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A BEHAVIORAL SYSTEMS ANALYSIS OF JOB  
PERFORMANCE IN A FOOD CO-OPERATIVE

by

Harry M. Kent

A Dissertation  
Submitted to the  
Faculty of The Graduate College  
in partial fulfillment  
of the  
Degree of Doctor of Philosophy

Western Michigan University  
Kalamazoo, Michigan  
December 1977

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Harry M. Kent

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## A BRIEF HISTORY OF CONSUMERS CO-OPERATIVES

The setting for the present experiment was a "new" food co-operative (one that emphasizes voluntary work and natural foods). This brief history and what follows should properly distinguish this setting from more traditional consumer co-operatives and from producer co-operatives, while giving some insight into the population with which this investigation deals.

Co-operatives of various sorts have existed throughout recorded history. "Primitive communist" societies, such as those comprising hunters and gatherers, were co-operatives of both producers and consumers. As the forces of production developed (mainly with the domestication of plants and animals), and the first surpluses amassed, the "market" which separated production and consumption came into being (Engels, 1898).

Producers traditionally organized into mutually beneficial arrangements prior to and more effectively than consumers. Guilds in the Middle Ages and later handicraft associations formed to secure raw materials and tools at lower cost, to control the supply and training of craftspeople, and to market the finished goods (Foner, 1947). As trusts emerged in the industrial revolution, co-operatives of small producers and of workers-turned-producers formed in self-defense, but often to no avail. Lack of capital and expertise contributed to a general inability to withstand the tactics of the trusts (Boyer and Morais, 1955).

The rise of formal consumer co-operation paralleled the rise of industrial monopoly. As workers and small producers tried and failed to make goods under their own control, they attempted to eliminate the middlemen and privately owned retail operations. If they could purchase at wholesale rates, and control as much of the process from the warehouse to the buyer as possible; then perhaps prices would be lower and consumers would have more say about the quality of items. Consumer co-operatives organized in Rochdale, England as early as 1844 (Alanne, 1935; Cole, 1944), and became popular in North America under the leadership of the Rochdale Pioneers at the turn of the century (Voorhis, 1961; Roy, 1964). Early unsuccessful attempts to undersell private enterprise led the Rochdale Pioneers to adopt as a principle that goods be sold at the market price. But they differed from privately controlled distributing and retailing operations in that members had an equal say in management. Anyone could join a Rochdale co-op for minimal fees, each had one vote regardless of shares purchased, and returns on investments to individuals were to be deemphasized. Their plan included the peaceful conversion of the economy into one big co-operative (Roy, 1964; Phillipson, 1974).

The depression and labor organization in the 1930's led to the formation of many producer and consumers co-operatives. Most disappeared with the New Deal as, again, the co-ops were weakened by legal and illegal tactics of the trusts, and as the voluntary alliances of unemployed workers and farmers dismantled with the

recovery of the war-bent economy (Boyer and Morais, 1955; Baran and Sweezy, 1966; Giese, 1976). However, a few Rochdale type co-ops have survived, and are typified by the Berkeley, California and Hyde Park, Illinois systems. Customer-members pay the same prices as in supermarkets for almost the same quality of items, and receive rebates proportional to their purchasing at the year's end. A very small percentage of members attends annual meetings to elect a board which appoints management. The Rochdale co-ops repeatedly assert in their literature that they are part of the "free enterprise" system; not opposed to it (Phillipson, 1974; Giese, 1976).

Co-operatives, as we know them, originated among small producers and consumer groups in response to the formation of trusts. Although highly political at first, these co-ops lost practically all semblance of their radical past.

#### The Ethic of Voluntarism in Consumer Co-operatives

In the beginning, part of the philosophy of co-ops was opposition to pay for labor. But the early utopians failed to establish such voluntary co-operatives. However, during the depression, groups of unemployed started co-ops which paid labor little or nothing, yet these later groups did so only out of necessity, not out of philosophy. Only the "new co-ops" of the late 1960s achieved this voluntary "ideal" under conditions of no clear deprivation.

Many who started co-operatives in the late 19th century were also involved in or sympathetic to utopian communities and other models of an alternative society. These utopians would say, let's build a communistic society in which wages are unnecessary. They pointed to several successful utopian communities as examples of what might be possible on a larger scale (Foner, 1947).

However, the co-operatives that succeeded differed considerably from the communistic societies. a) They were not relatively self-contained economic units which could ration out all the members' needs. b) They paid workers and, in fact, strove to pay fair wages. The Rochdale co-operatives were never in favor of voluntarism in work, and today insist on paying at least union-scale wages (Phillipson, 1974; Scharrs, 1971). Paying wages seemed to be necessary for the survival of the late 19th century co-operatives.

The Great Depression altered the relevant variables. In the co-operative boom of the 1930's much of the work was voluntary. A virtually moneyless economy existed among the unemployed as, instead of paying rent, they formed Unemployed Councils to resist eviction, and instead of paying for the distribution and retailing of food, they formed consumer co-operatives. As noted previously, these voluntary associations died out when the economy prospered (Boyer and Morais, 1955).

