical reasons.

The purposes of the second workshop were twofold: to respond to any problems the auditors might have encountered after the first workshop, and to bolster previous training. Most of the auditors were finding it difficult to practice our procedures, in part due to an increased workload since the first workshop. They reported that they often felt "rushed," and tended to resort to the comfort and efficiency of their prior procedures--at least some of the time. Together we reached a reasonable compromise; we suggested ways of reducing the length of our recommended procedure, utilizing it at the close of the audit. While this was a reasonable and necessary compromise, it weakened the social psychological impact of the procedure upon which we had all originally agreed.

Telephone Interviews. Following the first workshop, auditors in both the control and experimental conditions provided us with worksheets on a bi-weekly basis. These worksheets contained names, telephone numbers, and audit dates for homeowners. In the case of experimental auditors, worksheets also contained an approximate date by which audit recipients committed to take action on the auditors' recommendations.

The sample of audit recipients in this study was drawn from audit recipients included on the auditor worksheets. Experimental audit recipients were contacted via telephone approximately 1-2 weeks after the date they committed to take action. Control recipients were yoked to experimental recipients. That is, experimental and control audit recipients were matched according to the dates on which home audits were conducted, and were contacted during the same telephone session. This ensured that control recipients contacted had the same length of time in which to take action as the experimental audit recipients.

Telephone interviews were conducted by one of three trained interviewers. Each interview lasted approximately 5-10 minutes. Protocol items included both fixed-format and free response questions.

Audit recipients were excluded from the study if: (1) three unsuccessful attempts were made to contact the potential respondent; (2) the recipient declined to be interviewed; (3) no appropriate match was available for a potential respondent; or (4) audits were conducted or commitment dates fell after the data collection phase of the study. A total of 408 audit recipients were interviewed. Not all of the 204 experimental and 204 control customers, however, responded to all questions on the protocol. (For example, some respondents refused to answer income and education questions.) Thus, the number of recipients used in the analyses varied, depending on the questions of interest.

## RESULTS

### Preliminary Analyses

Because a non-equivalent control group design was employed, we initially conducted a series of analyses to detect any systematic differences in the demographic characteristics of audit recipients in the experimental and control conditions. Table I presents income and education distributions of audit recipients for both conditions. As indicated, analyses yielded non-significant omnibus chi-squares. Neither income nor education distributions in the experimental and control conditions differed significantly from one another. It is therefore highly unlikely that differences on various dependent measures were due to systematic differences in audit recipient 'demographic characteristics such as income and education.

Table I. Audit recipient income and education by condition.

| CATEGORY | EXPERIMENTAL | | CONTROL | |
|---|---|---|---|---|
| Income (a) | Number | Percent | Number | Percent |
| Under $10,000 per year | 7 | 4.0 | 6 | 3.4 |
| $10,000 to $19,999 per year | 26 | 14.8 | 18 | 10.3 |
| $20,000 to $29,999 per year | 35 | 19.9 | 35 | 20.0 |
| $30,000 to $39,000 per year | 33 | 18.8 | 40 | 22.9 |
| $40,000 to $49,999 per year | 18 | 10.2 | 33 | 18.9 |
| $50,000 and over per year | 57 | 32.4 | 43 | 24.6 |
| Education (b) | Number | Percent | Number | Percent |
| High school or less | 42 | 21.1 | 41 | 21.1 |
| Trade school | 1 | 0.5 | 0 | 0.0 |
| Some college | 69 | 34.7 | 77 | 39.7 |
| College degree | 58 | 29.1 | 52 | 26.8 |
| Graduate degree | 29 | 14.6 | 24 | 12.4 |

(a) N = 351, Chi-square = 8.572, p = .13

(b) N = 393, Chi-square = 2.186, p = .70

Systematic differences in the dependent measures used in this design could have been due to two additional factors--the effects of auditor workshop training, and/or systematic differences in the types of specific recommendations made by auditors. To investigate the latter possibility, we compared the eleven specific types of recommendations made by auditors in each of the two conditions. Table II summarizes the results of these comparisons. For the most part, experimental and control customers did not differ in their recall of the auditors' specific recommendations. Of the eleven comparisons, only two differences were statistically significant.

