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Design Options to Test the Effects of Financial Incentives in a Utility Conservation Program: TVA'S Heat Pump Water Heater Program

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DESIGN OPTIONS TO TEST THE EFFECTS OF FINANCIAL INCENTIVES IN A UTILITY CONSERVATION PROGRAM: TVA'S HEAT PUMP WATER HEATER PROGRAM

Linda Berry
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ABSTRACT

The Tennessee Valley Authority, as part of its residential energy conservation program efforts, is interested in determining the effectiveness of offering different levels and types of financial incentives to its residential customers. This report describes and assesses several experimental designs and analysis techniques. Using TVA's Heat Pump Water Heater Program as a carrier, it addresses questions related to financial incentives such as:

1. Do TVA residential customers prefer loans or rebates?

2. What is the incremental effect of financial incentives on conservation investments relative to information-only programs? How does the effect vary with increases in the incentive?

3. How do users of financial incentives differ from nonusers?

The five design alternatives include both household level and distributor level data collection and analysis. The designs are:

1. Random assignment of households to treatments within distributors with matching;

2. Random assignment of households to treatments within distributors without matching;

3. Random assignment of treatments at the distributor level;

4. Random assignment of households to treatments within distributors that are randomly selected from matched blocks of distributors (combination of designs 2 and 3).

5. Nonrandomized design that uses whatever data are available.

These methods are assessed in terms of administrative difficulties, strength of conclusions (both internal and external validity), and the costs of data collection and analysis.

1. INTRODUCTION

1.1 OBJECTIVES

Utilities throughout the country offer financial incentives to their residential customers to encourage installation of energy conservation measures. The Tennessee Valley Authority (TVA, Southeast), Northern States Power Company (Minnesota), Pacific Gas and Electric Company (California), and the Bonneville Power Administration (Pacific Northwest) offer low- or zero-interest loans for residential retrofits. Utilities in several states, especially Texas and Florida, offer rebates for purchase of energy-efficient appliances.

Because little is known about the effectiveness of these financial incentives, TVA asked the Oak Ridge National Laboratory to develop alternative designs to determine the effectiveness of offering different types of financial incentives to its customers. The specific questions of interest are:

(1) Which type of financial incentive is more attractive to TVA residential customers, a loan or a cash rebate (of equivalent value from TVA's perspective)? How much more attractive is one type than the other?

(2) What is the incremental effect (if any) of financial incentives of each type on conservation investments relative to information only programs?

(3) How do users of financial incentives differ from households that do not use incentives? How do loan users differ from rebate users?

The purpose of this report is to describe and evaluate a series of experimental designs and analysis techniques to address these questions. Several options are proposed and the advantages and disadvantages of each are discussed. Although this report focuses on TVA programs, the designs suggested could be used by most large utilities that divide their service area into several districts.

1.2 REASONS FOR CHOOSING THE HEAT PUMP WATER HEATER PROGRAM

Financial incentives, in the form of zero interest and/or market interest loans, already are offered in a number of TVA programs. Rebates have not yet been offered in TVA's residential programs. Utilities that offer rebates have found that they seem to stimulate customer response and that they appeal to different market segments than loans.[1,2]

It might be possible to test the relative effectiveness of loans vs rebates in stimulating customer adoption of conservation devices in a number of TVA programs such as the Home Weatherization, Solar Water Heater, or Wood Heating System options. In this evaluation plan, the Heat Pump Water Heater (HPWH) option is recommended as the vehicle or "carrier" program for testing the effectiveness of various types of financial incentives for several reasons.

First, heat pump water heaters are a new technology in the TVA residential market. While market penetration rates may have reached 1-2% in the best markets for heat pump water heaters (such as Hawaii or parts of Florida),[3] probably no more than several hundred are currently in use in the TVA region.[4] Prior to the TVA program's start in October 1982, HPWHs could be obtained only by mail and installed only by do-it-

yourself homeowners. There was no distribution, installation or service infrastructure. An important goal of the TVA HPWH program is to encourage development of such a distribution and service system.[4] The newness of the HPWH to the TVA market makes it an attractive choice for testing the effects of financial incentives on customer adoption since analysis of program effects is not complicated by prior adoptions of the technology.

A second reason for choosing the HPWH program to test the effect of incentives is that the program staff is interested in evaluation of the program. They welcome and are willing to take an active role in program evaluation activities.

A third reason is that distributors already have chosen to offer different types of incentives. Some distributors offer zero interest loans, some market interest loans, and some a combination of zero and market interest loans. Further, many distributors are not offering the HPWH program at all, which provides a potential no treatment comparison group for evaluation purposes. In contrast, all distributors already offer zero interest loans for the Home Weatherization Program measures.

1.3 MARKET POTENTIAL FOR HEAT PUMP WATER HEATERS

In general, heat pump water heaters are most cost effective when used to replace conventional electric water heating systems. The HPWH program restricts eligibility, in fact, to those households that have electric water heating systems.* A HPWH can produce the same amount of

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*Other eligibility requirements include: (1) the dwelling has received permanent electric service for at least one year and is owner occupied or receives a bill in the renter's name; (2) adequate weatherization (at least R-19 in the attic) has been or is being installed; (3) the HPWH is installed by a contractor and its installation meets TVA standards; (4) the customer obtains a TVA home energy audit.

hot water as a conventional electric system for about half the operating cost. Natural gas water heating systems, in contrast, usually cost less to operate than either conventional or heat pump electric systems.[3] In the TVA area as a whole, almost 80% of the homes have electric water heaters. The saturation of electric water heating ranges from 36% to over 90% in the sixteen largest distributor areas, however (Table 1).[5]

Table 1. Percent of living quarters with electric water heaters by distributor

Distributor	District, State	Electric water heaters (%)
Chattanooga	Southeastern, TN	86.2
Florence	Alabama, AL	90.7
Huntsville	Alabama, AL	87.2
Johnson City	Appalachian, TN	95.2
Knoxville	Appalachian, TN	93.0
Memphis	Western, TN	36.0
Nashville	Central, TN	78.6
Cumberland EMC	Central, TN	87.2
Duck River EMC	Central, TN	91.3
4-County EPA	Mississippi, MS	64.9
Middle Tennessee EMC	Central, TN	90.3
North Georgia EMC	Georgia, GA	95.6
Pennyrile RECC	Kentucky, KY	82.6
Tri-County EMC	Kentucky, KY	84.4
Volunteer EC	Southeastern, TN	88.2
Warren RECC	Kentucky, KY	79.0

Source: ref. 5.

The size of the potential market for HPWHs varies by distributor area not only because of variations in the saturation of conventional electric water heaters, but also because of variations in climate, alternate fuel prices and availability (especially of natural gas), housing stock characteristics and water heater installation practices. Memphis, for example, is a poor market for HPWHs because natural gas is

widely available and because most water heaters are installed in attics where temperatures sometimes exceed the 95°F limit acceptable for reliable operation of HPWHs.[4] Chattanooga, Knoxville, and Johnson City, in contrast, are good markets because little natural gas is available and water heaters are generally installed in basements.[4]

Other factors likely to affect the market response to HPWHs include age of present water heaters, income, education, and prior knowledge of and experience with space heating heat pumps among potential adopters.[4] People who are satisfied users of heat pump space heating are likely to have more favorable attitudes toward HPWHs. They may also perceive a decision to install a HPWH as less risky than would people with no heat pump experience. People with higher educational levels, especially technical educations, also are likely to see less risk associated with HPWH adoption because the concept is familiar. In addition, higher income households typically have lower discount rates for conservation investments.[6] That is, they are more willing to invest capital to reduce operating costs and will accept longer payback periods.

Given the large differences among distributors in the potential size of HPWH markets, it would be incorrect to attribute all distributor differences in HPWH adoption rates solely to program-related factors such as variations in incentive offers. If Memphis offered rebates and had 5% of its residential customers adopting HPWHs, while Johnson City offered loans and had 10% of its customers adopting HPWHs, one could not conclude that loans were more effective than rebates. Not only do water heater saturations vary (i.e., 36% in Memphis and 95% in Johnson City [see Table 1]) but the two cities also have different climates, housing

stock characteristics and water heater installation practices. Therefore, *measures of incentive effectiveness cannot be based on the percentage of a distributor's residential customers who adopt HPWHs. The measure of effectiveness must be based on the percentage of customers offered the incentive who adopt the HPWH.* Since only customers who request a home energy audit and meet qualification criteria can be offered an incentive, the percentage of audited and qualified customers offered an incentive who adopt is the appropriate measure.

Criteria that determine if a customer is qualified for HPWH financing include: (1) an electric water heating system, (2) suitable location for a HPWH and (3) adequate weatherization (at least R-19 in the attic). The customer also must obtain a home energy audit and the installation work must pass a TVA inspection (Fig. 1).

Because HPWH incentives are offered only to households that request and receive a TVA home energy audit (a self-selected sample of the TVA population), results obtained from the experiments suggested here will be valid only for this portion of the TVA population. In addition, self-selection is a function of time. That is, the characteristics of households requesting TVA audits, eligible for HPWH incentives, and interested in HPWH installation will change over time. This further complicates interpretation and generalizability of evaluation results.

1.4 DIFFICULTIES WITH MATCHING

A simple design to evaluate effectiveness of financial incentives is to "match" distributors and assign different treatments to the distributors within each pair. This approach is not feasible here because the data needed to successfully match distributors are not available.

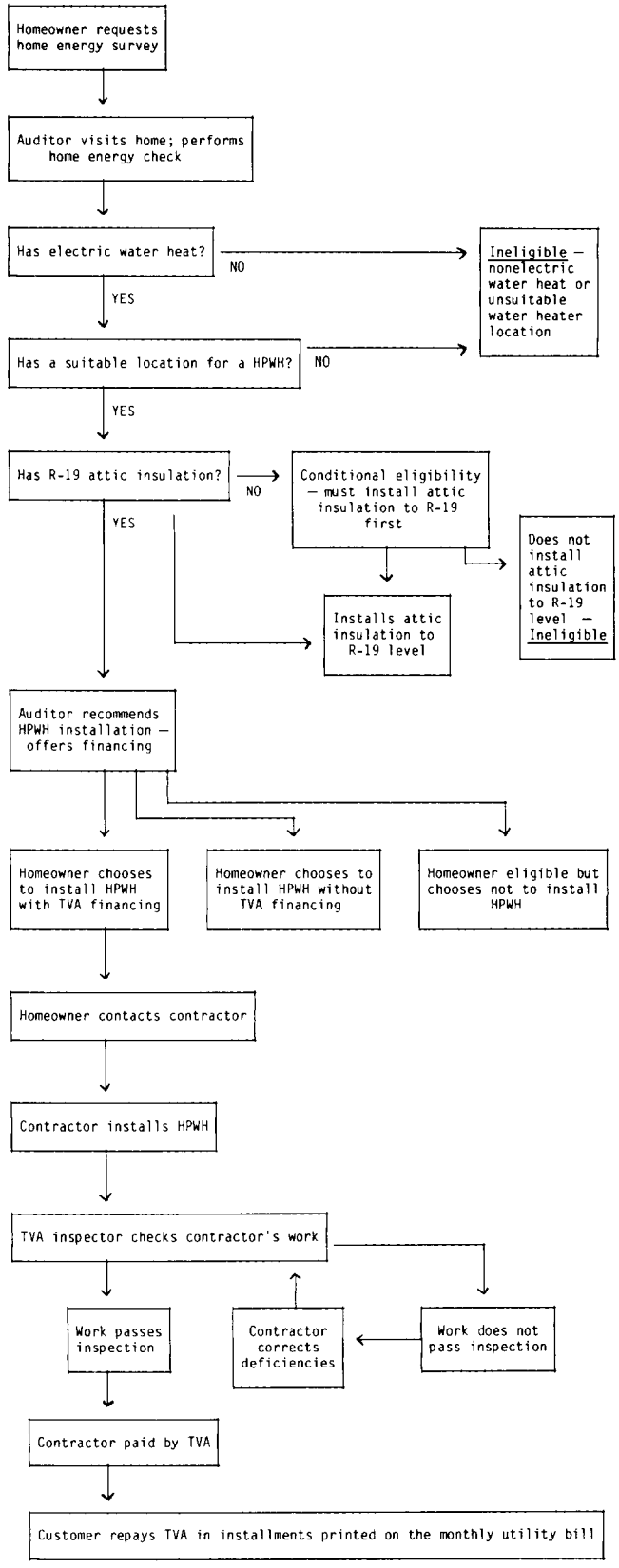

Fig. 1. Schematic of steps for customer participation in HPWH program.

Because an auditor's assessment is required to determine customer eligibility for HPWH financing, it is not possible to know how many, or which, customers will be qualified before their audits are completed. The most likely adopters of HPWHs are households that use large amounts of hot water, that have an electric water heater in an unheated basement or attached garage, that have already used heat pump space heating, and that have high income and educational levels. From existing TVA data sources (Appendix A),[5,7] it is not possible to determine what proportion of households in a distributor area would have a high (or low) propensity to adopt a HPWH. Further, even if the relevant characteristics of the population of households in the distribution area could be described in detail, the composition of the group that requests audits* will differ from the whole population of households. Thus, data that would allow one to match distributor areas on the variables likely to predict market potentials for heat pumps are not available.

1.5 NEED FOR RANDOM ASSIGNMENT

In addition to the practical difficulties with matching distributors, there are important theoretical objections to matching on distributor characteristics to obtain presumably equivalent treatment and control groups. As Campbell and Stanley[8] state, randomization is the only way — and the essential way — of assuring initial equivalence

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*In earlier stages of program development higher income households were overrepresented in TVA audit programs. Recently, strong efforts to reach low income households have raised their proportions among audit recipients.

of groups. Matching is no help in overcoming initial group differences and taboo as a substitute for randomization.[8] Matching can seriously bias estimates of treatment effects whenever there is measurement error or other irrelevance in the independent variables used for matching, or when models of the selection or post-test processes are misspecified or otherwise inadequate.[9]

There are, of course, acceptable uses of matching, such as blocking,* where one gains statistical precision by assigning the units under study to matched groups and then randomly assigns different treatments to units in the same group.[8,10] One also can reduce variability among groups and increase precision in the analytic stage by taking into account concomitant variables; that is, variables associated with the responses of principal interest.[9] Riecken and Boruch state:[10]

> The statistical technique to accomplish this adjustment is most commonly called "covariance analysis." It does not measure differences in response caused by the concomitant variable, but it eliminates variability in response associated with that variable. In true experiments, adjustment for concomitant variables simply increases the precision of treatment comparisons. It neither lessens the need for randomization nor vitiates its virtues of eliminating biases and justifying causal inferences. It cannot transform a quasi-experiment into a true experiment. One sometimes does encounter research designs that attempt to use covariance analysis to adjust for real differences in nonrandomly assigned treatment groups. This usage is not acceptable since, if the covariate contains error or unique variance, the adjustment will only partially remove such differences and the underadjusted residual is likely to be mistaken for treatment effects.

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*Blocking procedures are recommended in the designs discussed in Sections 2 and 3.

In short, covariance analysis, like matching, is a useful adjunct to randomization, but it is not a substitute for it.

Matching and covariance analysis must be used cautiously if they are not to create more problems than they solve. *Because of both the practical and theoretical problems with matching and covariance analysis, some sort of random assignment procedure is strongly recommended for testing incentive effects in the HPWH program.* A random assignment procedure ensures that each experimental unit (household or distributor) has an equal chance of being assigned to any group. Randomization generally results in equal distributions of subject characteristics in each group and thereby facilitates causal inference for treatments. If the number of subjects in a randomized study is large, it is unlikely that the treatment groups differ with respect to any characteristic that can affect the outcome under study, whether or not these characteristics are known to investigators.

Random assignment of treatments could be introduced at either the individual customer level or at the distributor level. Both possibilities are discussed below.

Generally, it is best to randomly assign treatments to the smallest possible units because a large number of small units ordinarily provides a more powerful experiment than a small number of large units.[8] Following this principle, random assignment of treatments at the level of the individual customer would provide the most powerful test of incentive effects (Section 2). Random assignment of treatments to customers could be achieved either by having different auditors offer different incentives (e.g., auditor A offers rebates and auditor B

offers loans) or by having the same auditor offer different incentives (e.g., an auditor offers all customers audited on Monday, Wednesday or Friday a loan and all those audited on Tuesday, Thursday or Saturday a rebate). If different auditors offer different incentives, then analysis of the effects of incentives may be confounded by the influence of the auditor. That is, auditor experience, thoroughness, enthusiasm, and salesmanship might influence customer response to the incentives. Therefore, the same auditor should offer different incentives to eliminate the threat of auditor/incentive interactions.

Having auditors within the same distributor area offer different incentives may cause customer dissatisfaction and complaints. Examining patterns of who complains about receiving which incentives would provide, however, valuable information about the relative attractiveness of the incentives. Further, as long as the number of customers who complain is small, one could simply give them the option they prefer and adjust for sample attrition in the analysis stage. Using incentives of equal economic value within a given distributor area also should help reduce the complaint problem.

When put in a larger perspective, offering an incentive to some customers and not to others is nothing new. TVA residential programs already have changed incentive offers over time and have been implemented by some distributors and not others.* Nevertheless, there may be

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*The feasibility of implementing a design that randomly assigns individual customers to different incentives also is supported by the fact that such a procedure has been used in previous evaluations of residential conservation programs. Both a Southern California Edison Company (ref. 11) and a Lawrence Berkeley Laboratory (ref. 12) evaluation of the relative effectiveness of audit types used random assignment of customers to audit treatments. A Pacific Gas and Electric Company evaluation (ref. 13) of a pilot program also used random assignments of customers to audit types and to a no-treatment control group.

legal problems associated with offering different incentives in the same distributor area. These problems would have to be resolved before an experiment at the individual level could be implemented.