At the same time as the anti-war movement of the late 1960's came a rebirth of voluntary consumer co-operatives. A different class of people volunteered, and for different reasons. Students

and the wilfully unemployed or underemployed; the sons and daughters of the new middle class, formed the vanguard of a new co-op movement. Dissatisfied with the "establishment", these new utopians set out to share the work equally and to earn nothing but the direct product of their labor. They spurned wages and all but the most obvious aspects of management and accounting (Danforth, 1974).

Besides favoring voluntarism, the new co-ops were different in other ways. Rachel Carson (1962) and Ralph Nader (cited in Esposito, 1970) combined to popularize consumerism as a defense against the growing threat of pollution. Along with the reaction against the "military-industrial complex" came the reaction against the processed-food-chemical-farming agribusiness complex (DeBell, 1970; Lerza and Jacobsen, 1975). So most of the new co-ops were "natural food" co-ops. Voluntary consumer co-operatives with regular supermarket food also became a part of the "war on poverty." But these later attempts usually met with failure when the organizer left or other subsidies were removed (Kahn, 1970).

Today, increasing unemployment and inflation are again leading to the formation of voluntary co-ops. There are now in existence several thousand small buying clubs (which take preorders and have no significant inventory) and storefront co-ops, most of which are voluntary and emphasize natural foods. The movement is nationwide and growing (Ronco, 1974; Vallela, 1975). While still not contributing much to total retail sales, the new voluntary co-ops are significant because one may be found in practically every large city and university town.

## A BEHAVIORAL ANALYSIS OF VOLUNTARISM IN CO-OPERATIVES

Interestingly, the term "volunteer" is quite elusive if we assume in our behavioral analysis that behavior is controlled by identifiable consequences. Certain material consequences, such as pay, are more readily identifiable than others, and it seems that they are rejected more quickly by those who value voluntarism. The behavior analyst's task is to explicate the less obvious consequences.

To those who value voluntarism, more subtle contingencies such as intrinsic consequences and unsystematic praise have come to mean that a person under such control has made a greater contribution and thus has more inner worth. Doing good for the good of others is more highly honored than doing good for some more coercive reason. This is necessarily the case when contingencies have never been analyzed; when they are said to be ineffable. (Skinner, 1974).

Several utopians argued that when a community was properly structured, people worked because they wanted to and not for pay (Nordhoff, 1966). Clearly, however, the communitard received the things which money purchased in the outside world. And money proved to be a necessary consequence for productive labor in all the utopians' attempts to foster co-operatives in the economy at large.

So, the first co-ops to survive for any period of time on a voluntary basis were the depression era co-ops. While these co-ops did not necessarily promote voluntarism in general, they promoted it as a temporary expedient. Much like the communistic



societies, these co-ops rationed goods and services to those who worked. A barter or labor exchange economy temporarily replaced a money economy to a large degree, but in no way did it do away with other extrinsic consequences for labor. For instance, deprivation may have added considerably to the reinforcing power of contingent food. Also, social factors undoubtedly played a large role in the 1930's co-ops. Social punishment may have been generated during crises such as the Great Depression for those who did not contribute to the general good of the group. Such extrinsic punishment may be effective in settings where escape is not a high probability response. For example, social punishment in the form of criticism, ostracism and threat of expulsion was used extensively in the 19th century communities (Nordhoff, 1966).

So it seems plausible that a volunteer in a co-op was one who worked for a different kind of immediate consequence--not someone who worked "on their own accord" or without consequences. This statement seems equally as plausible in the analysis of the volunteers in the new co-op movement, although things have gotten more complex as affluence, the welfare state, and universal and higher education have greatly altered the contingencies affecting work (as will be discussed in a behavioral analysis of voluntarism, a "volunteer" in the new co-op movement may have received some material rewards for work).

In the past, the new co-op workers were not paid. In addition they did not receive all of the necessities of life as a direct consequence of work. There was no major crisis in the late 1960's to compel

work to avoid starvation or social aversives threatened by desperate people. However, there were possibly several sources of extrinsic consequences for the work that went into these new co-ops of the 1960's. First of all, for those in particular "counter cultures," social aversives for collaborating with agribusiness and with corporations in general could be avoided by working and buying in a food co-operative. Buying unprocessed and organically grown food in the co-op was probably not maintained by the differential reinforcing consequences of preparing or consuming the food. Consequences in terms of better health in the long run, if existent, were too remote to directly affect purchasing. Probably the social consequences for buying natural foods and shopping at an alternative business included avoiding criticism by counter-culture members for buying the "wrong" things at the "wrong" places.