Table II. Percentage of audit recipients provided specific recommendations.

| RECOMMENDATION | EXPERIMENTAL (a) | | CONTROL (a) | | SIGNIFICANCE |
|---|---|---|---|---|---|
| | Number | Percent | Number | Percent | |
| Ceiling Insulation | 116 | 56.9 | 109 | 53.4 | n.s. |
| Weatherstripping | 126 | 61.8 | 113 | 55.4 | n.s. |
| Caulking | 84 | 41.2 | 65 | 31.9 | n.s. |
| Hot Water Heater Blanket | 71 | 34.8 | 59 | 29.9 | n.s. |
| Low-Flow Showerhead | 79 | 38.7 | 59 | 28.9 | $p < .05$ |
| Duct Insulation | 13 | 6.4 | 13 | 6.4 | n.s. |
| Wall Insulation | 83 | 40.7 | 67 | 32.8 | n.s. |
| Floor Insulation | 67 | 32.8 | 34 | 16.7 | $p < .05$ |
| Thermal Windows | 64 | 31.4 | 54 | 26.5 | n.s. |
| Clock Thermostat | 27 | 13.2 | 25 | 12.3 | n.s. |
| Fluorescent Lighting | 52 | 25.5 | 39 | 19.1 | n.s. |

(a) N = 204

The results of the preliminary analyses suggest that differences in the effectiveness of control and experimental auditors cannot be attributed to confounds such as the characteristics of the audit recipients, or to differential recommendations made to customers by the auditors.

## The Effects of Auditor Training

We conducted several analyses to assess the effects of the auditor training workshops. Dependent variable measures included customer ratings of the auditors' presentations, customer self-reported likelihood of making recommended changes, and reported and actual applications for CASHBACK rebate and ZIPLOAN programs.

Stepwise regression analyses controlling for recipient income and education, and for auditor effectiveness prior to workshop training yielded non-significant F-ratios for both auditor clarity and knowledge ratings provided by recipients in telephone interviews. As rated by audit recipients, experimental and control auditors did not differ significantly in the clarity of their audit presentations, or in their knowledge of the subject matter covered. Table III summarizes these findings.

Table III. Ratings of auditor clarity and knowledge.

| AUDIT RECIPIENT RATINGS | EXPERIMENTAL | CONTROL | | |
|---|---|---|---|---|
| Mean Clarity Rating (a) | 8.749 (sd=1.346) | 8.805 (sd=1.231) | F = .129 | n.s. |
| Mean Knowledge Rating (b) | 8.583 (sd=1.522) | 8.643 (sd=1.388) | F = .848 | n.s. |

(a) N = 405

(b) N = 398

Clarity Question

"How clear and understandable was the auditor's presentation on a '1' to '10' scale, '1' being 'not at all clear or understandable' and '10' being 'extremely clear and understandable'?"

Knowledge Question

"How knowledgeable was the auditor on a '1' to '10' scale, '1' being 'not at all knowledgeable' and '10' being 'extremely knowledgeable'?"

In the telephone interviews, the questions most pertinent to our hypotheses addressed: (1) customer reports of the probability of taking recommended actions, and (2) customer reports of CASHBACK and ZIPLOAN applications. Stepwise multiple regression analyses controlling for recipient income and education, and for auditor effectiveness prior to the workshops yielded a significant ($p < .05$) difference between experimental and control customers' estimates of the probability of acting on the auditors' recommendations. Experimental audit recipients reported a significantly greater likelihood of following through on the auditors' recommendations. Please refer to Table IV for these results.

Table IV. Self-reported probability of making recommended changes. (a)

| | EXPERIMENTAL | CONTROL | | |
|---|---|---|---|---|
| Mean Self-Reported Probability | 7.222 (sd=3.422) | 6.094 (sd=3.581) | F = 4.196 | p = .041 |

(a) N = 245

Question:

"Of those changes you have yet to make, how probable is it that you'll make those changes on a '1' to '10' scale, '1' being 'not at all probable' and '10' being 'extremely probable'?"

With regard to reported participation in finance programs, tests of proportions yielded the z-scores in Table V. A significantly ($p = .011$) greater proportion of experimental audit recipients reported that they had applied for a ZIPLOAN. While there was no significant difference between experimental and control recipients on reported CASHBACK rebate requests, results were in the predicted direction.

Table V. Number of audit recipients claiming to have applied for Ziploan or Cashback programs.

| PGandE PROGRAM | EXPERIMENTAL (a) | | CONTROL (a) | | | |
|---|---|---|---|---|---|---|
| | Number | Percent | Number | Percent | | |
| CASHBACK | 45 | 22.3 | 38 | 18.8 | z = .862 | n.s. |
| ZIPLOAN | 28 | 13.9 | 14 | 6.9 | z = 2.282 | p = .011 |
| CASHBACK or ZIPLOAN | 73 | 36.1 | 52 | 25.7 | z = 2.260 | p = .012 |

(a) N = 202

Utility records of actual applications for participation in CASHBACK rebate and ZIPLOAN finance programs were used as a behavioral measure of the success of the training program. Tests of proportions yielded the z-scores in Table VI. These data represent an increase in participation in both programs over the customer self-report data collected through November 1985, with a relatively greater increase in CASHBACK rebate requests. Based on utility records, significantly more ($p < .001$) customers in the experimental condition applied for CASHBACK rebates to finance retrofits. While experimental customers did not differ significantly from control customers in ZIPLOAN requests, results were in the predicted direction ($p < .06$). Noteworthy is the large proportion of customers in each condition who followed through with CASHBACK rebate and ZIPLOAN requests. This general increase, and the relatively greater increase in CASHBACK requests, was presumably due to the imminent demise of the CASHBACK rebate program and the eventual discontinuation of the ZIPLOAN finance program.