Another way to achieve equivalent treatment and control groups would be to persuade different distributors, selected from matched blocks of distributors in a randomized block design, to adopt different incentives (Section 3). Distributors that already adopted the HPWH program might be either excluded from the sample frame or asked, when required, to substitute the treatment (e.g., rebates for loans) that was assigned to them in the randomization procedure. Having all the distributors that already chose loan options simply continue offering them is not desirable because these distributors have special characteristics that are likely to influence HPWH adoption rates. The distributors that already are in the HPWH program tend to have, for example, the best potential markets for HPWH's.[4] They also tend to have previously offered heat pump space heating programs, to be more progressive, and to have better planning capabilities.[4] Thus, distributors that selected themselves into the program are likely to have higher HPWH adoption rates regardless of the type of incentive offered.

The random assignment of treatments at a distributor level, instead of at an individual level, is administratively simpler and reduces the possibility of customer complaints or legal difficulties. It is a less powerful test of incentive effectiveness, however, both because fewer units of observation are available and because there is likely to be greater within-group variability.

Because both individual and distributor level designs have advantages and disadvantages, the best strategy would be to use both.

If only one distributor would agree to implement random assignment of treatments at the individual level, a convincing test of the relative effectiveness of loans vs rebates for that group of subjects could be completed. This test of the relative effectiveness of incentive types could then be supplemented with additional evidence on the generalizability of results by assigning different treatments to different distributors.

Detailed discussions of the specific data requirements, procedural steps and analysis techniques for both the individual and distributor level randomized designs are presented in Sections 2 and 3; the methods recommended can be used to test the relative attractiveness and effectiveness of loan, rebate and information offers. Section 4 presents a design that combines features of those discussed in Sections 2 and 3.

While randomized designs (Sections 2, 3 and 4) are far superior to nonrandomized designs for testing treatment effects, there may be circumstances that prevent implementation of a randomized design. For this reason, a technique that could be used in a nonrandomized study of financial incentive effects, the odds ratio, is described in Section 5. The odds ratio also can be used to examine associations between background factors, such as income or education, and the likelihood of adopting the HPWH. Analysis of such associations can be used both to improve program performance by targeting specific market segments and to estimate the generalizability of findings about treatment effects.

Another tool for improving program performance is a study aimed at clarifying customer decisionmaking processes. Section 6 discusses the importance of conducting research that can be used to increase HPWH adoption rates before implementing the experiments.

Finally, Section 7 presents an overall summary of the advantages and disadvantages of each of the designs presented in the preceding sections.

Although the analyses suggested here focus on the effects of different incentives on customer adoption of HPWHs, the data collected in these experiments can be used for other purposes. In particular, one can construct qualitative choice models (e.g., discriminant analysis, logit models) using the data to better understand the decision process that households use and to determine the characteristics of the groups that do and do not install HPWHs. For example, discriminant analysis could be used to classify households as loan users, rebate users, information users, and nonadopters on the basis of data collected during the program (see Appendix A). This information would be useful in designing and marketing future financial incentive programs.

2. INDIVIDUAL LEVEL ASSIGNMENT OF TREATMENTS

2.1 METHODS

2.1.1 Blocked Comparisons: Sign Test and Cochran Test

Two tests that are appropriate for individual level testing of the effects of different incentives on HPWH adoption are the sign test for paired comparisons and the Q statistic proposed by Cochran.[14] Both tests are useful when the outcome measure is dichotomous as it is for HPWH adoption (i.e., the decision is to adopt or not to adopt) and when the division of subjects into more homogeneous subgroups is desired. Such methods, called distribution-free methods, are needed when the populations under study are far from normal. They are also useful in exploratory research, when the investigator does not know much about the distribution being studied.

For the sign test the subjects are first divided into matched pairs of customers who are alike with respect to geographic region, family size, payoff period, income and education* so that each pair forms a homogeneous group. Then, within each pair one subject is selected at random (with probability of 1/2) to receive one treatment (a rebate) while the other receives an alternate treatment (a loan).[15]

For the Q statistic, subjects are divided into larger homogeneous groups, or blocks, and then assigned at random to treatments. The

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*As defined in the TVA Participant Survey form shown in Appendix A.

number of subjects per block is equal to the number of treatments to be assigned (Table 2). In this example, four incentives are applied to 12 groups. Each of the 48 combinations is then classified as an adoption (+) or nonadoption (-) outcome.

Table 2. Example of an application of the statistic Q

Treatment	Block												Total
	1	2	3	4	5	6	7	8	9	10	11	12	
Rebate	-	-	-	-	+	-	+	-	-	-	+	+	4
Loan	-	+	-	-	+	+	+	+	+	-	+	+	8
Rebate or loan	-	-	+	-	+	+	-	-	+	+	-	+	6
Information only	-	-	-	-	+	+	-	+	+	-	+	+	6
Adoptions	0	1	1	0	4	3	2	2	3	1	3	4	4

Both the sign test for paired comparisons and the statistic Q assume that subjects are likely to vary greatly in characteristics related to the outcome of interest. When subjects are highly variable a simple comparison of the effect of two treatments (without matching) is likely to be ineffective because variations among subjects may swamp the treatment effect.[15] In such cases, the effectiveness of the comparison can be improved by dividing the subjects into more homogeneous subgroups and comparing treatments within each subgroup.[15] Such two-way layout (treatment by block) designs will generally be much more effective at detecting differences among treatments than a one-way layout (treatments) design involving the same total number of observations at each treatment level.

2.1.2 One Way Layout Design: Completely Randomized Design

While it seems likely that individual households will vary greatly in characteristics related to the propensity to adopt a HPWH (hot water usage, discount rates, prior experience with the heat pump space heating and income), there are practical difficulties with implementing the matching procedures discussed in the next subsection. As a result, the alternative method of a completely randomized design which simply assigns treatments at random to households without attempting to match on background variables is attractive.

For a completely randomized design, the N available households are assigned at random, n_1 to treatment 1, n_2 to treatment 2 ,..., n_k to treatment k. There are only two possible outcomes A (Adopt) and B (Not Adopt), and it is observed how many households in each treatment group give choice A and how many B. Data can be summarized with a 2 x k contingency table (Table 3).

Table 3. 2 x k contingency table

Response	Treatments				Total
	1	2	...	k	
Adopt	A_1	A_2	...	A_k	m
Not adopt	B_1	B_2	...	B_k	n
Total	n_1	n_2	...	n_k	N

Without matching, it may be harder to detect treatment effects and/or estimates of the magnitude of the differences between treatment effects may be smaller. Nevertheless, simple random assignment of treatments is easily implemented. A treatment assignment decision rule for energy advisors, such as systematically offering customers different incentives in different weeks, could be implemented without changing existing program procedures.

2.2 IMPLEMENTATION

2.2.1 **Blocked Comparisons**

The requirement for the sign test and the Q statistic that a matching procedure be completed before random assignment of treatments is the most difficult part of the method to implement. The a priori matching procedure is difficult to do for several reasons. First, matching must be done between customers who are eligible for a HPWH financing offer and the final determination of eligibility must be made by an on-site inspection. Thus, the timing of the matching procedure is problematic. If the matches are done before eligibility is determined, many members of the matched groups would not be eligible for the incentive that otherwise would be assigned to them. But, matching after eligibility is determined requires a second contact with customers to make the incentive offers, which adds an extra step to the usual program procedure (Table 4).

Second, it is unclear how the matching procedure could be handled organizationally. Distributor staff are unlikely to have the time,

Table 4. Implementation steps for the sign test or Q statistic at the individual customer level

What	Who
1. Identify households eligible for HPWH's	Energy advisors
2. Obtain matching data on eligible households	EC & R[a] or EU & DR[b] from Participant Survey form given to eligible households by Energy Advisors
3. Define matched pairs or blocks of eligible households	EC & R or EU & DR
4. Randomly assign treatments within pairs or blocks. Prepare list of customers to receive each treatment	EC & R or EU & DR
5. Make incentive offers	EU & DR by mail or telephone or Energy Advisors from list provided by EU & DR or EC & R
6. Tabulate results (i.e., numbers of customers made an offer who adopted)	EU & DR or EC & R from distributors reinspection reports
7. Analyze results	ORNL[c]/EC & R

[a]Energy Conservation and Rates Division, TVA.

[b]Energy Use and Distributor Relations Division, TVA.

[c]Oak Ridge National Laboratory under contract with TVA.

interest or training needed to implement matching. Yet, in present program procedures the actual incentive offers are made by the energy advisors at the time of the audit. Some new organizational linkages and procedures would have to be developed to coordinate matching, assignment of treatments, and presentation of incentive offers to homeowners.

One possibility might be to have a distributor that is not yet offering a HPWH program begin having its energy advisors make determinations of eligibility without offering incentives. If the distributor supplied a list of eligible households, TVA staff (from either the Energy Use and Distributor Relations or the Energy Conservation and Rates Divisions) could then carry out the matching procedure. After completing the matches they also could assign treatments and make the incentive offers by mail and/or telephone. Alternatively, energy advisors could be supplied with lists of which customers are to receive which treatments and then make the second contact with the customer.

It might be possible to use data from the Energy Conservation Program Participant Survey (Appendix A) for matching purposes. Data on 30,000 — 40,000 households already have been collected in this survey and at least that many more households will probably be surveyed in 1983. The survey includes questions on the household's number of occupants by age, income, education, length of residence and payback period (1 — 10 years) required for a conservation investment to be considered worthwhile. Each of these variables, especially the payback period information, would be suitable for matching households on assumed propensity to adopt HPWHs.

If an acceptable procedure can be developed for matching households, assigning treatments and delivering incentive offers, either the sign test for paired comparisons or the Q statistic would be a simple and powerful method for assessing the relative effectiveness of incentive types. Their computation and interpretation are straightforward.[14]

They require data collection on only about 50 pairs of households. Only the administrative difficulties involved recommend against their use.

So far, the procedures discussed for testing incentive effects by matching background variables at the individual level share the advantage of requiring only a small number of subjects.* Handling such small numbers of subjects should greatly simplify administration of the experiment and reduce its costs since only a few auditors would have to be trained in the experimental procedures and minimal data collection efforts would be required. The statistical analysis, however, is confined to the N available subjects. If these subjects are not a representative sample from the target population, results cannot be generalized to the total population.**

The individual level methods also share the disadvantage of possibly causing customer dissatisfaction and complaints due to perceived inequities in incentive offers. As noted in Section 1, using incentives of equal economic value within a given distributor area and giving those who complain the option they prefer should help control the problem. The fact that only small numbers of subjects are required for the individual level tests also should reduce the possibility of customer complaints. Further, if the experiment is conducted in a distributor area which previously offered no incentive, most customers

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*Of course, larger sample sizes would allow for both exploratory analysis and for analysis to test the effects of different auditors and of household characteristics on HPWH adoption.

**Recall that the target population consists of self-selected households that requested and received a home energy audit, and that were offered a HPWH financial incentive.

could be offered an information only package. Only the households that were among the experimental units would be offered a loan or rebate incentive. The great majority of customers who receive information only would probably not be aware of the experimental treatments. Those who did complain about not receiving incentive offers could be told that only a small experimental group received offers and that if the experiment is successful the incentives may be offered on a broader scale in the future. Even though it seems likely that complaints could be kept to manageable levels, potential legal issues would have to be resolved before individual level designs could be implemented.

2.2.2 **One Way Layout Design**

The experimental units for the one way layout design can be any group of N qualified customers. For simplicity, the size of each treatment group should be the same number, n_0, so that $N = n_0k$, where k is the number of treatments. The first implementation step for the completely randomized design is to determine the total number of subjects, N, and treatments, k, to be included in the experiment. Larger sample sizes are needed than in the treatment x block designs because the advantages of using matched samples are absent and the null distribution is quite skewed. As a rule of thumb, n_0 should be a function of the expected adoption rate (p). If the HPWH adoption rate is

expected to be around 5% of the qualified respondents ($p = 5\%$), then n_0, $= [5/p] = 100$.*,**

While one could implement this design by simply studying the first $N = n_0 k$ qualified customers receiving offers in one distributor area, generalizability of results would be improved if the subjects were a representative sample of the population of all qualified customers. Thus, it would be desirable to include customers from several distributor areas in the sample. In addition, it would be administratively simpler to implement the design if only a few treatments were offered in each distributor area. Therefore, it would be best to compare only a few treatments within each of several distributor areas for each study and then combine all study results in the final analysis (Table 5).

After determining the number of subjects, treatments and distributor areas to be studied, the next step is to randomly assign n_0 of the N subjects to each of the treatments (Appendix B). Each subject i is matched with a treatment number T(i); i takes values from 1, ..., N and T(i) takes values from 1, ..., k. If an auditor identifies a customer as qualified for an incentive offer during a home audit visit, he first labels the customer with a subject identification number i and then offers the customer the pre-matched financial incentive T(i).

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*The determination of n_0 through the equation $n_0 p \geq 5$ would provide a sample size n_0 large enough so that the distribution of the test statistic will be close to the asymptotic distribution. The sample size, n_0, will also assure that enough observations will be made to provide meaningful measurements of the adoption rates in each of the treatment groups.

**Problems associated with low participation rates are discussed in Section 6.

Table 5. Implementation steps for the one way layout design at the individual level

What	Who
1. Determine the total number of subjects, N, and treatments, k, to be studied in each distributor area that participates so that $N = n_0 k$	EC & R[a]
2. Randomly assign n_0 of the N subjects to each of the treatments	EC & R
3. Prepare a list of financial incentives to be offered to qualified customers by subject identification numbers that reflect the order in which they will be contacted by energy advisors	EC & R
4. Make pre-assigned offers to qualified customers as they are contacted	Energy Advisors
5. Tabulate results (i.e., numbers of customers made each offer who adopted)	EU & DR[b] or EC & R from distributor records
6. Analyze results	EC & R/ORNL[c]

[a]Energy Conservation and Rates Division, TVA.

[b]Energy Use and Distributor Relations Division, TVA.

[c]Oak Ridge National Laboratory under contract with TVA.

Before the experiment starts, a treatment assignment sheet should be prepared for each of the energy advisors participating. For example, Table 6 illustrates that auditor #5 should assign subject identification number i = 51 to the second customer qualified by him and then offer that customer financial incentive option 2.

Table 6. Incentive offer guide for auditor #5

Number your qualified customers starting from one	Subject identification number assigned (i)	Financial incentive option number[a] (T(i))
1	50	4
2	51	2
3	52	3
4	53	3
5	54	1

[a]Financial incentive option number: 1 = Rebate, 2 = Loan, 3 = Rebate or loan, 4 = Information only.

2.3 ANALYSIS OF RESULTS

2.3.1 Blocked Comparisons

For the sign test for paired comparisons, the test of the null hypothesis, H_0, of no treatment effect is based on the number of pairs for which one treatment (a rebate) comes out ahead of the other (a loan). If N_+, N_0, and N_- denote the numbers of positive (rebate adopts and loan does not), zero (both adopt or both do not), and negative (rebate does not adopt and loan does) differences in outcome among the N pairs, the test statistic is then equal to N_+.

Let $M = (N_+) + (N_-)$, the test statistic $S_M = N_+$ has the null distribution of a binomial distribution corresponding to M independent trials with success probability (p) of 1/2. Thus, the hypothesis of no difference in the effect of treatment 1 and treatment 2 can be tested by a two tailed test at the α level of significance:

$$\text{Reject } H_0 \quad \text{if } S_M \geq B(\alpha/2, M, 1/2) \text{ or}$$
$$S_M \leq M - B(\alpha/2, M, 1/2)$$

where $B(\alpha/2, M, 1/2)$ is the upper $\alpha/2$ percentile point of the binomial distribution with sample size M and $p = 1/2$. Note that the size of N_0 does not affect the test statistic value. When there is a low response rate among qualified consumers, sample size N should be determined so that M will be large enough to anticipate the frequent occurrence of tied pairs.

Let L_j denote the number of successes in the jth block, $j = 1, 2, \ldots, n$ and B_i the number of successes for the ith treatment, $i = 1, \ldots, k$. Then the Cochran statistic is

$$Q = \frac{(k-1)[k \sum_{i=1}^{k} B_i - (\sum_{i=1}^{k} B_i)^2]}{k \sum_{j=1}^{n} L_j - \sum_{j=1}^{n} L_j^2}$$

For the hypothetical data shown in Table 2, $Q = 3.69$ (for computational details see ref. 15, pp. 267-268). Since Q has approximately a χ^2 distribution with [4 (treatments) - 1] = 3 degrees of freedom, the probability of observing this result when the null hypothesis, H_0, is true is $P(\chi^2_3 \geq 3.69) = 0.297$. For the data in Table 2, therefore, one would fail to reject H_0 and conclude that there were no significant differences among treatments.

2.3.2 One Way Layout Design

Suppose that the response to the completely randomized experiment, with k different financial incentive types, is as shown in Table 7.