Secondly, this higher quality food was often cheaper and sometimes free for those who worked in the new co-ops. Often, only those who worked were allowed to purchase. Again, co-op workers as a rule did not have to work in the co-op to make ends meet--there were other sources of income for this special population, such as parents, spouses, friends, savings, welfare, and other jobs. But cheaper food meant more money which could be devoted to other things, or less work elsewhere to earn the desired amount of money. I made these and other unreferenced and non-experimental observations of new co-ops from 1970 to 1977.

From 1970 to 1973 I worked closely with the Credit Union League of the U.S.A., and with the Federation of Southern Cooperatives. From 1973 to 1977 I worked in one co-op and was a consultant for over one hundred more (all members of the Michigan Federation of Food Co-ops).

Perhaps food co-ops for poor people did not generate voluntary work because, among other things, these people generally did not have the history required to come under the control of and/or generate rules that would prevent purchasing "wrong" commodities ("wrong" for complex political or nutritional reasons). Also, the prices for regular food were not much lower in these co-ops with their typically small scale operations. According to Danforth (1974) agribusiness controls the regular food industry from the bottom up, and only those co-ops which vertically integrate, such as the Rochdale types, have been able to compete with the same line of products.

The number of hours worked per day and the number of weeks worked by the average individual in the new volunteer food co-ops was never substantial. The idea was to involve everyone and overwork no one. Frequently, however, one person ended up co-ordinating the operation and administering most of the punitive social consequences upon delinquent members. One of the major payoffs for the co-ordinator may have been the praise and censure of the active members. In addition, co-ordinators may have been under more rule-governed social control for doing the "right" thing. The operation would

cease when a co-ordinator would get "burned out," if someone could not be found with a history of reinforcement sufficient to make them "aware of the need to save the co-op." The "burn out" rate would increase when a co-op started to do a large volume of business and the job of co-ordinator and worker became more time consuming and/or more difficult in other ways.

As a result of this conflict between not wanting to pay people but also wanting to do a significant amount of business, many co-op people devised systems involving food credit pay, or pay for transportation and overhead costs but not labor, or paying some people but not others, or paying very low wages plus food credit and discounts, and so forth.

#### A Radical Behavioristic Analysis of Voluntarism

What are the essential features of the contingency relationship that evokes the term "volunteer"? They involve someone working, and not receiving any obvious material reward. The most obvious material reward to most in our society is money. Saying someone is a volunteer who works for very little money is a generalization, but one which the community will likely reinforce to some degree (Skinner, 1957). It is clear, however, that the absence of money is the most crucial element of the relation between work and its consequences that evokes the response "volunteer". Usually, money is the reward to most avoid to stay within the counterculture rules and mitigate criticism for being too

"capitalistic" or "materialistic" (terms for the use of obvious rewards).

People also generalize when they call a person a volunteer who receives free food and other non-monetary, material rewards, but the community is likely to reinforce that extension. Food made contingent upon work is a reward, though not so obvious a reward to the counterculture community as money. However, others may criticize a setting for not being voluntary enough if it uses too many non-monetary, material rewards, or if it makes the rewards (material or not) more obvious by making them explicitly contingent upon something more specific than just being present. For example, if a co-op gives its workers non-monetary but material rewards such as discounts on food, free "leftovers", free health insurance, a car to use with free gas and repairs, and free rent, it is likely to reduce its "voluntary" stature. "People frequently state that the dispensing of concrete (or "tangible") reinforcers is tantamount to giving bribes." (O'Leary, Poules, Devine, 1974). "Non-material" rewards include social reinforcement and feedback. As with most of the proponents of the literature of freedom and dignity, new-co-op people say that "thank-yous" and "knowledge of results" are fine, as long as they are not programmed for some ulterior purpose--like improved job performance. "The difference between conspicuous and inconspicuous control has led to many misunderstandings.... Contingencies designed for explicit purposes can be called manipulative...." (Skinner, 1974).

Each setting that opposed monetary rewards and promoted voluntarism (the 19th century communities, the 1930's co-ops, and the new co-ops) used social punishment to maintain performance. The stated goal of each community or co-op was to get people to work "because they wanted to," not for external reward. The incentive to work was said to have been "internalized", and the private events were often given an independent status as "moral incentives," or a "will to serve." In other words, those who promoted voluntarism would accept certain forms of aversive control (in addition to the less obvious forms of positive control previously discussed) as being consistent with their philosophy.

A causal role is given to internal events because their connection to external variables is not easily observed. But an analysis based on the philosophical position called radical behaviorism would point to a history of punishment as the likely source of covert responding that produces stimuli that in turn mediate external consequences (Malott and Whaley, 1976). Such a theoretical analysis might suggest the following: A worker who was off-task may have been criticized by a fellow commundard. The worker may emit that same criticism privately as an imitative response, due to a long history of reinforcement for imitation. Emitting the private response is reinforced by the subsequent overt avoidance of loss of money or the social criticism that typically follows detected off-task behavior in that setting. The

worker may then engage in the same or similar punishable behavior on some future occasion, and say privately, "that's lazy; that's bad." The private, response-produced stimuli may then serve as conditioned "self-punishers" of the ongoing overt behavior.