Table VI. Number of audit recipients who actually applied for Ziploan or Cashback programs.

| PGandE PROGRAM | EXPERIMENTAL (a) | | CONTROL (a) | | | |
|---|---|---|---|---|---|---|
| | Number | Percent | Number | Percent | | |
| CASHBACK | 75 | 37.1 | 44 | 21.8 | z = 3.383 | p < .001 |
| ZIPLOAN | 48 | 23.8 | 35 | 17.3 | z = 1.599 | p < .06 |
| CASHBACK or ZIPLOAN | 123 | 60.9 | 79 | 39.1 | z = 4.377 | p < .0001 |

(a) N = 202

## DISCUSSION

The above findings suggest that the auditor training program was a success--that training auditors to communicate vividly, to induce customer commitment, to personalize their presentations, and to frame their recommendations in terms of "loss" rather that "gain" did produce an increase in customer conservation behaviors. However, before we can justifiably draw these conclusions, we must address alternative explanations for the results.

Other factors could have accounted for the results. First, the data collected are for the most part self-report data. It is conceivable that audit recipients may have inflated their statements in order to make themselves "look better" (smarter, more efficient, more patriotic, etc.) in the eyes of the interviewers. Bias in self-report data is always possible. Subjects have been known to overestimate the probability of making a response in the future, and even to mislead interviewers. Thus, the data in Table IV might be questionable. However, the self-report data in Table V were subject to verification, and significant differences in finance program participation obtained when "hard" data (utility records) served as the dependent variable. Moreover, results in Table VI suggest that experimental customers were no more likely to inflate their estimates of the probability of taking action than were control customers. They reported a significantly greater probability of taking the recommended actions, and they generated significantly more requests for finance program participation.

Second, it is conceivable that auditors in the experimental condition tried harder simply because they knew they were getting special training. Were this the case, the greater effectiveness of the experimental auditors may have been due to utility interest implied by the training itself, rather than to the specifics of the training. This seems an unlikely possibility. Many of the auditors with whom we worked viewed the training as an additional burden, rather than a benefit. There had been a recent increase in the number of daily audits they were required to make. While most of the experimental auditors were pleased with the material learned in the workshop, all but one reported (during the second workshop) that the increased time pressure made it difficult to integrate the new material into the established routine of their audits. In addition, if the experimental auditors were trying harder, it is likely that they would be seen by audit recipients as more knowledgeable or as making more clear presentations. Results in Table III suggest that such was not the case.

Third, it is conceivable that the interviewers may have unwittingly biased the respondents in favor of the auditors in the experimental condition. However, the ratings of knowledge and clarity of presentation shown in Table III are much "softer" (easier to bias) than the statement about actions already taken by audit recipients (Table V) or utility company records (Table VI). If, for any reason, recipients were more kindly disposed toward the experimental auditors, it would be more likely to show up here. It did not.

There are reasons to believe the effects of the auditor training could be even greater under more favorable conditions. For example, the workshop training may have served as yet another hassle by auditors who were required to conduct one additional audit per day during the course of the study. Also,

we entered the scene after the auditors had been trained by the utility, and after they had firmly established a routine for conducting audits. It was thus necessary for trained auditors to "unlearn" some of the procedures to which they had already become accustomed. Such complications probably reduced the effects of our intervention.

Given the results above, we believe that further research is in order. While there are few examples of a specific psychological intervention which produces such clear effects, this study raises a number of questions best addressed in a larger experimental study. It would be desirable to train a larger number of home energy auditors at the outset of their employment. This would prevent previous training and established habits from interfering with the intervention.

Also, we trained home energy auditors to use a number of techniques to further customer compliance with their recommendations. It would be of theoretical and practical benefit to determine which single technique or combination of techniques would most efficiently produce the largest effects.

It would also be valuable to determine the extent to which consumers begin to save energy simply as a function of having committed themselves to the decision to conserve energy. That is, once a person has made a firm commitment to conserve (e.g., requesting an audit or signing up to participate in a retrofit finance program), it is possible that he or she might begin to use energy more efficiently even before the retrofits have been completed. Some indication of this phenomenon was demonstrated by Pallak and Cummings (1976), but as Stern and Aronson (1984) have pointed out, this has not yet been firmly established.

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