Table 7. Generic layout for analysis of the completely randomized design

Response	Treatments				Total
	1	2	...	k	
Adopt	A_1	A_2	...	A_k	m
Not adopt	B_1	B_2	...	B_k	n
Total	n_0	n_0	...	n_0	N

The null hypothesis of no difference in the effect of the k financial incentives can be tested by means of the Kruskal-Wallis statistic. Using

$$K = \frac{N(N - 1)}{m\ n} \left[\frac{1}{n_0} \sum_{i=1}^{k} A_i^2 \right] - \frac{m^2}{N}$$

we reject Ho at error rate α if

$$K \geq \chi^2(k - 1, \alpha) .$$

In comparing two treatments, Table 7 reduces to the form shown in Table 8.

Table 8. 2 x 2 contingency table

Response	Treatment		Total
	1	2	
Adopt	A_1	A_2	m
Not adopt	B_1	B_2	n
Total	n_0	n_0	N

The hypothesis of no difference in the effect of treatment 1 and treatment 2 can be tested against the three alternatives:

(1) H_1 = treatment 1 is more effective than treatment 2

The one-sided Wilcoxon statistic

$$W_1 = \frac{A_1B_2 - A_2B_1 - (1/2)N}{\sqrt{m\ n\ (n_0)^2/(N - 1)}}$$

rejects H_0 at error rate α when $W_1 \geq Z_{1-\alpha}$ where $Z_{1-\alpha}$ is the $(1-\alpha)$th percentile of the standard normal distribution.

(2) H_1: treatment 2 is more effective than treatment 1

The one-sided Wilcoxon statistic

$$W_2 = \frac{A_1B_2 - A_2B_1 + (1/2)N}{\sqrt{m\ n\ (n_0)^2/(N - 1)}}$$

rejects H_0 at error rate α when $W_2 \leq Z_{1-\alpha}$.

(3) H_1: the two treatments are different

The two-sided Wilcoxon statistic

$$W = \frac{[\,|A_1B_2 - A_2B_1| - (1/2)N\,]^2}{m\ n\ (n_0)^2/(N - 1)}$$

rejects H_0 at error rate α when $W \geq \chi^2_{(1,\alpha)}$.

3. DISTRIBUTOR LEVEL ASSIGNMENT OF TREATMENTS: THE RANDOMIZED COMPLETE BLOCK DESIGN

3.1 METHODS

There are 160 distributors of TVA power. As discussed in Section 1, HPWH market potentials vary greatly among these distributors. Data with which to predict distributor market potentials are, however, unavailable. As a result, the percentages of audited, qualified customers offered loan/rebate/information only packages that install a HPWH are the outcome measures of interest in a distributor-level experiment. Differences in the percentages adopting HPWHs under the loan/rebate/information only treatments can be examined to determine the incremental effects of the incentives.

Of course, some of the difference in the percentages of customers adopting HPWHs in different distributor areas will occur by chance and some of the difference will be due to distributor characteristics* other than the type of incentives offered (the treatments). One approach to separating the effects of the incentives from the other influences is to randomly assign treatments among distributors.

An appropriate method for a distributor-level test of the effects of different incentive treatments is a Randomized Complete Block Design (Table 9) with a Friedman Rank Sums test of significance (Appendix C).

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*The design discussed in Section 4 eliminates the influence of distributor effects more effectively than the design suggested in this section by randomly assigning treatments to individuals within each distributor area.

Table 9. Randomized complete block design with rank-sums test

Blocks	Financial incentive types						
	1	2	3	4	5	...	k
1	P_{11}	P_{12}	.	.	.	.	P_{1k}
2	P_{21}	P_{22}	.	.	.	.	P_{2k}
3	.	.	.	.	.	.	.
4	.	.	.	.	.	.	.
.	.	.	.	.	.	.	.
.	.	.	.	.	.	.	.
.	.	.	.	.	.	.	.
n	P_{n1}	P_{n2}	.	.	.	.	P_{nk}

The data to be collected for the Friedman Rank Sums test consist of nk observations with one observation, P_{ij}, for each of k treatments in each of n blocks. Each P_{ij} observation is the percentage of HPWH adoptions among customers offered incentive j in a distributor area of type i. Ideally, the n blocks are defined by matching distributors on variables likely to affect outcomes (i.e., percentages of adoptions). As noted above, however, data on such matching variables are largely unavailable. Nevertheless, an attempt to create somewhat homogeneous groups of distributors should be made.

3.2 IMPLEMENTATION

The first step in implementing this design is to determine the number of treatments, k, to be considered and the number of blocks, n, to be constructed. A method of partitioning distributors into n subpopulations also must be defined. After distributors are divided into n subpopulations, k distributors are randomly selected from each of the n categories to form a block of experimental units. Each treatment is then assigned at random to distributors selected from block n. At this

stage nk distributors will have been selected. Administratively, it is desirable to keep nk small so that fewer changes in distributor HPWH program contracts have to be negotiated.

With the above general principles in mind, the following specific steps are recommended (Table 10).

(1) Stratify the distributors by the seven geographic districts (i.e., Appalachian, Southeastern, etc) and by the number of residential customers* served to form a matrix of the form shown in Table 11. After arranging the distributors by district and by number of customers (Table 11), form four to six strata of approximately equal size (i.e., each stratum should contain 20-40 distributors). The cutting point on the district variable is shown in Table 11. The cutting points for the number of customers variable is determined after the matrix is filled in, and should be based on the frequency patterns observed for distributor sizes. The goal is to choose size cutting points that will produce blocks with approximately equal numbers of distributors in each stratum. The smallest distributors may be dropped from the matrix if desired.

(2) Randomly select four distributors from each of the strata defined in step one to form blocks of distributors. Then,

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*The variables of geographic district and number of residential customers are suggested because they are readily available for all distributors and because they seem likely to have some relation to market potential. Other variables could be used if desired (e.g., geographic density of customers, average electricity consumption per customer, utility conservation program expenditures per customers). The only limitation is that a value must be known for all distributors.

Table 10. Implementation steps for randomized model at the distributor level

	What	Who
1.	Define matched blocks of distributors by district, based on electric water heater saturation and number of customers	EC & R[a]
2.	Randomly assign treatments to distributors in each block	EC & R
3.	Negotiate with distributors to obtain agreement on incentive offer assignment	EU & DR[b] through District Offices
4.	Make the assigned incentive offers to qualified customers	Energy advisors
5.	Collect data on background variables	Energy advisors
6.	Tabulate results (percentage of customers made each offer who adopted HPWHs)	EU & DR or EC & R from distributor records
7.	Analyze results	EC & R/ORNL[c]

[a]Energy Conservation and Rates Division, TVA.

[b]Energy Use and Distributor Relations, TVA.

[c]Oak Ridge National Laboratory under contract with TVA.

within each block randomly assign the following four treatments (a randomization procedure is described Appendix B) to the four distributors:

Number	Treatment
1	Rebate
2	Loan
3	Rebate or Loan
4	Information only

Table 11. Format for blocking distributors by geographic district and number of customers

	District	Number of customers				
		Smallest				Largest
High[a]	Appalachian	___	___	___	___	___
	Southeastern	___	___	___	___	___
	Central	___	___	___	___	___
	Alabama	___	___	___	___	___
	Kentucky	___	___	___	___	___
Low[a]	Western	___	___	___	___	___
	Mississippi	___	___	___	___	___

[a]The cutting point for districts in high/low categories is based on the saturation of electric water heaters in the largest distributor areas (see Table 1, Section 1). Those in the low category have distributors with less than 70% of households with electric water heat; those in the high category have more than 70%.

(3) Negotiate with the selected distributors to obtain their agreement on incentive offer assignments. If a selected distributor will not agree to offer the assigned treatment another distributor from the same block may be used as a replacement.

(4) Make the assigned incentive offers to qualified customers.

(5) Collect data on the background variables* of income, education, acceptable payback periods, and number and ages of occupants

*Information on background variables serves two purposes. First, it can be used to examine differences among users of different incentives. Secondly, the variables can be combined at different levels to form a number of study categories (e.g., INCOME = High, PAYOFF PERIOD = Less than 4 years, etc). Then, comparisons of adoption rates can be made for groups (study categories) from different distributors whose response may be expected to be comparable.

from all customers who receive offers. Data on each of these variables are already collected in the Energy Conservation Program Participant Survey. This survey (Appendix A) is used to collect information on all customers qualified for HPWHs.

(6) Collect data on the percentage of qualified customers offered incentives that adopted HPWHs (P_{ij}). The observed percentage P_{ij} (Table 9) will be calculated and analyzed for all customers with the same combination of background variable(s). Matching across background variables will reduce the confounding effect caused by them.

(7) Ranks are assigned only within rows (or blocks). The highest percentage of qualified customers adopting within row i receives the rank of 1, the next highest percentage a rank of 2, etc. Thus, a distributor in row i who received treatment j will be assigned rank R_{ij}.

The main advantage of the distributor-level design over the individual level designs discussed in Section 2 is its greater ease of implementation. Only step 3, obtaining distributor agreement with assigned incentive offers, is likely to present problems. The other steps represent little, if any, change from existing program procedures.

The major disadvantage of the distributor-level design is that it is likely to be a less powerful test of incentive effects. This is because data on distributor-level matching variables that are good predictors of adoption levels are largely unavailable. As a result,

distributor area and administrative differences that may account for variation in adoption rates are difficult to control for and may mask treatment effects.

Another disadvantage of the distributor level design is that it requires a much larger data collection effort than the individual level designs. Records must be kept on all the incentive offers made and accepted by customers in all the participating distributor areas. In addition, Participant Surveys must be administered to all customers who receive HPWH incentive offers. Much of the large data collection effort required for the distributor level design is, however, already part of routine program procedures. Still, the large number of individuals involved with this experiment will lengthen the time required to complete the experiment and the cost of conducting it.

3.3 ANALYSIS OF RESULTS

After steps one through six (Section 3.2) are completed, data analysis and significance tests can be undertaken. The null hypothesis, H_0, is that there is no difference in the effect of the k treatments.

If we let R_{ij} denote the rank received by P_{ij}, then for each treatment, j, an indication of the position of the jth treatment is provided by the average column sum of ranks, $R_{\cdot j}$. Where,

$$R_{\cdot j} = R_j/n = \sum_{i=1}^{n} R_{ij}/n \qquad j = 1, \ldots, k \ .$$

The basic logic of the Friedman rank sums test rests on the observation that if there are no treatment effects, all possible permutations of ranks within rows are, with randomization, equally likely. This

implies that the column sums of ranks ($R_{\cdot j}$) are expected to be identical under the null hypothesis.

After obtaining average ranks, one computes the Friedman statistic as follows:

$$S = \frac{12n}{k(k+1)} \sum_{j=1}^{k} [R_{\cdot j} - 1/2\ (k+1)]^2$$

$$= \left[\frac{12}{nk(k+1)} \sum_{j=1}^{k} R_j^2 \right] - 3n(k+1)$$

At the α level of significance we reject H_0 if

$$S \geq s(\alpha, k, n)$$

The constants $s(\alpha, k, n)$ are obtained from Table A.15 of ref. 16 (reproduced in Appendix D) for k=3, n=2(1)13; k=4, n=2(1)8; k=5, n=3, 4, 5. With values of k and n beyond the ranges of the tables we reject H_0 if

$$S \geq \chi^2_{(k-1,\alpha)}$$

Typically, the approximation above will work well provided $kn \geq 30$, with the accuracy also depending on the configuration of ties. (See ch. 7 of ref. 16 for more detail on the methods of developing approximations and of handling tied ranks).

If H_0 is rejected and treatment effects are assumed to exist, one can then determine the ordering of k treatments by reaching a decision for each pair of treatments.

At the α level of significance, the uth treatment is declared superior to the vth treatment if $|R_u - R_v| \geq \gamma(\alpha, k, n)$ and $R_u > R_v$ holds. The order of treatments is left in doubt if

$$|R_u - R_v| < \gamma(\alpha, k, n) ,$$

where values of γ are from Table A.17 of ref. 16 (Appendix D).

The result of such a set of comparisons can be presented visually in the following tabular form. Here, we use < to indicate that treatment u is inferior to treatment v and ~ to indicate that the difference between the two is not significant.

u/v	2	3	4	5	6	7
1	~	<	<	<	<	<
2		~	<	<	<	<
3			~	<	<	<
4				~	<	<
5					~	<
6						~ .

The null distribution, H_0, of the difference in the effect of the k treatments is applicable to Model 2 as described in Appendix C.

If we focus attention on pairwise comparisons of each treatment vs the control condition rather than on all treatment comparisons described above, then the following procedure is more powerful. At α level of significance, decide treatment u to be superior to the control condition if $|R_u - R_k| \geq \gamma^{**}(\alpha, k-1, n)$ and $R_u > R_k$ where u = 1, ..., k-1, treatment k is the control or standard treatment, and where $\gamma^{**}(\alpha, k - 1, n)$ can be found in Table A.19 of ref. 16 (Appendix D). The treatment of large sample approximations and ties also can be found in ch. 7 of ref. 16. The null distribution is applicable to Model 2 as described in Appendix C.

4. INDIVIDUAL LEVEL ASSIGNMENT OF TREATMENTS WITHIN DISTRIBUTOR

A final randomized approach is to combine elements of the designs discussed in Sections 2 and 3. In this way the advantages of the individual level design can be added to the advantages of the distributor level design. Most importantly, the ability of the individual level design to detect small treatment effects and to eliminate distributor characteristics as an alternate explanation for observed differences among treatments is combined with the ability of the distributor level designs to allow for greater generalizability of results.

The basic structure of the combined design (Table 12) is the same as that of the distributor level design (Table 9, Section 3). Each P_{ij} observation is the percentage of HPWH adoptions among customers in block i who were offered incentive j.

Table 12. Combined individual/distributor level design

Distributor	Financial incentive types			
	1	2	. . . . .	k
1	P_{11}	P_{12}	. . . . .	P_{1k}
2	P_{21}	P_{22}	. . . . .	P_{2k}
.	.	.	. . .	
m	P_{m1}	P_{m2}	. . .	P_{mk}

The difference in design arises from the way treatments are assigned and the way blocks are defined. In the distributor level design (Section 3) each P_{ij} represents the percentage of adoptions among customers of a distributor area in block i who were offered a given incentive j. Each block consists of a group of distributor areas of a certain type. Since only one assigned incentive is offered in each distributor area, the distributor being observed changes as one moves across a row of Table 9.

For the combined design, in contrast, several incentives assigned at random to individual customer groups are offered in each distributor area. Thus, as one moves across a row of Table 12 only the customer groups change but the distributor does not change. That is, each distributor serves as a block in this design. Only one distributor selected from each stratum i (1 to m) is included in the experiment, and each row shows variations in adoption by incentive type for that one distributor.

Because only one distributor of each stratum is selected in the combined design, fewer distributors are required to participate than in the distributor level design. Selecting only one distributor from each stratum also makes it possible to define a larger number of strata and, therefore, to make distributors in each stratum more homogeneous.

The procedure for dividing distributors into homogeneous groups is essentially the same as that described in Section 3.2. Because only one distributor is selected from each stratum, a larger number of strata and a larger number of stratifying variables may be used. Within each stratum, one distributor will be selected at random. TVA will negotiate

with the selected distributor to obtain agreement on participating in the experiment. If a selected distributor decides not to participate, another distributor selected at random from the same stratum may be used as a replacement. All participating distributors will carry out the procedures for the individual level design described in Section 2.2.2.

Data can be analyzed both at the distributor level by using the tests suggested in Section 3.3 and at the individual level by using the tests suggested in Section 2.3.2.

5. NONRANDOMIZED DESIGN

5.1 METHOD

Because nonrandomized studies offer no assurance of comparability between the treatment and control groups, they often give inaccurate estimates of treatment effects in the presence of counfounding variables. Nevertheless, nonrandomized studies are sometimes the only ones possible and are usually less expensive and less difficult to implement than randomized studies. In nonrandomized (observational) studies a useful measure of association based on comparisons of the probability of outcomes among groups receiving different treatments is the odds ratio. It is important to emphasize that the odds ratio is a measure of association and not a test of significance. While the significance of an association, as measured by a x^2 statistic for example, will increase as sample sizes increase, the extent of association, as measured by the odds ratio, is independent of sample size.[17]

The odds ratio was orginally proposed as a measure of the degree of association between an antecedent factor and an outcome event such as morbidity or mortality.[18] It is especially appropriate in cases of rare outcomes (such as the low adoption rates typical of HPWH program*) and has been widely used in epidemological studies of rare diseases.

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*Without special promotional efforts HPWH programs seem unlikely to produce adoption rates of more than 2-3%. In a 1981-82 Pacific Gas and Electric program, for example, 16,783 direct mail offers of a $300 rebate for HPWHs produced only 180 installations (ref. 19). The direct mail offers were targeted to electric water heater owners who were high electricity users. In a similar Southern California Edison program 5,000 offers produced only 18 installations.

The odds ratio is useful both for measuring the extent of association between the probability of HPWH adoption and exposure to various incentive offers and for measuring the extent of association between HPWH adoption and background factors such as income or education. Thus, it can provide measurements both of the magnitude of treatment effects and of the importance of background factors. Since estimates of the importance of background factors are not directly provided by the signficance tests recommended in previous sections, the odds ratio is a useful adjunct to these tests. It can be applied to any data set for which information on outcomes (i.e., HPWH adoption or nonadoption) and on either incentive offers made or background factors present is available.