Much continued external support for this type of "self-control," or "guilt control," is necessary because the self-punishment act punishes itself (Malott and Whaley, 1976). Nor can this type of "control" be relied upon in any given instance. After being punished, a person may resolve not to do the punished act. Recalling the resolution at a later date is a gesture of self-management, though possibly ineffective. A resolution is a kind of self made rule, designed to extend the effect of punishment into the future, but on a later occasion immediate reinforcement for engaging in the punished act may still take over (Skinner, 1974).

In summary, it would seem that social consequences were critical factors, if not the only factors, in voluntary settings. Interestingly, the non-generalized or pure term "volunteer" is quite elusive if we assume in our behavioral analysis that behavior is controlled by identifiable consequences. And yet the survival of a new co-op could well depend on its ability to remain "voluntary" according to its active members and workers.

## APPLIED BEHAVIOR ANALYSIS IN VOLUNTARY SETTINGS

"Behavioral community psychology" has come to denote applications of behavior analysis to socially significant problems in unstructured community settings where the behavior of individuals is not considered deviant in the traditional sense (Briscoe, Hoffman and Bailey, 1975). In each of the cases that follow, extrinsic reinforcers have been successfully added in settings which have typically relied on voluntary participation; that is, settings that did not use obvious material rewards for the particular behavior under investigation.

Several studies have added small monetary rewards contingent upon appropriate behavior (Burgess, Clark and Hendee, 1971; Chapman and Risley, 1974; Hayes, Johnson and Cone, 1975; Powers, Osborne and Anderson, 1973; Meyers, Artz and Craighead, 1976; Jones and Azrin, 1973; Reiss, Piotrowski and Bailey, 1976). Other studies have included variations of monetary payment which place payment on a ratio schedule as in a raffle (Witmer and Geller, 1976), allow for savings of money rather than payments as in rent reductions (Feallock and Miller, 1976) and electric rate reductions (Kohlenberg, Phillips and Proctor, 1976), or they use other conditioned generalized reinforcers such as in a token or point system (Everett, Hayward and Meyers, 1974; Kohlenberg and Phillips, 1973; Miller and Miller, 1970; Fixsen, Phillips, and Wolf, 1973; Pierce and Risley, 1974; Witmer and Geller, 1976). Another



study used a selection of small rewards without a mediating exchange system (Clark, Burgess and Hendee, 1972).

Each of the above procedures made their respective settings less "voluntary," particularly those that used money. However, many studies used more subtle consequences that would not likely change the voluntary status of the setting. These include several with unsuccessful applications of written feedback (where social consequences are minimized) (Kohlenberg, Phillips and Proctor, 1976; Meyers, Artz and Craighead, 1976; Seaver and Patterson, 1976). Social reinforcement combined with modelling in training situations was successful (Kifer, Lewis and Green, 1974; Briscoe, Hoffman and Bailey, 1975), and social commendation was effective in the form of a decal received from a power company but not necessarily displayed to the public (Seaver and Patterson, 1976). These types of social consequences, which can be crucial in voluntary settings, were not contrasted with any of the more obvious material forms of reward--only with written feedback (Seaver and Patterson, 1976). Very few of the studies in behavioral community psychology have contrasted obvious material rewards with each other--only with written feedback (Kohlenberg et al., 1976; Meyers et al., 1976). One study (Witmer and Geller, 1976) purported to compare the effects of prompts and two types of material reinforcement (raffle and contest), and another contrasted the effects of activities versus gifts (Bunck and Iwata, in press).

It is important to note again that social consequences were

not frequently used with these behaviors. Nor were social consequences contrasted with obvious material rewards. Systematically delivered social reinforcement and punishment may have been more cost-effective in some settings where other rewards such as tokens were used. But no contrasts were made. It is of interest that Feallock and Miller (1976) in the group living study discussed the relatively small reversal in task completion when credits were still posted in terms of the effects of unspecified social consequences. Perhaps they should have systematized the delivery of social consequences, rather than having relied on the rent-reductions.

The preceding brief literature review helps to reveal the novelty of the present study. The present study sought to evaluate the effects of adding various conditioned reinforcers contingent upon satisfactory levels of job performance in a new voluntary food co-operative. Programmed social consequences were used, as well as a small monetary bonus. This study differed from those in other volunteer settings in the standard Applied Behavior Analysis literature in that it contrasted social consequences with a treatment package that included obvious material rewards in addition to the social consequences; and by studying these consequences in a setting which traditionally opposed the use of such obvious rewards.

#### Social Importance

•

Studies in applied behavior analysis do not as a rule add

anything to our theoretical knowledge of the basic principles of behavior; they are experimental-engineering studies which attempt to demonstrate that a particular procedure was effective with a given population and setting and behavior. Often the demonstration is for the field itself--and it is for others who may not be convinced that contingency management is the thing to do, but they may be inclined to engage in the behavior being managed, e.g., people who want to successfully participate in group living.

Practitioners of applied behavior analysis have frequently promoted the dissemination to diverse audiences of their ideology, methods, and programs. Behavior analysts have also stated that their research should be "important to man and to society" (Baer, Wolf and Risley, 1968). Perhaps a good indicator of whether research will be disseminated and whether it is deemed "important" is whether or not the problem with which it deals is recognized as a problem by significant others.

Because group living is a setting which attracts many of the same type of people who joined communistic societies and the new voluntary co-ops--that is, people whose histories lead them to criticize monetary or material reward systems, it would be useful to see if a method that meets their approval is more or less successful than one of which they initially may not approve, e.g., rent-reductions. Furthermore, a comparison of various consequences, including monetary and social consequences, could be of interest to the politically active people of the socialist (Marxist-Leninist)

countries--countries that comprise 40% of the world's population. These people vigorously oppose the use of monetary and other material rewards. Such a comparison done in a production setting that involves a substantial amount of behavior (such as the co-op) should be particularly interesting to this significant audience.

While appealing to what society or a certain part of society recognizes as a problem may aid in dissemination in the short run, we are not necessarily validating the "social importance" of the problem, as others would have us believe (Wolf, 1976). Rather, we must investigate the extent to which our target behaviors (in this case job performance) contribute to the survival of the culture (Skinner, 1971). We may do this by assessing such possible consequences as depletion of non-renewable resources, pollution, and failure to maintain performance in the long run. Directly or indirectly contingencies for job performance influence all of these long range consequences. Studying such contingencies in the co-op may not enhance survival of our culture. The co-op is a setting like those that many think will survive; because of its worker control and consumer counter control components; because of its emphasis on planning for goals other than maximum profit (like maintaining resources and not polluting); and because of its stress on quality products. However, to the extent that the co-op involves people in activities that preclude their participation in more important survival related activities--studying job performance there will be of little ultimate avail.

## BEHAVIORAL SYSTEMS ANALYSIS

We used a behavioral systems analysis to approach the problem of job performance in a food co-op. Systems analyses of various types have been used increasingly by government and corporate operations since World War II (Black, 1966), but the principles which contributed to their effectiveness went without formal integration with the principles of behavioral contingency management until recently.

The first step in behavioral systems analysis (Malott, 1972) provides a description of the existing system in behavioral terms, in order to suggest what behaviors and what consequences need and are amenable to modification. The analysis then calls for a statement of behavioral objectives for the modification or replacement of specific parts of the existing system. The design of an improved system takes place next and involves the theoretical applications of the principles of behavior. Implementation involves observing and consequating specified behaviors. Evaluation and recycling are based upon the early emphasis on observable and measurable behaviors; perhaps the most important common attribute of all systems approaches. So, a behavioral systems analysis is basically the same as a standard systems analysis in that it involves little, if any, additional steps, but at all steps, an analysis of the behavioral processes involved is made in terms of the principles of behavior.

A behavioral systems analysis does not, by any means, exclude a functional analysis. Evaluation should include methods for assessing the relative contributions of each potential independent variable. However, if such an evaluation involves more cost in effort, time or money than the projected benefits in achieving the desired results, some experimental rigor is best sacrificed. And as replication is of major interest to most applied systems, isolation of various factors of apparently successful manipulations may more efficiently or practically be attempted in later applications. Another reasonable way to resolve this difficulty of experimenting within a system is to use an "interrupted time series" quasi-experimental design with no control baselines (Campbell and Stanley, 1966), and to add multiple baseline and reversals to this basic time series if their use is necessary and opportune.

Without "between group" comparisons, we may be confident in a basic time series design if there is a relatively stable dependent variable measure prior to the introduction of the independent variable, and then we observe relatively abrupt changes following this introduction. The basic time series design has been used in several studies with this rationale (Christopherson, Arnold, Hill and Quilitch, 1972; Foxx and Azrin, 1973; Schnelle and Lee, 1972; Azrin and Wesolowski, 1974). As Michael (1974) has said in support of this type of design:

In the type of research emphasizing experimental control, and thereby often involving prolonged study of a small

number of organisms using relatively simple experimental designs, it is usually possible to change the procedure while the experiment is under way. If it appears that some previously unrecognized source of variation is causing trouble, the main manipulations can be postponed until means for controlling the interfering factor are developed. Or, if some aspect of the incoming results suggests an interesting variation, the experiment can be redirected immediately.

The basic time series design is also far easier than most to implement in a setting in which the clients, especially the present clients, participate in the design. From these clients' viewpoint, the basic time series design avoids the "unfairness" of multiple baselines in which some people are getting what others aren't, and it avoids obvious and "ominous" control involved in a reversal of treatment.