5.2 IMPLEMENTATION

To implement a nonrandomized study one would simply observe the outcomes that occurred when different incentives were offered (Table 13). There would be no requirement that a formal assignment procedure for the incentives (treatments) be followed. Any household for which the outcome (i.e., adoption/nonadoption of a HPWH) is known and which has received one of the incentive offers under study would be included in the data set to be analyzed.

Since existing programs offer only loans, the addition of other incentive options (rebates and/or information only) to program procedures would be necessary even for an observational study. In a nonrandomized study, however, one could use the most convenient method of adding new treatment options. One could ask, for example, for volunteers among the distributors to offer rebates or information only options. Once different treatments had been offered to different groups for some

Table 13. Implementation steps for an observational study using the odds ratio

To estimate size of treatment effects:	
1. Locate distributors willing to offer different incentives	EU & DR[a] through district offices
2. Identify adopting/nonadopting households that were offered different incentives	EU & DR from distributor records
3. Obtain data on background factors that can be used for matching	EC & R from participant survey forms
4. Define matched groups of adopting/nonadopting households that were offered different incentives	EC & R[b]/ORNL[c]
5. Calculate odds ratio to estimate the size of the treatment effect	EC & R/ORNL
To estimate importance of background factors:	
1. Identify groups of adopting/nonadopting households with different levels of background factors	EU & DR from Distributor records and EC & R from participant survey forms
2. Calculate odds ratio	EC & R/ORNL

[a]Energy Use and Distributor Relations Division, TVA.

[b]Energy Conservation and Rates Division, TVA.

[c]Oak Ridge National Laboratory under contract with TVA.

time (say one year), one compares the proportions of adopting vs nonadopting households that received each treatment.

This approach also can be used in combination with any of the randomized designs discussed in Sections 2 and 3. One would assemble all the available data on which households had adopted/not adopted after receiving which treatments.

The procedure for studying the influence of background factors is similar. Any household for which information on both outcomes and background factors is available would be included in the data set to be analyzed. Then, the proportions of adoptors vs. nonadoptors with different levels of background factors are compared.

Since the effect of a given incentive is unlikely to be identical for all types of households, it is desirable to match on background factors that have been found to be important before calculating a measure of treatment effects such as the odds ratio. Obviously, since not all possible confounding background factors can be identified, measured, or included in a matching procedure, no assurance of comparability between treatment groups is possible. Nevertheless, matching on at least some background factors will increase one's confidence in the credibility of the results.

5.3 ANALYSIS OF RESULTS

Once data on matched groups of adopters and nonadopters are assembled, the odds ratio for treatment effects is computed. First, suppose that a sample of n_1 adopters and a sample of n_2 nonadopters are studied, with results arrayed as shown in Table 14. X represents the number of adopters who were offered the incentive under study, and m represents the total number of households offered the incentive in the two samples (adopters and nonadopters) combined. Let P_1 denote the underlying proportion of adopters who were offered the incentive and P_2 the underlying proportion of adopters who were offered information only. The parameter of interest is the odds ratio, or cross-product ratio, W.

Table 14. Generic lay-out for calculation of the odds ratio

Sample	Antecedent factor		
	Financial incentive offered	Information only	Total
Adopted HPWH	X	$n_1 - X$	n_1
Did not adopt HPW	$m - X$	$n_2 - m + X$	n_2
Total	m	$n_1 + n_2 - m$	

$$W = \frac{P_1/(1 - P_1)}{P_2/(1 - P_2)} = \frac{X\ (n_2 - m + X)}{(m - x)\ (n_1 - X)}$$

We illustrate the use of odds ratio with the data given in Table 15. In this example, the rebate is effective because the adoption rate is higher in the rebate group than in the information only group.

Table 15. Measure of treatment effect for dichotomous treatment and outcome

Sample	Antecedent factor		
	Rebate Offered	Information only	Total
Adopted HPWH	6 ($P_1 = 0.06$)	10 ($P_2 = 0.01$)	16
Did not adopt HPWH (control)	94 ($1 - P_1 = 0.94$)	990 ($1 - P_2 = 0.99$)	1084
Total	100	1000	1100

$$\text{Odds ratio (W)} = \frac{0.06}{0.94} \Big/ \frac{0.01}{0.99} = 6.32$$

The odds ratio (W) is based on the notion of odds. The odds of an event are defined as the ratio of the probability of the event to the probability of its complement. For instance, the odds of adopting the

HPWH in our example are equal to the adoption rate (0.06) divided by the nonadoption rate (0.94), or 0.064. When the odds of adopting are less than 1, the probability of adopting is less than that of not adopting. Now, the odds ratio in our example is the ratio of the odds of adopting in the rebate group (0.064) to the odds of adopting in the information group (0.01/0.99 = 0.01), or 6.32. The odds of adopting are 6.32 times higher in the rebate group.

In addition to providing an estimate of treatment effect, the odds ratio also can be used to examine the influence of background variables.* As an example, a summary of odds ratios for groups of varying income levels that are offered a rebate or that are offered only information is given in Table 16. The conclusion is that the effect of the financial incentive is essentially constant across income intervals. If the observed odds ratio for any interval departed appreciably from 2.0, further research would be indicated. A similar analysis could be done for other background variables of interest.

Table 16. Adoption rates odds ratios by income and treatment group

Income interval ($)	Odds of adopting HPWH		
	Rebate offered group	Information only group	(W) Odds ratio
0 ~ 20,000	0.11	0.05	2.2
20,000 ~ 30,000	0.17	0.07	2.4
30,000 ~ 40,000	0.04	0.02	1.9
40,000 ~ 50,000	0.21	0.01	2.1
50,000 ~ 60,000	0.08	0.04	2.0
60,000 and above	0.11	0.06	1.8

*Other methods, such as discriminant analysis and logit models, can also be used to analyze the relationships between household characteristics and the probability of choosing different incentives.

6. NEED TO INCREASE ADOPTION RATES

Purchase of a HPWH is a major consumer decision, involving substantial initial cost ($800-$1200). A successful program to encourage such purchases must be based on a thorough understanding of consumer decisionmaking processes. Questions such as what motivates a HPWH purchase and what barriers prevent their purchase need to be answered. At present, little evidence on why customers do or do not purchase HPWHs is available. It is clear, however, that few purchases are being made. After the HPWH program had been in operation for three months (October 1982 - January 1983), only eight installations had been completed.

Program managers offer a number of explanations for low HPWH adoption rates such as the following:

(1) HPWHs are too expensive. They cost nearly three times as much as conventional electric water heaters;

(2) The technology is unfamiliar, while solar water heaters are much better known and more valuable as a status symbol;

(3) Most customers don't know how much electricity is used for water heating;

(4) There are no HPWH distribution or service systems in many areas;

(5) High unemployment, mobility, and economic uncertainty make people reluctant to invest;

(6) TVA audit programs are currently attracting large proportions (35-45%) of low income households, who are unlikely to invest in HPWHs. Higher income households, who are better prospects for HPWHs, participated in the earlier years (1977-1980) of audit program operation;

(7) HPWHs compete with a number of more pressing, more cost effective and/or more attractive home weatherization needs (e.g., basic structural repairs, attic insulation, storm windows). The maximum investment households are willing to make in conservation is likely to be used up on these higher priority items.

Each of the above explanations for low HPWH adoption rates seems plausible. A systematic effort is needed, however, to identify the most important barriers to HPWH adoption and to increase adoption rates. Efforts to test the effects of incentives in the HPWH program are presently premature. Unless and until higher adoption rates are achieved, experiments aimed at testing incentive effects will have too few adoptions to support convincing conclusions with moderate sample sizes. With the recommended experimental designs, an overall adoption rate of at least 5% is needed to make analysis of incentive effects feasible when group sample sizes are about 100.

7. SUMMARY AND CONCLUSIONS: COMPARISON OF DESIGN ADVANTAGES AND DISADVANTAGES

In Sections 2 through 5 five design alternatives were presented: 1) random assignment of treatments at the individual level with matching, 2) random assignment of treatments at the individual level without matching, 3) assignment of treatments at the distributor level 4) a combined design that randomly assigns individuals to treatments within distributors that are randomly selected from matched blocks of distributors, and 5) an analysis procedure (the odds ratio) that uses whatever data are available from either a nonrandomized or randomized design to measure associations between the probability of HPWH adoption and exposure to various incentive offers and between HPWH adoption and background factors such as education or income.

As a whole, we judge the combined design alternative (Section 4) to be the best choice. There are, however, numerous trade-offs to be made in selecting among alternatives. Therefore, TVA staff may reach a different conclusion. To clarify the process of choosing among alternatives, our criteria for judging the designs and our ratings of them on each criteria are presented in Table 17.

The individual level designs provide the highest degree of internal validity. By internal validity we mean the extent to which observed differences in HPWH adoption rates can be unambiguously attributed to the influence of the financial incentives offered. In other words, internal validity is high if non-treatment explanations for differences in outcomes have been ruled out. Because, in the individual level

Table 17. Comparisons of design advantages and disadvantages

	Blocked comparison at individual level: sign test or Q statistic	One way layout at individual level: completely randomized design	Distributor level assignment of treatments	Combined design: individual level assignment of treatments within distributor	Nonrandomized study: the odds ratio
Administrative difficulty					
to TVA	High	Moderate	Moderate	Moderate	Low
to distributor	High	High	Low	High	Low
Strength of conclusions (internal validity)	High	High	Moderate	High	Low
Generalizability of results (external validity)	Low	Moderate	High	High	Low
Time required for implementation	Low	Low	High	High	Low
Cost/size of sample and data collection effort	Low	Low	High	Moderate	Moderate
Analytical difficulty[a]	Low	Low	Low	Low	Low
Suitability of data base for alternate uses	Low	Moderate	High	High	Not applicable

[a]None of the design alternatives requires complicated statistical analyses. All the tests can be implemented by a person with a background in statistics. Thus, this criterion need not influence choices among the alternatives.

designs, individuals are assigned at random to different treatments, the treatment groups can be assumed to be initially similar in their characteristics. Thus, differences in the composition of the groups receiving different incentives have been controlled as explanations for differences in outcomes. In addition, differences among distributor areas or among the ways in which distributors run their programs cannot influence outcomes (measures of the treatment effects) in the individual level designs because the outcomes are compared only <u>within</u> a given distributor area.

While the internal validity of the individual level designs is high, their external validity, or the generalizability of their results, is low. The individual level designs do not indicate the extent to which

treatment effects observed in one distributor area also will apply to other areas. If the experiments are replicated several times, however, it is possible to increase the generalizability of the results considerably. A major advantage of the combined design (Section 4) is that it not only replicates the individual level experiments but also performs these replications in a variety of distributor strata.

Increases in external validity are not without costs, however. While the individual level designs are quick to implement and require only small sample sizes and inexpensive data collection efforts, the combined design takes longer to implement and requires larger samples and more extensive and expensive data collection efforts. Similarly, the higher external validity of the distributor level design is achieved only with longer implementation times, and with larger sample sizes and data collection efforts. The distributor level design also suffers from lower internal validity because differences among distributors that could affect outcomes cannot be controlled well.

While the distributor level design has lower internal validity than the individual level designs, it is administratively one of the easiest to implement. The possibility of causing customer dissatisfaction and complaints about perceived inequities in incentive offers and of raising legal issues are problems associated only with the individual level designs. The distributor level design does not share these disadvantages. Except for the initial renegotiation of HPWH program contracts to add or change incentive offers, the distributors' program procedure would remain essentially unchanged.

The odds ratio analysis also is administratively easy to implement. Since no formal treatment assignments are required, the odds ratio can be applied to any data set for which information on outcomes and on either incentive offers made or background factors present is available. Because the odds ratio can be used easily in combination with any of the other alternatives, or indeed with nearly any data collection procedures, it is probably worthwhile to include it in whatever analysis approach is ultimately chosen. The fact that the odds ratio is the only technique presented that directly measures the importance of background factors is another reason for combining it with whatever other alternative is chosen.

Even though we recommend combining an odds ratio analysis with any of the other design alternatives, we do not recommend that it be used alone. When used alone, the odds ratio offers no control of confounding factors that may seriously affect the validity of conclusions reached about treatment effects. In addition, the odds ratio does not provide a significance test of observed treatment effects.

A final consideration in choosing among the designs is the suitability of the data base each generates for alternate uses. The individual level design with matching is the most powerful test of treatment effects but is is useful only for that purpose. Because the other designs collect data on larger samples and gather more information over a longer time period, the data they generate are more useful for a variety of purposes. They could be used, for example, to address questions about how to improve program performance or about how to reach different market segments.

As discussed above, there are a number of criteria and tradeoffs that affect choices among designs for testing the effects of financial incentives in TVA's heat pump water heater program. If the administrative difficulties associated with the random assignment of treatments at the individual level can be overcome, this approach with replications in representative distributor areas (as in the combined design) seems to be the best choice. If these administrative difficulties make the combined design infeasible, a distributor level design may be preferred. Final decisions on the appropriate tradeoffs must, of course, be made by TVA.

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APPENDIX A. TVA SURVEYS OF RESIDENTIAL CUSTOMERS

Energy Conservation Program Participant Survey (OMB No. 3316-0031)

(PLEASE MARK YOUR ANSWER BY **PLACING A CHECK MARK IN THE BLOCK** CORRESPONDING TO THE RESPONSE YOU SELECT. DO NOT PUT YOUR NAME ON THIS FORM.)

The first set of questions asks about your interest in the Energy Conservation Program.

1. How did you first hear about this program? (CHECK ONLY ONE.)

A. ☐ Someone else participating in it

B. ☐ A friend, neighbor, or relative who was not participating at that time

C. ☐ Manufacturer, dealer, or contractor who provides services under the program

D. ☐ Newspaper (Which one?) _____

E. ☐ Radio (Which station?) _____

F. ☐ TV (Which station?) _____

G. ☐ Magazine (Name of magazine?) _____

H. ☐ Information sent with my utility bill

I. ☐ Community group (church, community service agency)

J. ☐ Other (PLEASE SPECIFY) _____

2. Below is a list of factors that may have influenced your decision to ask for an energy survey of your home. Please indicate the importance of each factor. (FOR EACH FACTOR LISTED, SELECT ONE IMPORTANCE LEVEL.)

	Very Important	Somewhat Important	Not Important
A. Increasing the comfort of my home	1 ☐	2 ☐	3 ☐
B. Lowering my monthly electric bill	1 ☐	2 ☐	3 ☐
C. Increasing the value of my home	1 ☐	2 ☐	3 ☐
D. Conserving energy	1 ☐	2 ☐	3 ☐
E. Other (SPECIFY) _____	1 ☐	2 ☐	3 ☐

3. How important is (was) each of the following factors in your decision to take any of the actions recommended by the energy advisor? (FOR EACH FACTOR LISTED, SELECT ONE IMPORTANCE LEVEL.)

	Very Important	Somewhat Important	Not Important
A. Availability of an income tax credit	1 ☐	2 ☐	3 ☐
B. Availability of a low-interest or no-interest loan from the utility	1 ☐	2 ☐	3 ☐
C. Possibility of installing the recommended actions yourself	1 ☐	2 ☐	3 ☐
D. Utility making all financing arrangements	1 ☐	2 ☐	3 ☐
E. Cost of the recommended measures	1 ☐	2 ☐	3 ☐
F. Condition of your current heating cooling water-heating equipment	1 ☐	2 ☐	3 ☐
G. Inspection after installation	1 ☐	2 ☐	3 ☐
H. Plans to move to a different house	1 ☐	2 ☐	3 ☐
I. Amount it would save on monthly utility bill	1 ☐	2 ☐	3 ☐

4. How soon would a recommended energy conservation action have to pay for itself in reduced utility bills for you to consider it a worthwhile investment?

A. ☐ 1 year
B. ☐ 2 years
C. ☐ 3 years
D. ☐ 4-5 years
E. ☐ 6-7 years
F. ☐ 8-9 years
G. ☐ 10 years or more

5. How long have you lived in this house?

A. ☐ Less than 1 year
B. ☐ 1 up to 2 years
C. ☐ 2 up to 3 years
D. ☐ 3 up to 4 years
E. ☐ 4 up to 5 years
F. ☐ 5 up to 10 years
G. ☐ 10 years or more

6. Do you think you will move in the next 5 years? ☐ Yes ☐ No

The following information is requested solely for the purpose of determining compliance with Federal Civil Rights Law, and your response will not affect consideration of your application, or your participation in the program. By providing this information you will assist us in assuring that this program is administered in a nondiscriminatory manner.

7. How many persons living in this household are in each of the following age groups? (RECORD THE NUMBER IN EACH AGE GROUP IN THE SPACE TO THE LEFT.)

A. ____Under 10 years
B. ____10-19 years
C. ____20-24 years
D. ____25-34 years
E. ____35-44 years
F. ____45-54 years
G. ____55-64 years
H. ____65 years and over

8. Please indicate the total annual household income before taxes and deductions. Include all sources of income such as wages, interest, retirement, social security, assistance, etc.

_____

(Estimate to the nearest thousand dollars.)

9. How many employed persons are living in this household?

_____

10. Does anyone in this household have a history of physical or mental impairment or condition (for example, vision or hearing loss, diabetes, epilepsy, mental retardation) which substantially limits one or more of his/her major life activities (for example, walking, seeing, hearing, working)?

☐ Yes ☐ No

Please answer the following questions about the person who makes most of the decisions to purchase major appliances or to make major improvements to your house.