But sometimes, even in settings such as these, it is possible to use a more rigorous design. The goal of a science is to reduce the guesswork in replication and to increase the probability of successful implementation in other settings in the long run. If the outcome of a manipulation is in doubt, a reversal may be used as a form of recycling after evaluation; this will also greatly improve the design. Circumstances may allow critical parts of a system to be evaluated using a multiple baseline design, and the opportunity should not be missed. Each of the last three cycles of the present study (Cycles Two, Three and Four) represents a recycling after evaluating the earlier cycle (Cycle One) and finding one or more of its components in need of improvement. "Cycle One" is thus a post hoc label for what would have been a complete

experiment had the evaluation proved satisfactory. The several cycles are similar to a sequence in a standard experimental design. The time series rationale is relevant to Cycle One, but it is not particularly relevant to the evaluation of later cycles, as different designs superceded the basic time series in later cycles.

To be thorough, the essentials of each cycle are described, whether they proved successful or not. However, as replication of negative results is not of high priority, certain details are omitted. Further, in order to reduce redundancy, features in later cycles that are equivalent to those in earlier cycles are not detailed. Therefore, each cycle does not stand alone. Additionally, results that become interesting only when cumulated over several cycles are withheld from discussion until the appropriate moment.



CYCLE ONE: EVALUATION OF THE PERFORMANCE REVIEW USING A TIME  
SERIES DESIGN WITH THE GROUP AS THE UNIT OF ANALYSIS

Overview

In this cycle we attempted to evaluate the effects of a special performance review--one that takes place at the end of each shift and makes use of social reinforcement as well as more customary written feedback--on job performance and on interactions with workers. Changes in the co-op arose, causing us to discontinue observations on interactions, and to alter the design. Nevertheless, we demonstrated a substantial increase in job performance associated with the introduction of the performance review.

Phase 1. Behavioral Analysis - Setting and Subjects

This section describes the setting and subjects for all cycles of the present study. The setting was the People's Food Co-operative of Kalamazoo, a member-owned grocery and bakery. In the Spring of 1975, this co-op was the third largest of about eighty co-ops in the Michigan Federation of Food Co-ops, with a gross dollar volume of over \$10,000 per month.

Like all of the members of the Michigan Federation, this was part of a "counterculture" in that it contained few of the conspicuous features of traditional grocery stores. By design, the co-op avoided the use of advertising, display, or packing techniques associated with supermarkets. Customers waited on themselves when

purchasing non-prepacked items. According to a survey the co-op conducted, the average price was at least fifteen percent below similar items in supermarkets. However, the nutritional quality of the products, e.g., "natural foods", was often higher. Members made decisions at weekly meetings in which anyone who had paid monthly dues could vote, although those who worked had more votes in proportion to their participation.

Prior to this study the co-op had gone through two years of voluntarism followed by two years of minimal payments to some workers. Several members who did not work more than a few hours per month met each introduction of pay with strong opposition (Kent, 1974). They claimed that wages were evidence that workers were "out for themselves" and not for the good of the community. They said that paying workers threatened to replace mass participation with control by a few. Members who opposed paying workers added that their own motivation to volunteer would decrease when pay was offered--pay would stifle their spirit of service.

Managers (a category of worker in the co-op) who participated in the present experiment were not paid in money. Rather, they were paid \$.75 per hour in food credit for attending. This system was similar to those which gave discounts of food to workers. The difference was that managers in the food credit system were paid a specific hourly amount toward food purchases in the co-op--an amount not dependent upon how much food they actually bought.

Most of the managers who served as subjects in this study (in all four cycles) were under twenty-five years of age, many

being students or former students of Western Michigan University and Kalamazoo College. To the author's knowledge, in no case did a manager who earned food credit try to "make a living" from this form of pay. They may have occasionally traded the food credits (which were in "currency" form) to customers for money, but the absolute amount earned (about \$8.00 per week on the average) could not have paid for rent, food and other necessities. The managers seemed typical of the members of the new co-ops, supporting themselves with help from parents, spouses, savings, welfare and other jobs.

#### The co-op's history of job performance

Poor job performance had traditionally been a problem at the food co-op. In its early days shoppers were expected to do everything from adding their bills and making their own change to cleaning the store and ordering replacement products. This method of operation did not work. The co-op was constantly short of money and goods, and was engaged in a running battle with the health department. A hierarchy of work responsibilities soon developed, as did some primordial job descriptions. "Managers" worked four-hour shifts, supervising workers doing two-hour shifts. A couple of managers offered their phone numbers for others to call if any questions occurred about what to do. A booklet described how to wait on customers, how to find the "People's Broom Closet," and how to find "Zolt's house" to get the key to open and close the store.

An earlier study (Kent, 1974) indicated that job completion was at an unsatisfactory level under this manager-worker system. Cleaning and restocking under some managers had occasionally reached the stated 90% criterion even with adverse conditions such as busy days with some staff absent, but average job completion was still only 70%. This percentage did not improve appreciably when managers received more detailed checklists and job descriptions, nor when they received delayed feedback about their performance-- a delay of three days to a week depending on when the managers returned for their next shift. Neither small raises in pay nor the presence of more fellow workers and managers subsequently improved job performance.