11. What is this person's sex? ☐ Male ☐ Female

12. What is this person's age? (CHECK ONLY ONE.)

A. ☐ Under 19 years
B. ☐ 19-24 years
C. ☐ 25-34 years
D. ☐ 35-44 years
E. ☐ 45-54 years
F. ☐ 55-64 years
G. ☐ 65 years and over

13. What is the highest level of education completed by this person? (CHECK ONLY ONE.)

A. ☐ 8th grade or less
B. ☐ Some high school
C. ☐ High school graduate
D. ☐ Some college or technical school
E. ☐ College graduate
F. ☐ Graduate or professional school

14. What is the racial background of this person? (CHECK ONLY ONE.)

A. ☐ **American Indian or Alaskan Native** (a person having origins in any of the original peoples of North America, and who maintains cultural identification through tribal affiliation or community recognition)
B. ☐ **Asian or Pacific Islander** (a person having origins in any of the original peoples of the Far East, Southeast Asia, the Indian subcontinent, or the Pacific Islands, including, for example, China, Japan, India, Korea, the Philippine Islands, and Samoa)
C. ☐ **Black** (a person having origins in any of the black racial groups of Africa)
D. ☐ **White** (a person having origins in any of the original peoples of Europe, North America, or the Middle East)

15. Is this person's national origin Hispanic?

☐ Yes (Hispanic origin means a person of Mexican, Puerto Rican, Cuban, Central or South American, or other Spanish culture or origin, regardless of race.)

☐ No

If you wish, you may enter the country this person claims as the country of national origin (Sweden, Korea, etc.).

_____

16. Which of the following categories best describes the primary work done by the person who makes most of the major purchasing decisions? (CHECK ONLY ONE.)

A. ☐ Technical (for example, engineer, computer programmer, lab technician)
B. ☐ Skilled craft or trade (for example, carpenter, mechanic)
C. ☐ Clerical (for example, secretary, bank teller, file clerk)
D. ☐ Government (for example, city, county, state or Federal employee, but not public school employees)
E. ☐ Professional (for example, doctor, CPA, manager)
F. ☐ Education (for example, teacher, school principal)
G. ☐ Agriculture (for example, farmer, tenant farmer)
H. ☐ Sales (for example, retail sales, real estate)
I. ☐ Manufacturing (for example, operator)
J. ☐ Retired
K. ☐ Other (PLEASE SPECIFY) _____

Residential Survey: Customers of Local Electric Utilities Distributing TVA Power (OMB No. 3316-0050)

The questions in this survey are about:

- ELECTRIC APPLIANCES AND EQUIPMENT COVERED BY YOUR ELECTRIC ACCOUNT.
- NON-ELECTRICAL APPLIANCES AND EQUIPMENT.
- LIVING QUARTERS AND THEIR OCCUPANTS.

If you have more than one electric account in your name, please answer these questions for your account number_____.

THIS FIRST SET OF QUESTIONS IS ABOUT THE LIVING QUARTERS COVERED BY THIS ELECTRIC ACCOUNT. LIVING QUARTERS ARE A GROUP OF ROOMS WITH SEPARATE KITCHEN APPLIANCES SET UP AS LIVING SPACE FOR A FAMILY, A GROUP OF PEOPLE OR ONE PERSON.

1. Which of the following are covered by this electric account? (Circle one number.)

   1 LIVING QUARTERS ONLY IN A HOUSE, APARTMENT, ROOMING HOUSE, A MOBILE HOME OR TRAILER

   2 NON-LIVING QUARTERS ONLY SUCH AS A BUSINESS, A BARN OR WATER PUMP → IF ONLY NON-LIVING QUARTERS ARE COVERED BY THIS ACCOUNT, CHECK HERE ☐ AND PLEASE RETURN THIS QUESTIONNAIRE IN THE POSTAGE-PAID ENVELOPE. THANK YOU FOR YOUR COOPERATION.

   3 BOTH LIVING QUARTERS AND NON-LIVING QUARTERS

(If living quarters are covered by this account.)

2. How many separate living quarters are covered by this account? Count as separate living quarters each group of rooms with separate kitchen appliances set up as living space for a family, a group of people or one person. (Circle one number.)

   1 ONE LIVING QUARTERS

   2 TWO LIVING QUARTERS

   3 THREE OR MORE LIVING QUARTERS

   → IF TWO OR MORE LIVING QUARTERS ARE COVERED BY THIS ELECTRIC ACCOUNT, THE QUESTIONS IN THIS SURVEY ARE EASIER TO ANSWER OVER THE TELEPHONE. SO THAT WE MAY ASK YOU THESE SAME QUESTIONS, PLEASE CALL TOLL FREE TO (800) 638-8985 BETWEEN THE HOURS OF 10:00 AM AND 10:00 PM. ASK FOR CAROL GREEN. THANK YOU FOR YOUR HELP.

GO TO QUESTION 3 ON PAGE 2.

(If one living quarters.)

3. In which of the following are these living quarters located? (Circle one number.)

1 SINGLE FAMILY HOUSE
2 TWO FAMILY HOUSE (DUPLEX)
3 BUILDING WITH 3 OR 4 UNITS
4 BUILDING WITH 5 OR MORE UNITS
5 MOBILE HOME OR TRAILER
6 OTHER TYPE OF BUILDING (DESCRIBE) _____

4. Are these living quarters usually lived in all year round or only parts of the year?

1 LIVED IN ALL YEAR ROUND
2 LIVED IN ONLY PARTS OF THE YEAR

5. Do you live in these living quarters? (Circle one number.)

1 YES
2 NO, OTHER PEOPLE LIVE IN THESE LIVING QUARTERS
3 NO, LIVING QUARTERS ARE VACANT

IF YOU DO NOT LIVE IN THESE LIVING QUARTERS PLEASE CALL TOLL FREE TO (800) 638-8985 BETWEEN THE HOURS OF 10:00 AM AND 10:00 PM. ASK FOR CAROL GREEN. THANK YOU FOR YOUR HELP.

(If you live in these living quarters.)

6. How long have you lived here? (Circle one number.)

1 LESS THAN 1 YEAR
2 ONE UP TO TWO YEARS
3 TWO UP TO THREE YEARS
4 THREE UP TO FOUR YEARS
5 FOUR UP TO FIVE YEARS
6 FIVE OR MORE YEARS

7. Do you own these living quarters, rent them for cash or live in them without payment of cash rent? (Circle one number.)

1 OWN
2 RENT FOR CASH
3 LIVE IN WITHOUT PAYMENT OF CASH RENT
4 OTHER (EXPLAIN) _____

8. About how old are these living quarters? (Circle one number.)

1 LESS THAN 1 YEAR OLD
2 ONE UP TO TWO YEARS OLD
3 TWO UP TO THREE YEARS OLD
4 THREE UP TO FOUR YEARS OLD
5 FOUR UP TO FIVE YEARS OLD
6 FIVE OR MORE YEARS OLD

9. Counting all areas that are used as living space, how many rooms are there in these living quarters? Do not count bathrooms, foyers or hallways.

   NUMBER OF ROOMS: _____

10. Approximately how many square feet of living space are there in these living quarters? Include bathrooms, foyers and hallways. (To help you estimate, a room that measures 6 feet by 8 feet is 48 square feet, a 10 foot by 10 foot room is 100 square feet, a 9 foot by 12 foot room is 108 square feet and a room 12 feet by 15 feet is 180 square feet.) (Circle one number.)

    01 UNDER 500 SQUARE FEET
    02 500 UP TO 800 SQUARE FEET
    03 800 UP TO 1100 SQUARE FEET
    04 1100 UP TO 1400 SQUARE FEET
    05 1400 UP TO 1700 SQUARE FEET
    06 1700 UP TO 2000 SQUARE FEET
    07 2000 UP TO 2500 SQUARE FEET
    08 2500 UP TO 3000 SQUARE FEET
    09 3000 UP TO 3500 SQUARE FEET
    10 3500 UP TO 4000 SQUARE FEET
    11 4000 OR MORE SQUARE FEET

11. During the last <u>five</u> years, have you or the owner of these living quarters taken any of the following steps to save energy in <u>these</u> living quarters: (PLEASE ANSWER <u>EACH</u> ITEM BY CIRCLING YES OR NO.)

    PUT IN ATTIC INSULATION?
    1 YES
    2 NO
    ↓
    PUT IN FLOOR INSULATION?
    1 YES
    2 NO
    ↓
    PUT IN WEATHERSTRIPPING?
    1 YES
    2 NO
    ↓
    CLOSED OFF UNUSED ROOMS?
    1 YES
    2 NO
    ↓
    INSTALLED ATTIC FAN(S)?
    1 YES
    2 NO
    ↓
    ADDED ROOF VENT(S)?
    1 YES
    2 NO
    ↓
    ADDED STORM WINDOWS?
    1 YES
    2 NO
    ↓
    ADDED STORM DOORS?
    1 YES
    2 NO

THE NEXT SET OF QUESTIONS IS ABOUT THE HEATING AND COOLING EQUIPMENT AND FUEL IN THESE LIVING QUARTERS.

12. What is the heating equipment used most of the time in these living quarters? (Circle one number.)

01 HEAT PUMP
02 CENTRAL FURNACE
03 FLOOR FURNACE
04 CEIL HEAT (RADIANT HEAT IN CEILING)
05 BUILT-IN INDIVIDUAL SPACE HEATERS ONLY
06 PORTABLE INDIVIDUAL SPACE HEATERS ONLY
07 BOTH BUILT-IN AND PORTABLE INDIVIDUAL SPACE HEATERS
08 WOOD STOVE
09 NON-WOOD BURNING STOVE
10 FIREPLACE
11 OTHER (DESCRIBE) _____
12 NONE, HAVE NO HEAT → SKIP TO QUESTION 24 ON PAGE 7.

13. How old is this heating equipment? (Circle one number.)

1 LESS THAN 1 YEAR OLD
2 ONE UP TO TWO YEARS OLD
3 TWO UP TO THREE YEARS OLD
4 THREE UP TO FOUR YEARS OLD
5 FOUR UP TO FIVE YEARS OLD
6 FIVE OR MORE YEARS OLD → SKIP TO QUESTION 16 ON PAGE 5.

(If equipment less than 5 years old.)

14. Was the type of heating equipment used most of the time in these living quarters changed within the past five years? (Circle one number.)

1 YES
2 NO, TYPE OF HEATING EQUIPMENT NOT CHANGED
3 NO, USED NO TYPE OF HEATING EQUIPMENT BEFORE
→ SKIP TO QUESTION 16 ON PAGE 5.

(If equipment changed within past 5 years.)

15. What kind of heating equipment was used most of the time just before the change? (Circle one number.)

01 HEAT PUMP
02 CENTRAL FURNACE
03 FLOOR FURNACE
04 CEIL HEAT (RADIANT HEAT IN CEILING)
05 BUILT-IN INDIVIDUAL SPACE HEATERS ONLY
06 PORTABLE INDIVIDUAL SPACE HEATERS ONLY
07 BOTH BUILT-IN AND PORTABLE INDIVIDUAL SPACE HEATERS
08 WOOD STOVE
09 NON-WOOD BURNING STOVE
10 FIREPLACE
11 OTHER (DESCRIBE) _____

16. What is the fuel for the heating equipment used most of the time in these living quarters? (Circle one number.)

1 ELECTRICITY
2 NATURAL GAS
3 BOTTLED LP GAS (BUTANE, PROPANE, TANK GAS)
4 FUEL OIL OR KEROSENE
5 COAL
6 WOOD
7 SOLAR ENERGY
8 OTHER (DESCRIBE) _____

17. For how many years has this been the fuel used most of the time for heating these living quarters? (Circle one number.)

1 LESS THAN 1 YEAR
2 ONE UP TO TWO YEARS
3 TWO UP TO THREE YEARS
4 THREE UP TO FOUR YEARS
5 FOUR UP TO FIVE YEARS
6 FIVE OR MORE YEARS → SKIP TO QUESTION 20 ON PAGE 6.

(If fuel used for less than 5 years.)

18. Was the type of fuel used most of the time for heating these living quarters changed within the past five years? (Circle one number.)

1 YES
2 NO, TYPE OF FUEL NOT CHANGED
3 NO, USED NO TYPE OF FUEL FOR HEATING BEFORE

→ SKIP TO QUESTION 20 ON PAGE 6.

(If fuel type changed within past 5 years.)

19. Which of the following fuels was used most of the time for heating these living quarters just before the change? (Circle one number.)

1 ELECTRICITY
2 NATURAL GAS
3 BOTTLED LP GAS (BUTANE, PROPANE, TANK GAS)
4 FUEL OIL OR KEROSENE
5 COAL
6 WOOD
7 SOLAR ENERGY
8 OTHER (DESCRIBE) _____

20. IN ADDITION TO THE HEATING SYSTEM USED MOST OF THE TIME, are any of the following used to heat these living quarters? (PLEASE ANSWER EACH ITEM BY CIRCLING YES OR NO.)

ELECTRICITY?
1 YES
2 NO

NATURAL GAS?
1 YES
2 NO

BOTTLED LP GAS? (BUTANE, PROPANE, TANK GAS)
1 YES
2 NO

FUEL OIL OR KEROSENE?
1 YES
2 NO

COAL?
1 YES
2 NO

WOOD?
1 YES
2 NO

SOLAR ENERGY?
1 YES
2 NO

OTHER?
1 YES (DESCRIBE) _____
2 NO

21. IN ADDITION TO THE HEATING SYSTEM USED MOST OF THE TIME, is a wood stove used to heat these living quarters?

1 YES → What fuel(s) is used in the wood stove(s)? (CIRCLE ALL THAT APPLY.)
1 WOOD
2 COAL
3 OTHER (DESCRIBE) _____

2 NO

GO TO QUESTION 22 ON PAGE 7.

22. IN THE WINTER are these living quarters kept at a different temperature during the day than during the night?

1 YES → What temperature are these living quarters kept during most winter days? _____ °F
What temperature are these living quarters kept during most winter nights? _____ °F

2 NO → What temperature are these living quarters kept during most of the winter? _____ °F

23. During most of the winter, are these living quarters kept cooler than 5 years ago?

1 YES

2 NO

24. Is there air-conditioning in all or part of these living quarters?

1 YES

2 NO → *SKIP TO QUESTION 28 ON PAGE 8.*

25. What type of equipment is used to air-condition these living quarters? *(Circle one number.)*

1 CENTRAL AIR-CONDITIONING UNIT ONLY

2 INDIVIDUAL WINDOW OR WALL UNITS ONLY

3 BOTH CENTRAL AND INDIVIDUAL UNITS

26. IN THE SUMMER are these living quarters kept at a different temperature during the day than during the night?

1 YES → What temperature are these living quarters kept during most summer days? _____ °F
What temperature are these living quarters kept during most summer nights? _____ °F

2 NO → What temperature are these living quarters kept during most of the summer? _____ °F

27. During most of the summer, are these living quarters kept warmer than 5 years ago?

1 YES

2 NO

THIS NEXT SET OF QUESTIONS IS ABOUT APPLIANCES USED IN THESE LIVING QUARTERS.

28. Are any of the following appliances used in these living quarters? (FOR EACH APPLIANCE BELOW CIRCLE YES IF THE APPLIANCE IS USED, OR NO IF IT IS NOT USED. IF "YES," FOLLOW THE ARROW AND ANSWER ANY QUESTIONS ABOUT THE APPLIANCE BEFORE GOING ON TO THE NEXT APPLIANCE; IF "NO," CONTINUE WITH THE NEXT APPLIANCE.)

WATER HEATER?

1 YES → How many water heaters are used? TOTAL NUMBER: _____

How many of these water heaters use each energy source? (PUT THE NUMBER USING EACH ENERGY SOURCE ON THE LINE NEXT TO THE SOURCE.)

_____ ELECTRICITY

_____ GAS

_____ SOLAR ENERGY

_____ WOOD

_____ OTHER (DESCRIBE) _____

2 NO

KITCHEN RANGE OR COOKSTOVE?

1 YES → How many ranges or cookstoves are used? TOTAL NUMBER: _____

How many of these ranges or cookstoves use each energy source? (PUT THE NUMBER USING EACH ENERGY SOURCE ON THE LINE NEXT TO THE SOURCE.)

_____ ELECTRICITY

_____ GAS

_____ SOLAR ENERGY

_____ WOOD

_____ OTHER (DESCRIBE) _____

2 NO

MICROWAVE OVEN?

1 YES

2 NO

ELECTRIC DISHWASHER?

1 YES

2 NO

ELECTRIC REFRIGERATOR?

1 YES

2 NO

SEPARATE FOOD FREEZER?

1 YES

2 NO

ELECTRIC CLOTHES WASHER?

1 YES

2 NO

CLOTHES DRYER?

1 YES → How many dryers are used? TOTAL NUMBER: _____

How many of these dryers use each energy source? (PUT THE NUMBER USING EACH ENERGY SOURCE ON THE LINE NEXT TO THE SOURCE.)

_____ ELECTRICITY

_____ GAS

2 NO

TELEVISION?

1 YES → How many television sets do you have that are normally operated at least once a week? TOTAL NUMBER: _____

2 NO

THIS NEXT SET OF QUESTIONS IS ABOUT THE PEOPLE WHO LIVE IN THESE LIVING QUARTERS.

29. How many people, including yourself, usually live in these living quarters?

NUMBER: _____

30. Of these people, how many are in each age group? (PUT THE NUMBER OF PEOPLE IN EACH AGE GROUP ON THE LINE NEXT TO THE GROUP.)