At the time of the present study, two "co-ordinators" handled the ordering and receiving of goods, paying bills, bookkeeping, and financial management of the co-op. They earned \$1.25 per hour for thirty to forty hours per week. Untrained workers waited on customers and did most of the cleaning, restocking, and packaging. They earned \$.50 per hour in food credit, and worked two hour shifts on an irregular basis. Managers, the subjects of the present study, and cashiers worked a minimum of two four-hour shifts per week, with the manager supervising the cashier and the untrained workers. Managers and cashiers earned \$.75 per hour in food credit.

#### Dependent variables

Manager tasks were the dependent variables in the present study.

A behavioral analysis of the tasks should include an analysis of each of the three parts of the contingency:

1. Antecedents. Managers trained by working at least one four-hour shift, by reading a manual explaining the jobs and by completing a take-home fill-in-the-blank questionnaire over the manual. (The manual may be found in the Appendices.) Then they took an oral quiz over the manual and over the questionnaire. Oral remediation was available for those who initially did not pass. Once trained, managers used on-site job descriptions to aid them in their work. During each four-hour shift, managers filled out a checklist which included all their jobs and the criteria for completion. Other miscellaneous job aids were available. A "main supply" list depicted where various items might be found in stock, and the "People's Broom Closet" supplied appropriate implements and cleansers.

Social discriminative stimuli for completing work may have been present, although stated values toward work varied from the Puritan ethic to a "Mr. Natural" position, i.e. one favoring the path of least resistance.

2. Responses. The managers were responsible for seeing that certain tasks were completed, whether a worker did them or whether they did the tasks themselves. Jobs varied from cleaning the counter to making sure that over thirty

bins were properly stocked. The manual describing and giving the rationale for each of the jobs is included in Appendix A.

3. Consequences. At the time of this study no consequences were specifically programmed for job performance. The co-op had reverted to the checklist-only system (after group and individual-delayed feedback had failed to improve job completion [Kent, 1974]).

Immediate social consequences for work must have been present, given the low pay and the fact that it was not contingent upon work but upon attendance. It seems likely that unsystematic praise and criticism composed the most important of the otherwise "ineffable" contingencies referred to previously in the behavioral analysis of voluntarism. Managers often stated that they participated in the co-op in order to promote health food or a non-exploitative society, and to interact with others with the same goals. These statements may have occasionally served as discriminative stimuli for customers and the more concerned co-ordinators to praise managers for doing well or criticize them for not completing tasks. To the extent that such criticism was imitated by the managers, self-punishment, or "guilt control" (Malott, 1976) may have also been functional.

Another possible source of punishment for job completion was the effort involved in training and supervising naive workers to help with the jobs. We observed that managers more often than

not were doing jobs themselves, neglecting the other workers present. Presumably as a result of the lack of direction, the unaided other workers were often unproductive, so that often managers did not perform at criterion levels. At this point we suspected that better management of the managers was the solution to job performance problems. So we hypothesized that if managers better supervised workers, so that the workers contributed more to getting the jobs done, the jobs would be completed at criterion levels.

#### Independent variables

We attempted to improve manager supervisory behavior by giving them personal feedback about the quality and quantity of their supervisory behavior.

Our rationale for selecting this independent variable was as follows: we were interested in determining an effective and fairly inexpensive method of maintaining criterion job performance in the co-op. As previously mentioned, applied, behavior-analysis research in voluntary settings had not contrasted social and material consequences (nor has it investigated the effects of adding material consequences to social consequences, as in the present study).

In the present setting we had earlier observed job performance under conditions which followed a continuum of known procedures from the less "obvious" and material to the more obvious. The less obvious a procedure is, the less objectionable it is to many new co-op workers, and thus the higher the probability of successful implementation, as discussed.

Immediate individual feedback might have been a next logical step following the failure of delayed individual feedback. However, there were several reasons why we did not use immediate, written feedback alone as our independent variable:

1. Written feedback alone had proven relatively ineffective in voluntary settings (Kohlenberg et al., 1976; Meyers, et al., 1976; Seaver and Patterson, 1976), perhaps because it involved a minimum of direct social reinforcement and punishment.
2. We could lessen problems due to misunderstandings by giving the manager an opportunity to ask questions.
3. And so the two-way verbal interaction seemed more readily attained with an oral presentation rather than with a written presentation.

So we chose orally-delivered feedback, as it would facilitate communication and it might increase the social consequences associated with that feedback--that is it might involve more social reinforcers for work completed and more social punishers for work not completed. Small monetary rewards were not used at this time because they were more costly and more offensive to many of the members--a next step to be tried only if the other procedures failed.

#### Phase 2. Statement of the Behavioral Objective

There were two basic behavioral objectives for this study. The first was to improve manager-worker interactions and related



activities, and the second, related objective, was to improve job performance as measured by observing permanent products.