9 YEARS OLD AND UNDER: _____

10-19 YEARS OLD: _____

20-34 YEARS OLD: _____

35-49 YEARS OLD: _____

50-64 YEARS OLD: _____

65 YEARS AND OVER: _____

31. Of these people how many are employed full-time (35 hours or more a week) or part-time (less than 35 hours a week), or not at all?

NUMBER EMPLOYED FULL-TIME: _____

NUMBER EMPLOYED PART-TIME: _____

NUMBER NOT EMPLOYED: _____

GO TO PAGE 10.

32. Have these living quarters ever had a Home Energy Survey done by your local electric utility or TVA? The Home Energy Survey is done by a trained energy advisor to find out what changes can be made in your living quarters to save energy. (Circle one number.)

1 YES → SKIP TO QUESTION 34.

2 NO

3 NOT SURE

33. Would you like your living quarters to receive a free Home Energy Survey scheduled at your convenience? (Circle one number.)

1 YES

2 WOULD LIKE MORE INFORMATION → WE WILL GIVE YOUR NAME TO TVA SO THAT YOUR REQUEST CAN BE MET.

3 NO

34. Would you please indicate which of the following best describes your total family income from all sources before taxes in 1981? (Circle one number.)

01 LESS THAN $5,000

02 $ 5,000 - $ 6,999

03 $ 7,000 - $ 9,999

04 $10,000 - $14,999

05 $15,000 - $19,999

06 $20,000 - $24,999

07 $25,000 - $29,999

08 $30,000 - $34,999

09 $35,000 - $39,999

10 $40,000 OR MORE

35. Finally, could you please give us a phone number at which you can be reached just in case we have any questions about your answers?

|___|___|___| - |___|___|___| - |___|___|___|___|
(AREA CODE)

APPENDIX B. PROCEDURES FOR RANDOMIZATION

This appendix presents methods to draw random samples for the designs discussed in Sections 2 and 3. We first discuss a method to assign households to different treatments (loan, rebate, information only). Then we discuss a method to assign treatments to blocks of either households or distributors.

COMPLETELY RANDOMIZED DESIGN

Randomization forms the basis of all valid statistical tests used in this report. The role of randomization in executing an experiment is well presented in Chapter 1 of Controlled Experiment by Freedman, Pisan, and Purves (1978) where they used the Salk vaccine field trial as a simple example.

To assist in assigning treatments at random to the units, we suggest the following procedures suitable for the designs discussed in this report.

For each household i in the set $1, \ldots, N$, the following algorithm will match i with a treatment number T(i) in the set $1, \ldots, k$ so that each of the treatments represented by a number in $1, \ldots k$ will be randomly assigned to the subjects represented by N numbers in the set $1, \ldots, N$.

First, we generate a sequence $Z_1, Z_2, \ldots, Z_N$ by continuously taking simple random samples of size one without replacement from the set $1, \ldots, N$. Next we define the function T from $1, \ldots, N$ to $1, \ldots, k$ as follows:

$$T(Z_u) = j \text{ if } u \text{ is in the set of } [(j-1)n_0] + 1, \ldots, (j)n_0$$

where $j = 1, 2, \ldots, k$.

RANDOMIZED BLOCK DESIGN

The procedure for a randomized block design is straightforward. Within each block a random permutation is used to assign treatments to experimental units. Independent permutations are selected for the several blocks.

Suppose treatment assignments need be made to seven households in a block. We number the households from 1 to 7. We use Table 15.5 from Cochran and Cox (1957), which contains random permutations of 9 (we use these since a table of 7 is not available). This table has been obtained by selecting permutations of 1 to 9 at random, thus making each permutation equally likely. Suppose we pick the random permutation in the lower right corner:

1 8 6 4 5 9 2 7 3

We drop the numbers 8 and 9 since we have only seven experimental units, and obtain the following random assignment:

Eligible household:	1	6	4	5	2	7	3
Treatment:	T_1	T_2	T_3	T_4	T_5	T_6	T_7

Thus, household 6 receives treatment 2 and so on.

15.5 Tables of Random Permutations

TABLE 15.6 PERMUTATIONS OF 9

5 5 6 7 1	4 3 3 7 3	8 7 4 6 3	9 7 4 9 4	9 2 2 8 8	2 7 9 3 5	8 3 1 9 4
4 1 2 8 2	7 1 1 2 9	9 5 7 8 2	8 9 3 6 6	1 7 7 2 4	4 8 5 7 3	3 7 4 5 6
9 3 3 2 9	8 8 8 4 5	2 4 6 1 6	3 6 7 7 8	7 4 4 7 1	7 3 2 8 6	6 1 2 2 2
7 9 7 4 3	5 5 2 9 2	1 6 5 3 5	7 8 5 1 9	5 1 9 1 3	6 5 1 4 9	2 9 8 7 8
1 6 9 6 5	6 9 4 3 6	4 3 9 2 9	5 1 8 2 3	8 3 3 3 2	8 9 6 1 2	4 5 7 6 9
6 4 4 3 6	2 4 6 8 1	7 9 3 4 1	6 2 6 4 2	2 9 8 5 9	9 2 4 2 8	9 6 9 8 1
8 7 8 1 7	1 2 5 6 8	3 1 2 9 8	4 4 1 8 7	6 5 1 6 7	5 4 3 5 1	1 4 3 1 7
3 2 1 9 4	3 6 7 5 7	6 8 8 7 7	2 5 9 5 1	3 8 5 4 6	3 6 7 9 4	5 2 5 4 5
2 8 5 5 8	9 7 9 1 4	5 2 1 5 4	1 3 2 3 5	4 6 6 9 5	1 1 8 6 7	7 8 6 3 3
7 4 6 1 5	9 2 2 2 9	2 8 1 7 3	2 4 2 1 9	2 4 8 3 1	2 6 5 4 8	8 4 9 4 2
9 3 8 3 2	1 1 1 9 8	9 4 9 5 4	8 8 8 8 6	7 7 5 4 6	5 3 2 7 6	9 3 8 2 1
1 6 3 4 7	6 5 8 4 5	6 1 7 1 9	5 2 5 6 3	8 5 7 5 5	6 9 9 8 1	3 6 7 9 7
6 8 2 8 4	4 8 7 8 6	5 7 5 4 5	9 6 7 5 8	5 9 9 7 7	8 5 3 3 5	6 9 4 6 9
4 1 4 7 8	2 3 9 3 4	4 2 2 3 6	4 7 4 2 5	6 3 3 6 9	1 7 8 5 4	4 5 2 1 4
2 9 1 9 3	7 9 6 6 2	1 6 4 6 1	7 9 9 7 4	1 8 4 1 8	9 2 7 9 3	1 8 3 5 5
5 5 5 5 1	3 7 4 7 7	8 5 8 9 2	1 5 1 3 2	9 6 2 8 4	3 8 1 1 9	5 7 1 3 3
8 2 9 2 9	8 6 5 5 3	7 9 6 8 8	3 1 6 9 7	4 1 6 9 3	4 4 6 6 2	7 2 6 8 8
3 7 7 6 6	5 4 3 1 1	3 3 3 2 7	6 3 3 4 1	3 2 1 2 2	7 1 4 2 7	2 1 5 7 6
9 7 7 5 5	9 9 9 3 8	9 8 6 1 7	5 8 6 1 2	1 9 8 3 3	3 1 7 7 3	7 6 6 5 5
3 8 1 7 2	6 2 7 1 6	4 1 3 4 2	3 6 2 4 3	2 6 1 2 8	8 8 6 2 7	8 9 7 4 7
4 3 4 2 7	7 3 1 7 2	1 5 4 8 6	6 2 1 6 1	7 8 5 1 7	5 9 1 3 6	3 1 2 3 1
5 9 2 8 3	3 7 5 8 9	2 9 1 7 1	2 3 8 3 4	3 5 9 9 9	7 2 3 4 1	5 7 1 7 8
1 6 5 1 1	5 6 4 4 1	7 3 7 2 3	4 7 3 8 8	9 3 2 5 6	6 6 9 5 9	9 8 9 1 2
6 2 8 3 6	8 4 6 2 5	5 2 2 6 8	9 1 7 5 6	4 7 4 6 4	1 7 4 6 4	1 2 8 8 6
2 4 9 6 4	1 8 3 5 4	3 6 5 9 4	8 5 9 7 9	8 1 6 8 1	4 5 5 9 5	2 4 5 9 4
8 5 6 9 9	2 5 2 6 7	8 7 8 3 9	1 9 4 2 5	6 4 7 4 5	2 3 2 8 2	6 3 3 2 3
7 1 3 4 8	4 1 8 9 3	6 4 9 5 5	7 4 5 9 7	5 2 3 7 2	9 4 8 1 8	4 5 4 6 9
7 4 9 8 7	9 7 1 7 1	9 2 3 8 7	7 8 5 3 5	5 1 6 4 9	7 8 6 1 8	2 9 7 3 4
5 6 1 1 2	6 4 6 1 4	5 9 1 2 8	2 4 6 8 7	7 3 7 6 1	5 1 7 4 1	9 3 4 7 7
4 9 3 5 6	1 1 8 4 8	3 5 4 9 3	3 6 1 2 3	2 6 8 7 7	4 5 3 8 5	8 5 9 5 1
3 3 2 2 8	5 2 3 2 2	7 3 8 6 9	4 1 8 6 1	1 9 2 3 6	3 9 5 7 7	1 2 8 1 2
2 1 4 9 4	4 6 2 8 3	2 7 6 5 1	5 7 3 1 2	9 8 4 1 3	6 3 1 2 9	6 1 5 8 8
9 7 5 4 5	3 9 7 9 9	1 4 2 3 4	6 9 7 4 4	3 2 5 2 2	8 4 2 6 3	5 6 3 6 3
6 2 6 3 9	8 8 5 5 5	8 6 7 7 2	9 3 4 5 8	8 7 9 9 4	9 2 4 9 4	4 8 1 2 9
8 5 8 7 1	2 3 9 3 7	4 1 5 1 5	8 5 9 7 6	4 5 3 5 8	1 6 8 5 2	3 4 6 4 5
1 8 7 6 3	7 5 4 6 6	6 8 9 4 6	1 2 2 9 9	6 4 1 8 5	2 7 9 3 6	7 7 2 9 6
8 4 6 8 6	2 1 9 9 7	2 2 1 8 9	5 1 9 2 4	5 2 6 2 8	1 6 8 8 3	8 1 9 4 1
9 9 4 5 8	4 4 8 7 8	8 7 5 9 7	3 6 4 7 7	3 8 5 3 6	4 4 6 7 7	6 6 8 7 8
6 6 3 1 1	6 8 3 1 9	7 5 7 5 5	6 5 1 8 5	2 4 3 8 2	5 1 4 3 6	4 9 7 8 6
7 3 7 7 2	7 3 6 2 2	3 8 9 4 6	4 7 2 6 9	7 9 7 4 1	3 8 2 6 5	3 5 3 1 4
2 8 9 3 4	1 5 5 5 1	5 4 3 6 4	7 8 7 5 3	9 5 8 6 5	8 2 7 9 2	5 3 4 3 5
3 7 2 6 9	8 6 4 6 3	4 1 8 2 1	1 9 6 4 8	4 7 2 1 3	6 3 5 5 1	2 2 6 9 9
5 1 8 4 5	9 9 1 8 4	1 9 4 3 2	8 2 8 9 6	6 3 4 9 9	2 7 1 2 4	9 8 2 6 2
4 5 5 2 7	3 2 7 3 6	9 3 2 1 8	9 3 5 1 2	1 6 9 7 7	9 5 9 1 8	7 7 1 5 7
1 2 1 9 3	5 7 2 4 5	6 6 6 7 3	2 4 3 3 1	8 1 1 5 4	7 9 3 4 9	1 4 5 2 3

From Cochran, W. G., and G. M. Cox (1957) *Experimental Designs*, John Wiley and Sons, Inc. with permission of the publisher.

APPENDIX C. MODELS FOR COMPARISON OF TREATMENTS

The test of treatment effect can be applied to two different comparison problems, summarized in the following models.

Model 1. Randomized Model: There are n matched blocks of k objects each. The N = nk objects are not chosen but are given; chance enters only through the random assignments within each block to treatments. This is called a randomized complete block design; it has the usual disadvantage of restricting the inference to the subjects in the study.

Model 2: Population Model:

(1) We take the model to be

$$x_{ij} = \mu + \beta_i + \tau_j + e_{ij}, \; i=1,\ldots, n, \; j=1,\ldots, k$$

where μ is the unknown overall mean, β_i is the block i effect (the βs are unknown nuisance parameters), τ_j is the unknown treatment j effect, $\sum_{i=1}^{n} \beta_i=0$, and $\sum_{j=1}^{k} \tau_j=0$.

(2) The e's (error variables) are mutually independent.

(3) Each e comes from the same continuous population.

The hypothesis of no treatment differences is

$$H_0: \; \tau_1 = \tau_2 = \ldots = \tau_k.$$

Somewhat different assumptions frequently are more realistic. Let $\Pi_1, \ldots, \Pi_h$ = denote the different strata, and suppose that a sample of size k is drawn from each. Let X_{ij} denote the

response of a subject drawn at random from Π_i and receiving treatment j. If the subpopulations $\Pi_1, \ldots, \Pi_h$ are large enough so that subjects in the sample can be taken to be independent, we are thus led to Model 2.

APPENDIX D. STATISTICAL TABLES

The tables are reproduced from Hollander, Myles and Douglas A. Wolfe (1973) Nonparametric Statistical Methods, John Wiley and Sons, Inc. with permission of the publisher.

Table A.15. Upper tail probabilities for the null distribution of Friedman's S statistic:

$k = 3, n = 2(1)13;\ k = 4, n = 2(1)8;\ k = 5, n = 3, 4, 5$

For given k and n, the tabled entry for the point x is $P_0\{S \geq x\}$. Under these conditions, if x is such that $P_0\{S \geq x\} = \alpha$, then $s(\alpha, k, n) = x$. For given k and n, the entries are terminated at $x_{k,n}$, where $x_{k,n}$ is the smallest value of x such that $P_0\{S \geq x\}$ is zero to three decimal places.

$k = 3, n = 2$

x	$P_0\{S \geq x\}$
0	1.000
1	.833
3	.500
4	.167

$k = 3, n = 3$

x	$P_0\{S \geq x\}$
.000	1.000
.667	.944
2.000	.528
2.667	.361
4.667	.194
6.000	.028

$k = 3, n = 4$

x	$P_0\{S \geq x\}$
.0	1.000
.5	.931
1.5	.653
2.0	.431
3.5	.273
4.5	.125
6.0	.069
6.5	.042
8.0	.005

$k = 3, n = 5$

x	$P_0\{S \geq x\}$
.0	1.000
.4	.954
1.2	.691
1.6	.522
2.8	.367
3.6	.182
4.8	.124
5.2	.093
6.4	.039
7.6	.024
8.4	.008
10.0	.001

$k = 3, n = 6$

x	$P_0\{S \geq x\}$
.000	1.000
.333	.956
1.000	.740
1.333	.570
2.333	.430
3.000	.252
4.000	.184
4.333	.142
5.333	.072
6.333	.052
7.000	.029
8.333	.012
9.000	.008
9.333	.006
10.333	.002
12.000	.000

$k = 3, n = 7$

x	$P_0\{S \geq x\}$
.000	1.000
.286	.964
.857	.768
1.143	.620
2.000	.486
2.571	.305
3.429	.237
3.714	.192
4.571	.112
5.429	.085
6.000	.051
7.143	.027
7.714	.021
8.000	.016
8.857	.008
10.286	.004
10.571	.003
11.143	.001
12.286	.000

$k = 3, n = 8$

x	$P_0\{S \geq x\}$
.00	1.000
.25	.967
.75	.794
1.00	.654
1.75	.531
2.25	.355
3.00	.285
3.25	.236
4.00	.149
4.75	.120

$k = 3, n = 8$

x	$P_0\{S \geq x\}$
5.25	.079
6.25	.047
6.75	.038
7.00	.030
7.75	.018
9.00	.010
9.25	.008
9.75	.005
10.75	.002
12.00	.001
12.25	.001
13.00	.000

$k = 3, n = 9$

x	$P_0\{S \geq x\}$
.000	1.000
.222	.971
.667	.814
.889	.685
1.556	.569
2.000	.398
2.667	.328
2.889	.278
3.556	.187
4.222	.154
4.667	.107
5.556	.069
6.000	.057
6.222	.048
6.889	.031
8.000	.019

Table A. 15 (continued)

$k = 3, n = 9$

x	$P_0\{S \geq x\}$
8.222	.016
8.667	.010
9.556	.006
10.667	.004
10.889	.003
11.556	.001
12.667	.001
13.556	.000

$k = 3, n = 10$

x	$P_0\{S \geq x\}$
.0	1.000
.2	.974
.6	.830
.8	.710
1.4	.601
1.8	.436
2.4	.368
2.6	.316
3.2	.222
3.8	.187
4.2	.135
5.0	.092
5.4	.078
5.6	.066
6.2	.046
7.2	.030
7.4	.026
7.8	.018
8.6	.012
9.6	.007
9.8	.006
10.4	.003
11.4	.002
12.2	.001
12.6	.001
12.8	.001
13.4	.000

$k = 3, n = 11$

x	$P_0\{S \geq x\}$
.000	1.000
.182	.976
.545	.844
.727	.732
1.273	.629
1.636	.470
2.182	.403
2.364	.351
2.909	.256
3.455	.219
3.818	.163
4.545	.116
4.909	.100
5.091	.087
5.636	.062
6.545	.043
6.727	.038
7.091	.027
7.818	.019
8.727	.013
8.909	.011
9.455	.006
10.364	.004
11.091	.003
11.455	.002
11.636	.001
12.182	.001
13.273	.001
13.636	.000

$k = 3, n = 12$

x	$P_0\{S \geq x\}$
.000	1.000
.167	.978
.500	.856
.667	.751

$k = 3, n = 12$

x	$P_0\{S \geq x\}$
1.167	.654
1.500	.500
2.000	.434
2.167	.383
2.667	.287
3.167	.249
3.500	.191
4.167	.141
4.500	.123
4.667	.108
5.167	.080
6.000	.058
6.167	.051
6.500	.038
7.167	.027
8.000	.020
8.167	.017
8.667	.011
9.500	.007
10.167	.005
10.500	.004
10.667	.003
11.167	.002
12.167	.002
12.500	.001
12.667	.001
13.167	.001
13.500	.000

$k = 3, n = 13$

x	$P_0\{S \geq x\}$
.000	1.000
.154	.980
.462	.866
.615	.767
1.077	.675

$k = 3, n = 13$

x	$P_0\{S \geq x\}$
1.385	.527
1.846	.463
2.000	.412
2.462	.316
2.923	.278
3.231	.217
3.846	.165
4.154	.145
4.308	.129
4.769	.098
5.538	.073
5.692	.065
6.000	.050
6.615	.037
7.385	.028
7.538	.025
8.000	.016
8.769	.012
9.385	.009
9.692	.007
9.846	.005
10.308	.004
11.231	.003
11.538	.002
11.692	.002
12.154	.001
12.462	.001
12.923	.001
14.000	.001
14.308	.000

$k = 4, n = 2$

x	$P_0\{S \geq x\}$
.0	1.000
.6	.958
1.2	.833

Table A.15 (continued)

$k = 4, n = 2$

x	$P_0\{S \geq x\}$
1.8	.792
2.4	.625
3.0	.542
3.6	.458
4.2	.375
4.8	.208
5.4	.167
6.0	.042

$k = 4, n = 3$

x	$P_0\{S \geq x\}$
.2	1.000
.6	.958
1.0	.910
1.8	.727
2.2	.608
2.6	.524
3.4	.446
3.8	.342
4.2	.300
5.0	.207
5.4	.175
5.8	.148
6.6	.075
7.0	.054
7.4	.033
8.2	.017
9.0	.002

$k = 4, n = 4$

x	$P_0\{S \geq x\}$
.0	1.000
.3	.992
.6	.928
.9	.900
1.2	.800
1.5	.754
1.8	.677
2.1	.649
2.4	.524
2.7	.508
3.0	.432
3.3	.389
3.6	.355
3.9	.324
4.5	.242
4.8	.200
5.1	.190
5.4	.158
5.7	.141
6.0	.105
6.3	.094
6.6	.077
6.9	.068
7.2	.054
7.5	.052
7.8	.036
8.1	.033
8.4	.019
8.7	.014
9.3	.012
9.6	.007
9.9	.006
10.2	.003
10.8	.002
11.1	.001
12.0	.000

$k = 4, n = 5$

x	$P_0\{S \geq x\}$
.12	1.000
.36	.975
.60	.944
1.08	.857
1.32	.771
1.56	.709
2.04	.652
2.28	.561
2.52	.521
3.00	.445
3.24	.408
3.48	.372
3.96	.298
4.20	.260
4.44	.226
4.92	.210
5.16	.162
5.40	.151
5.88	.123
6.12	.107
6.36	.093
6.84	.075
7.08	.067
7.32	.055
7.80	.044
8.04	.034
8.28	.031
8.76	.023
9.00	.020
9.24	.017
9.72	.012
9.96	.009
10.20	.007
10.68	.005
10.92	.003
11.16	.002
11.64	.002
11.88	.002
12.12	.001
12.60	.001
12.84	.000

$k = 4, n = 6$

x	$P_0\{S \geq x\}$
.0	1.000
.2	.996
.4	.957
.6	.940
.8	.874
1.0	.844
1.2	.789
1.4	.772
1.6	.679
1.8	.668
2.0	.609
2.2	.574
2.4	.541
2.6	.512
3.0	.431
3.2	.386
3.4	.375
3.6	.338
3.8	.317
4.0	.270
4.2	.256
4.4	.230
4.6	.218
4.8	.197
5.0	.194
5.2	.163
5.4	.155
5.6	.127
5.8	.114
6.2	.108
6.4	.089
6.6	.088
6.8	.073
7.0	.066
7.2	.060
7.4	.056
7.6	.043
7.8	.041
8.0	.037
8.2	.035
8.4	.032
8.6	.029
8.8	.023
9.0	.022
9.4	.017
9.6	.014
9.8	.013
10.0	.010
10.2	.010
10.4	.009
10.6	.007

Table A.15 (continued)

$k = 4, n = 6$

x	$P_0\{S \geq x\}$
10.8	.006
11.0	.006
11.4	.004
11.6	.003
11.8	.003
12.0	.002
12.2	.002
12.6	.001
12.8	.001
13.0	.001
13.2	.001
13.4	.001
13.6	.000

$k = 4, n = 7$

x	$P_0\{S \geq x\}$
.086	1.000
.257	.984
.429	.963
.771	.906
.943	.845
1.114	.800
1.457	.757
1.629	.685
1.800	.652
2.143	.590
2.314	.557
2.486	.524
2.829	.456
3.000	.418
3.171	.382
3.514	.366
3.686	.310
3.857	.297
4.200	.262
4.371	.239
4.543	.220
4.886	.195
5.057	.180
5.229	.161
5.571	.143
5.743	.122
5.914	.118
6.257	.100
6.429	.093
6.600	.085
6.943	.073
7.114	.063
7.286	.056
7.629	.052
7.800	.041
7.971	.038
8.314	.035
8.486	.033
8.657	.030
9.000	.023
9.171	.020
9.343	.017
9.686	.015
9.857	.013
10.029	.012
10.371	.010
10.543	.009
10.714	.008
11.057	.007
11.229	.005
11.400	.004
11.743	.004
11.914	.003
12.086	.003
12.429	.002
12.600	.002
12.771	.002
13.114	.001
13.286	.001
13.457	.001
13.800	.001
13.971	.001
14.143	.001
14.486	.000

$k = 4, n = 8$

x	$P_0\{S \geq x\}$
.00	1.000
.15	.998
.30	.971
.45	.959
.60	.912
.75	.890
.90	.849
1.05	.837
1.20	.765
1.35	.757
1.50	.710
1.65	.681
1.80	.654
1.95	.629
2.25	.558
2.40	.517
2.55	.507
2.70	.471
2.85	.450
3.00	.404
3.15	.389
3.30	.362
3.45	.350
3.60	.326
3.75	.323
3.90	.287
4.05	.278
4.20	.242
4.35	.226
4.65	.219
4.80	.193
4.95	.191
5.10	.168
5.25	.158
5.40	.148
5.55	.141
5.70	.121
5.85	.117
6.00	.110
6.15	.106
6.30	.100
6.45	.094

$k = 4, n = 8$

x	$P_0\{S \geq x\}$
6.60	.081
6.75	.079
7.05	.068
7.20	.060
7.35	.058
7.50	.051
7.65	.049
7.80	.046
7.95	.042
8.10	.038
8.25	.037
8.55	.031
8.70	.028
8.85	.025
9.00	.023
9.15	.022
9.45	.019
9.60	.016
9.75	.015
9.90	.014
10.05	.014
10.20	.011
10.35	.011
10.50	.009
10.65	.009
10.80	.008
10.95	.008
11.10	.006
11.25	.006
11.40	.005
11.55	.005
11.85	.004
12.00	.004
12.15	.004
12.30	.003
12.45	.003
12.60	.002
12.75	.002
12.90	.002
13.05	.002
13.20	.002
13.35	.001
13.50	.001

Table A.15 (continued)

$k = 4, n = 8$

x	$P_0\{S \geq x\}$
13.65	.001
13.80	.001
13.95	.001
14.25	.001
14.40	.001
14.55	.001
14.70	.001
14.85	.000

$k = 5, n = 3$

x	$P_0\{S \geq x\}$
.000	1.000
.267	1.000
.533	.988
.800	.972
1.067	.941
1.333	.914
1.600	.845
1.867	.831
2.133	.768
2.400	.720
2.667	.682
2.933	.649
3.200	.595
3.467	.559
3.733	.493
4.000	.475
4.267	.432
4.533	.406
4.800	.347
5.067	.326
5.333	.291
5.600	.253
5.867	.236
6.133	.213
6.400	.172
6.667	.163
6.933	.127
7.200	.117
7.467	.096
7.733	.080

$k = 5, n = 3$

x	$P_0\{S \geq x\}$
8.000	.063
8.267	.056
8.533	.045
8.800	.038
9.067	.028
9.333	.026
9.600	.017
9.867	.015
10.133	.008
10.400	.005
10.667	.004
10.933	.003
11.467	.001
12.000	.000

$k = 5, n = 4$

x	$P_0\{S \geq x\}$
.0	1.000
.2	.999
.4	.991
.6	.980
.8	.959
1.0	.940
1.2	.906
1.4	.895
1.6	.850
1.8	.815
2.0	.785
2.2	.759
2.4	.715
2.6	.685
2.8	.630
3.0	.612
3.2	.579
3.4	.552
3.6	.500
3.8	.479
4.0	.442
4.2	.413
4.4	.395
4.6	.370

$k = 5, n = 4$

x	$P_0\{S \geq x\}$
4.8	.329
5.0	.317
5.2	.286
5.4	.275
5.6	.249
5.8	.227
6.0	.205
6.2	.197
6.4	.178
6.6	.161
6.8	.143
7.0	.136
7.2	.121
7.4	.113
7.6	.095
7.8	.086
8.0	.080
8.2	.072
8.4	.063
8.6	.060
8.8	.049
9.0	.043
9.2	.038
9.4	.035
9.6	.028
9.8	.025
10.0	.021
10.2	.019
10.4	.017
10.6	.014
10.8	.011
11.0	.010
11.2	.008
11.4	.007
11.6	.006
11.8	.005
12.0	.004
12.2	.004
12.4	.003
12.6	.002
12.8	.002
13.0	.001
13.2	.001
13.4	.001

$k = 5, n = 4$

x	$P_0\{S \geq x\}$
13.6	.001
13.8	.000

$k = 5, n = 5$

x	$P_0\{S \geq x\}$
.00	1.000
.16	1.000
.32	.994
.48	.986
.64	.972
.80	.958
.96	.932
1.12	.925
1.28	.891
1.44	.865
1.60	.842
1.76	.823
1.92	.789
2.08	.765
2.24	.721
2.40	.707
2.56	.679
2.72	.657
2.88	.613
3.04	.594
3.20	.562
3.36	.535
3.52	.518
3.68	.494
3.84	.454
4.00	.443
4.16	.410
4.32	.398
4.48	.371
4.64	.349
4.80	.325
4.96	.316
5.12	.295
5.28	.275
5.44	.255
5.60	.246

Table A.15 (continued)

$k = 5, n = 5$

x	$P_0\{S \geq x\}$
5.76	.227
5.92	.218
6.08	.195
6.24	.183
6.40	.174
6.56	.164
6.72	.151
6.88	.146
7.04	.130
7.20	.121
7.36	.112
7.52	.107
7.68	.094
7.84	.089
8.00	.082

$k = 5, n = 5$

x	$P_0\{S \geq x\}$
8.16	.077
8.32	.073
8.48	.066
8.64	.058
8.80	.056
8.96	.049
9.12	.046
9.28	.042
9.44	.038
9.60	.035
9.76	.032
9.92	.029
10.08	.026
10.24	.024
10.40	.022

$k = 5, n = 5$

x	$P_0\{S \geq x\}$
10.56	.019
10.72	.018
10.88	.015
11.04	.013
11.20	.012
11.36	.012
11.52	.010
11.68	.009
11.84	.008
12.00	.007
12.16	.006
12.32	.006
12.48	.005
12.64	.004
12.80	.004

$k = 5, n = 5$

x	$P_0\{S \geq x\}$
12.96	.003
13.12	.003
13.28	.003
13.44	.002
13.60	.002
13.76	.002
13.92	.002
14.08	.001
14.24	.001
14.40	.001
14.56	.001
14.72	.001
14.88	.001
15.04	.000

Computed by G. A. Mack on the Ohio State University IBM 370/165.

Table A.16. Selected critical values for the null distribution of Page's L statistic:
$k = 3, \quad n = 2(1)20;$
$k = 4(1)8, \quad n = 2(1)12$

For given k, n, and α, the tabled entry is $\ell(\alpha, k, n)$ satisfying $P_0\{L \geq \ell(\alpha, k, n)\} \approx \alpha$.

	k = 3			k = 4			k = 5		
	α			α			α		
n	.001	.01	.05	.001	.01	.05	.001	.01	.05
2			28		60	58	109	106	103
3		42	41	89	87	84	160	155	150
4	56	55	54	117	114	111	210	204	197
5	70	68	66	145	141	137	259	251	244
6	83	81	79	172	167	163	307	299	291
7	96	93	91	198	193	189	355	346	338
8	109	106	104	225	220	214	403	393	384
9	121	119	116	252	246	240	451	441	431
10	134	131	128	278	272	266	499	487	477
11	147	144	141	305	298	292	546	534	523
12	160	156	153	331	324	317	593	581	570
13	172	169	165						
14	185	181	178						
15	197	194	190						
16	210	206	202						
17	223	218	215						
18	235	231	227						
19	248	243	239						
20	260	256	251						

	k = 6			k = 7			k = 8		
	α			α			α		
n	.001	.01	.05	.001	.01	.05	.001	.01	.05
2	178	173	166	269	261	252	388	376	362
3	260	252	244	394	382	370	567	549	532
4	341	331	321	516	501	487	743	722	701
5	420	409	397	637	620	603	917	893	869
6	499	486	474	757	737	719	1,090	1,063	1,037
7	577	563	550	876	855	835	1,262	1,232	1,204
8	655	640	625	994	972	950	1,433	1,401	1,371
9	733	717	701	1,113	1,088	1,065	1,603	1,569	1,537
10	811	793	777	1,230	1,205	1,180	1,773	1,736	1,703
11	888	869	852	1,348	1,321	1,295	1,943	1,905	1,868
12	965	946	928	1,465	1,437	1,410	2,112	2,072	2,035

Adapted from E. B. Page, Ordered hypotheses for multiple treatments: a significance test for linear ranks, *J. Amer. Statist. Ass.* 58, 216-30 (1963), with the permission of the author, and the editor of the *Journal of the American Statistical Association.*

Table A.17. Selected critical values for all treatments multiple comparisons based on Friedman rank sums:
$k = 3, n = 3(1)15; \; k = 4(1)15, n = 2(1)15$

For a given k and n, the entries in the table correspond to $P_0\{|R_u - R_v| < r(\alpha, k, n),$ $u = 1, \ldots, k-1, v = u+1, \ldots, k\} \approx 1 - \alpha$.

k = 3		
n	$r(\alpha, 3, n)$	α
3	6*	.028
4	7*	.042
	8*	.005
5	8*	.039
	9*	.008
6	9*	.029
	10*	.009
7	9*	.051
	10	.023
	11*	.008
8	10*	.039
	11	.018
	12*	.007
9	10*	.048
	11	.026
	12*	.013
10	11*	.037
	12	.019
	13*	.010
11	11*	.049
	12	.028
	14*	.008
12	12*	.038
	13	.022
	14*	.012
13	12*	.049
	13	.030
	15*	.009

k = 3		
n	$r(\alpha, 3, n)$	α
14	13*	.038
	14	.023
	16*	.007
15	13*	.047
	14	.028
	16*	.010

k = 4		
n	$r(\alpha, 4, n)$	α
2	6*	.083
3	8*	.049
	9*	.007
4	10*	.026
	11*	.005
5	11*	.037
	12*	.013
6	12*	.037
	13	.018
	14*	.006
7	13*	.037
	14	.020
	15*	.008
8	14*	.034
	15	.019
	16*	.009
9	15	.032
	17*	.010

k = 4		
n	$r(\alpha, 4, n)$	α
10	15*	.046
	16	.029
	18*	.010
11	16	.041
	17	.026
	19	.009
12	17	.038
	18	.023
	20	.008
13	18	.032
	19	.021
	21	.008
14	18	.042
	19	.028
	21	.011
15	19	.037
	20	.024
	22	.010

k = 5		
n	$r(\alpha, 5, n)$	α
2	8	.050
3	10	.067
	11	.018
	12	.002
4	12	.054
	13	.020
	14	.006

Table A.17 (continued)

$k = 5$

n	$r(\alpha, 5, n)$	α
5	14	.040
	16	.006
6	15	.049
	16	.028
	17	.013
7	16	.052
	17	.033
	19	.009
8	18	.036
	19	.022
	20	.012
9	19	.037
	20	.024
	22	.008
10	20	.038
	21	.025
	23	.009
11	21	.038
	22	.025
	24	.010
12	22	.038
	23	.025
	25	.011
13	23	.035
	24	.024
	26	.011
14	24	.034
	25	.024
	27	.011
15	24	.045
	26	.022
	28	.010

$k = 6$

n	$r(\alpha, 6, n)$	α
2	10	.033
3	13	.030
	14	.008
4	15	.047
	16	.018
	17	.006
5	17	.047
	18	.022
	19	.010
6	19	.040
	20	.021
	21	.010
7	20	.049
	21	.032
	23	.010
8	22	.039
	23	.026
	25	.008
9	23	.043
	24	.030
	26	.012
10	24	.047
	26	.023
	28	.009
11	26	.036
	27	.026
	29	.012
12	27	.039
	28	.028
	31	.009

$k = 6$

n	$r(\alpha, 6, n)$	α
13	28	.039
	29	.028
	32	.010
14	29	.040
	30	.030
	33	.011
15	30	.040
	32	.023
	34	.012

$k = 7$

n	$r(\alpha, 7, n)$	α
2	12	.024
3	15	.048
	16	.016
4	18	.040
	20	.007
5	20	.052
	21	.028
	22	.014
6	22	.050
	23	.032
	25	.009
7	24	.047
	25	.032
	27	.011
8	26	.041
	27	.030
	29	.011

Table A.17 (continued)

$k = 7$

n	$r(\alpha, 7, n)$	α
9	27	.050
	29	.026
	31	.011
10	29	.042
	30	.031
	33	.010
11	30	.049
	32	.027
	35	.009
12	32	.040
	33	.030
	36	.011
13	33	.043
	35	.025
	38	.009
14	34	.047
	36	.028
	39	.011
15	36	.038
	37	.030
	41	.009

$k = 8$

n	$r(\alpha, 8, n)$	α
2	14	.018
3	17	.067
	18	.027
	19	.009
4	21	.036
	23	.007

$k = 8$

n	$r(\alpha, 8, n)$	α
5	23	.057
	24	.034
	26	.009
6	26	.045
	27	.027
	29	.009
7	28	.048
	29	.032
	31	.012
8	30	.046
	31	.033
	34	.009
9	32	.043
	33	.032
	36	.010
10	34	.040
	35	.031
	38	.010
11	35	.048
	37	.028
	40	.010
12	37	.042
	39	.026
	42	.010
13	39	.039
	40	.030
	44	.009
14	40	.042
	42	.027
	45	.012
15	42	.037
	43	.030
	47	.011

$k = 9$

n	$r(\alpha, 9, n)$	α
2	15	.069
	16	.014
3	20	.041
	22	.005
4	23	.064
	24	.034
	26	.008
5	27	.040
	28	.023
	29	.013
6	29	.058
	30	.038
	33	.008
7	32	.046
	33	.032
	36	.008
8	34	.049
	36	.026
	38	.012
9	36	.050
	38	.030
	41	.010
10	38	.050
	40	.031
	43	.011
11	40	.048
	42	.030
	46	.009
12	42	.046
	44	.029
	48	.009

Table A. 17 (continued)

$k = 9$

n	$r(\alpha, 9, n)$	α
13	44	.042
	46	.027
	50	.009
14	46	.041
	48	.026
	52	.009
15	47	.046
	50	.025
	54	.009

$k = 10$

n	$r(\alpha, 10, n)$	α
2	17	.056
	18	.011
3	22	.057
	23	.026
	24	.010
4	26	.060
	27	.033
	29	.009
5	30	.047
	31	.029
	33	.010
6	33	.051
	34	.033
	37	.008
7	36	.047
	37	.033
	40	.010
8	38	.052
	40	.031
	43	.010

$k = 10$

n	$r(\alpha, 10, n)$	α
9	41	.046
	43	.027
	46	.009
10	43	.047
	45	.030
	49	.009
11	45	.049
	47	.032
	51	.010
12	48	.040
	50	.027
	54	.009
13	50	.039
	52	.026
	56	.009
14	52	.039
	54	.026
	58	.010
15	53	.045
	56	.026
	60	.010

$k = 11$

n	$r(\alpha, 11, n)$	α
2	19	.045
	20	.009
3	25	.038
	27	.007
4	29	.057
	30	.033
	32	.010

$k = 11$

n	$r(\alpha, 11, n)$	α
5	33	.055
	34	.035
	37	.008
6	37	.045
	38	.030
	41	.008
7	40	.049
	41	.035
	44	.011
8	43	.046
	44	.035
	48	.009
9	46	.043
	47	.034
	51	.009
10	48	.047
	50	.031
	54	.009
11	51	.040
	53	.027
	57	.009
12	53	.043
	55	.029
	59	.011
13	55	.046
	57	.031
	62	.010
14	57	.045
	60	.026
	64	.011
15	59	.046
	62	.027
	67	.009

Table A.17 (continued)

$k = 12$

n	$r(\alpha, 12, n)$	α
2	21	.038
	22	.008
3	27	.053
	28	.027
	29	.012
4	32	.055
	33	.033
	35	.011
5	37	.042
	38	.027
	40	.011
6	40	.059
	42	.028
	45	.008
7	44	.050
	46	.026
	49	.009
8	47	.050
	49	.030
	52	.011
9	50	.048
	52	.032
	56	.010
10	53	.047
	55	.032
	59	.010
11	56	.043
	58	.029
	62	.011
12	58	.048
	61	.027
	65	.011

$k = 12$

n	$r(\alpha, 12, n)$	α
13	61	.043
	63	.030
	68	.010
14	63	.046
	66	.027
	71	.009
15	66	.040
	68	.028
	73	.011

$k = 13$

n	$r(\alpha, 13, n)$	α
2	23	.032
	24	.006
3	30	.038
	32	.009
4	35	.054
	36	.033
	38	.012
5	40	.049
	41	.033
	44	.009
6	44	.054
	46	.027
	49	.009
7	48	.051
	50	.028
	53	.010
8	52	.046
	53	.035
	57	.010

$k = 13$

n	$r(\alpha, 13, n)$	α
9	55	.048
	57	.030
	61	.010
10	58	.047
	60	.032
	65	.009
11	61	.046
	63	.032
	68	.010
12	64	.045
	66	.032
	71	.010
13	67	.041
	69	.030
	74	.011
14	69	.046
	72	.028
	77	.010
15	72	.040
	74	.030
	80	.010

$k = 14$

n	$r(\alpha, 14, n)$	α
2	25	.027
	26	.005
3	32	.052
	33	.028
	35	.006
4	38	.053
	39	.034
	41	.013

Table A.17 (continued)

$k = 14$

n	$r(\alpha, 14, n)$	α
5	43	.057
	45	.027
	47	.012
6	48	.050
	50	.026
	53	.009
7	52	.053
	54	.030
	57	.012
8	56	.051
	58	.031
	62	.010
9	60	.047
	62	.029
	66	.010
10	63	.048
	65	.033
	70	.010
11	66	.049
	69	.029
	74	.009
12	69	.048
	72	.030
	77	.010
13	72	.047
	75	.030
	80	.011

$k = 14$

n	$r(\alpha, 14, n)$	α
14	75	.045
	78	.028
	84	.009
15	78	.043
	81	.028
	87	.010

$k = 15$

n	$r(\alpha, 15, n)$	α
2	26	.071
	27	.024
	28	.005
3	35	.039
	37	.010
4	41	.053
	42	.035
	45	.008
5	47	.046
	48	.033
	51	.010
6	52	.047
	53	.035
	57	.009
7	56	.055
	58	.032
	62	.010

$k = 15$

n	$r(\alpha, 15, n)$	α
8	60	.056
	63	.027
	67	.009
9	64	.052
	67	.028
	71	.011
10	68	.049
	71	.028
	75	.011
11	72	.043
	74	.032
	79	.011
12	75	.045
	78	.028
	83	.010
13	78	.046
	81	.030
	87	.009
14	81	.046
	84	.030
	90	.010
15	84	.043
	87	.029
	94	.009

Adapted in part from B. J. McDonald and W. A. Thompson, Jr., Rank sum multiple comparisons in one- and two-way classifications, *Biometrika* 54, 487-97 (1967), with the permission of the authors, and the editor of *Biometrika*. The starred values are adapted from P. Nemenyi, Distribution-free multiple comparisons, Ph. D. thesis, Princeton University, (1963), with the permission of the author.

Table A.18. Critical values for one-sided treatments versus control multiple comparisons based on Friedman rank sums: $k = 3, n = 2(1)18; k = 4, n = 2(1)5$

For given n and k, the tabled entry for x is $P_0\{(R_u - R_1) < x, u = 2, \ldots, k\}$. Thus if x is such that $P_0\{(R_u - R_1) < x, u = 2, \ldots, k\} = 1 - \alpha$, then $r^*(\alpha, k-1, n) = x$. For given n and k, the entries are terminated at $x_{k,n}$, where $x_{k,n}$ is the smallest value of x for which $P_0\{(R_u - R_1) < x, u = 2, \ldots, k\} = 1$, to four decimal places.

$k = 3$

x	$n = 2$	$n = 3$	$n = 4$	$n = 5$	$n = 6$	$n = 7$	$n = 8$	$n = 9$	$n = 10$
1	.5000	.3981	.4213	.3976	.3983	.3903	.3879	.3840	.3816
2	.6111	.5648	.5386	.5185	.5037	.4918	.4821	.4739	.4670
3	.7778	.7593	.6899	.6664	.6349	.6161	.5976	.5834	.5706
4	.9444	.8519	.8194	.7719	.7453	.7173	.6967	.6776	.6617
5		.9352	.8982	.8594	.8288	.8017	.7784	.7580	.7400
6		.9907	.9537	.9303	.9008	.8777	.8546	.8348	.8161
7			.9861	.9663	.9476	.9272	.9086	.8904	.8735
8			.9985	.9869	.9749	.9602	.9456	.9308	.9163
9				.9972	.9899	.9817	.9714	.9608	.9496
10				.9997	.9969	.9922	.9861	.9788	.9708
11					.9994	.9972	.9939	.9894	.9841
12					1.0000	.9993	.9977	.9954	.9922
13						.9999	.9993	.9982	.9964
14						1.0000	.9998	.9994	.9985
15							1.0000	.9998	.9995
16								1.0000	.9998
17									1.0000

$k = 3$

x	$n = 11$	$n = 12$	$n = 13$	$n = 14$	$n = 15$	$n = 16$	$n = 17$	$n = 18$
1	.3791	.3771	.3753	.3737	.3723	.3710	.3698	.3687
2	.4610	.4557	.4590	.4468	.4431	.4396	.4365	.4336
3	.5598	.5502	.5417	.5341	.5272	.5210	.5154	.5101
4	.6474	.6349	.6236	.6135	.6043	.5960	.5883	.5812
5	.7239	.7095	.6965	.6847	.6739	.6640	.6549	.6464
6	.7995	.7840	.7840	.7570	.7451	.7340	.7237	.7141
7	.8576	.8429	.8290	.8160	.8039	.7926	.7819	.7718
8	.9023	.8889	.8761	.8639	.8523	.8413	.8308	.8208
9	.9383	.9271	.9161	.9054	.8950	.8849	.8752	.8658
10	.9623	.9536	.9446	.9357	.9268	.9180	.9094	.9010
11	.9781	.9716	.9647	.9576	.9504	.9431	.9358	.9285
12	.9884	.9840	.9791	.9739	.9683	.9626	.9568	.9508
13	.9942	.9913	.9880	.9844	.9803	.9760	.9715	[illegible]

Table A.18 (continued)

$k = 3$

x	$n = 11$	$n = 12$	$n = 13$	$n = 14$	$n = 15$	$n = 16$	$n = 17$	$n = 18$
14	.9973	.9956	.9935	.9910	.9882	.9851	.9817	.9782
15	.9989	.9979	.9967	.9952	.9934	.9913	.9889	.9863
16	.9996	.9991	.9984	.9975	.9965	.9950	.9935	.9917
17	.9999	.9997	.9993	.9988	.9981	.9973	.9963	.9951
18	1.0000	.9999	.9997	.9995	.9991	.9986	.9980	.9972
19		1.0000	.9999	.9998	.9996	.9993	.9990	.9985
20			1.0000	.9999	.9998	.9997	.9995	.9992
21				1.0000	.9999	.9999	.9998	.9996
22					1.0000	.9999	.9999	.9998
23						1.0000	1.0000	.9999
24								1.0000

$k = 4$

x	$n = 2$	$n = 3$	$n = 4$	$n = 5$
1	.3542	.3029	.3033	.2960
2	.4792	.4358	.4121	.3957
3	.6354	.5733	.5325	.5019
4	.7708	.7105	.6481	.6095
5	.9062	.8121	.7563	.7091
6	.9792	.8954	.8434	.7960
7		.9540	.9082	.8666
8		.9878	.9519	.9196
9		.9983	.9795	.9554
10			.9935	.9778
11			.9987	.9905
12			.9999	.9968
13				.9992
14				.9999
15				1.0000

Computed by B. M. Windham on the Florida State University CDC 6400.

Table A.19. Critical values for two-sided treatments versus control multiple comparisons based on Friedman rank sums: $k = 3, n = 2(1)18; k = 4, n = 2(1)5$

For given n and k, the tabled entry for x is $P_0\{|R_u - R_1| < x, u = 2, \ldots, k\}$. Thus if x is such that $P_0\{|R_u - R_1| < x, u = 2, \ldots, k\} = 1 - \alpha$, then $r^{**}(\alpha, k-1, n) = x$. For given n and k, the entries are terminated at $x_{k,n}$, where $x_{k,n}$ is the smallest value of x for which $P_0\{|R_u - R_1| < x, u = 2, \ldots, k\} = 1$, to four decimal places.

$k = 3$

x	$n = 2$	$n = 3$	$n = 4$	$n = 5$	$n = 6$	$n = 7$	$n = 8$	$n = 9$	$n = 10$
1	.1667	.0556	.0695	.0463	.0437	.0360	.0326	.0288	.0263
2	.2778	.1944	.1620	.1337	.1157	.1013	.0903	.0814	.0741
3	.5556	.5278	.3997	.3601	.3067	.2763	.2469	.2253	.2061
4	.8889	.7037	.6404	.5491	.4992	.4475	.4104	.3765	.3489
5		.8704	.7963	.7189	.6589	.6058	.5612	.5222	.4883
6		.9815	.9074	.8606	.8017	.7556	.7099	.6708	.6343
7			.9722	.9326	.8952	.8543	.8173	.7809	.7475
8			.9969	.9738	.9498	.9205	.8912	.8615	.8327
9				.9943	.9799	.9634	.9428	.9216	.8991
10				.9995	.9937	.9844	.9722	.9576	.9417
11					.9989	.9944	.9878	.9789	.9682
12					.9999	.9986	.9955	.9908	.9844
13						.9998	.9986	.9963	.9929
14						1.0000	.9997	.9987	.9971
15							1.0000	.9997	.9989
16								.9999	.9997
17								1.0000	.9999
18									1.0000

$k = 3$

x	$n = 11$	$n = 12$	$n = 13$	$n = 14$	$n = 15$	$n = 16$	$n = 17$	$n = 18$
1	.0239	.0220	.0204	.0190	.0178	.0167	.0157	.0149
2	.0680	.0628	.0584	.0545	.0512	.0482	.0455	.0432
3	.1904	.1768	.1650	.1547	.1456	.1375	.1303	.1238
4	.3245	.3036	.2851	.2688	.2542	.2411	.2293	.2186
5	.4584	.4320	.4084	.3873	.3682	.3509	.3351	.3207
6	.6019	.5722	.5454	.5207	.4982	.4775	.4584	.4408
7	.7160	.6868	.6596	.6342	.6106	.5886	.5680	.5488
8	.8048	.7780	.7526	.7284	.7054	.6837	.6630	.6435
9	.8767	.8543	.8324	.8110	.7902	.7701	.7508	.7321
10	.9246	.9071	.8893	.8714	.8537	.8362	.8190	.8022
11	.9562	.9432	.9295	.9153	.9008	.8862	.8716	.8570
12	.9768	.9679	.9582	.9477	.9367	.9252	.9135	.9016

Table A.19 (continued)

$k = 3$

x	$n = 11$	$n = 12$	$n = 13$	$n = 14$	$n = 15$	$n = 16$	$n = 17$	$n = 18$
13	.9883	.9827	.9761	.9687	.9607	.9521	.9430	.9336
14	.9945	.9912	.9870	.9821	.9764	.9702	.9635	.9563
15	.9977	.9959	.9934	.9904	.9867	.9825	.9778	.9727
16	.9991	.9982	.9969	.9951	.9928	.9901	.9869	.9833
17	.9997	.9993	.9987	.9976	.9963	.9946	.9926	.9901
18	.9999	.9998	.9994	.9989	.9982	.9972	.9960	.9945
19	1.0000	.9999	.9998	.9996	.9992	.9986	.9979	.9970
20		1.0000	.9999	.9998	.9997	.9994	.9990	.9984
21			1.0000	.9999	.9999	.9997	.9995	.9992
22				1.0000	1.0000	.9999	.9998	.9996
23						1.0000	.9999	.9998
24							1.0000	.9999
25								1.0000

$k = 4$

x	$n = 2$	$n = 3$	$n = 4$	$n = 5$
1	.0417	.0000	.0076	.0000
2	.1042	.0417	.0397	.0254
3	.2917	.2023	.1477	.1153
4	.5417	.4288	.3182	.2578
5	.8125	.6241	.5155	.4277
6	.9583	.7908	.6870	.5932
7	-	.9080	.8165	.7332
8		.9757	.9039	.8392
9		.9965	.9590	.9107
10			.9871	.9556
11			.9974	.9811
12			.9997	.9936
13				.9984
14				.9997
15				1.0000

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