#### Interactions and related activities

Some simple management guidelines, or "manager duties", specified a first set of objectives, which included what to do when interacting with workers who were on-task or off-task (on-task being defined as engaging in a job on the checklist, including the "manager duties"). The guidelines were: 1) Praise on-task workers: Say something nice to workers who are doing a job right, who are improving, or who are working though making mistakes (but tell them how to do it right). 2) Prompt off-task workers. Ask idle workers to do any specific job that needs to be done. 3) Work: Be busy when other workers are busy. (Note that this last category does not involve worker supervision.) The manager was to be engaged in one of the above three activities at all times. The manager manual includes examples and rationale for these duties, though a detailed, behavioral specification was not given (See Appendix B). In addition to the three objectives, we measured interactions themselves (actually a subcategory of objective three) to see if managers' behavior changed with respect to frequency as well as quality of interactions. We added data concerning whether workers were busy to try to measure whether this aspect of worker behavior changed as a function of any change in manager behavior.

### Job performance

A second objective, set by the members of the co-op, was that each shift under supervision of a manager would complete at least 87% of the non-priority jobs and 100% of the priority jobs if a cashier were present. The members of the co-op decided that four jobs should be "priority" jobs, due to the crucial role of those jobs in the immediate economic survival of the co-op. The members of the co-op deemed these jobs "more important" than the other jobs in the sense that they should be completed even if the other jobs were not completed. However, the priority jobs need not have been completed prior to other jobs. The rationale was simply to get them done. The additional weight placed on the priority jobs by the 100% criterion rule (and later by consequences backing up the rules) was meant to reflect this expressed emphasis.

These priority jobs involved restocking the shelves (jobs 23, 29, 31, 33 in Appendix A). For example, one priority job required: "Reach-in cooler stocked to capacity--no less than two of an item if stock available." Thirty to thirty-four non-priority jobs filled out the checklist, (varying slightly in number from shift to shift. For a complete description of each job and an operational definition of its completion see Appendix A. The checklist and other instrumentation are not included because they are unique to the setting and also difficult to interpret without much additional explanation).

Most of the non-priority jobs were cleaning tasks. For example, one non-priority job required: shelves, bottles and bins dusted--

clean to sight and touch. If no more than four out of the minimal 30 tasks were left incomplete, the 87% criterion would be met.

With the approval of the members, the experimenters specified that these criteria were expected only when a cashier was present throughout the shift. This was because the volume of business in the store had increased to the point where the managers often could not complete the jobs otherwise.

The performance review was conducted as follows: The co-ordinator asked the manager to turn in the checklist at the end of each shift. Then he or she put the percentages of jobs done (as observed) on the list, and added a check in a box if performance met or exceeded criterion, and marked each job not done before returning the list to the manager. At that time the co-ordinator also gave the manager written feedback on interactions with the workers, by checking the boxes corresponding to descriptions of proper interactions if those interactions occurred as specified. Some examples of subsequent oral feedback on interactions follow:

"At the time I checked, you were working but one of the workers in the same area was not busy. Next time if you could try to ask that worker to do a specific job, that would be good" (for manager guideline number two above).

"You were talking to a worker who was busy, and you said \_\_\_\_\_". If you could say something nice or encouraging to the worker next time, that would be better" (for manager guideline number one above).

Co-ordinators were to praise the managers for completing jobs and for interacting appropriately (according to the manager task description in Appendix B), and were to answer any questions the manager had after feedback was given.

### Phase 3. Design

We planned to use a reversal design to assess the effects of the treatment package--a written and oral review of job performance and social interactions performance. The planned conditions of the design were:

1. Baseline (no consequences).
2. Performance review (on job performance and social interactions).
3. Baseline (no consequences).
4. Performance review (on interactions only).
5. Performance review (on job performance and social interactions).

The design was a time-series, and we used the group as the unit of analysis in that we made the changes for all managers at the same time (Campbell and Stanley, 1966).

Each condition was to be in effect until stability was attained. We planned a reversal in the event that the performance review resulted in criterion performance. That is, we would remove the performance review, and see if performance returned to baseline levels. We would then reinstate the performance review on interactions before reinstating it on job performance to assess the

independent effects on job performance of those two major components of the original performance review.

Feedback was delivered personally by a co-ordinator. We programmed it to occur right after the shift for each of the four current managers. We called this treatment package a "performance review". One or the other of the two co-ordinators supervised every shift. The co-ordinators' other schedules mainly determined which specific shifts which co-ordinator supervised.

#### Observations, reliability and procedural details

Co-ordinators observed job performance in the middle and at the end of the shifts, to avoid the problem of jobs being done during a shift without being observed as complete at the end of that shift. Our concern was that even though managers knew that the jobs were to be completed at the end of the shift, they would sometimes be too busy doing other things, like waiting on customers, and thus mid-shift would exceed end-of-shift performance. For example, a manager might have waited until the last minute to do or check on several cleaning tasks that looked at mid-shift like they would require little or no additional effort to re-do at the end of the shift. However, by the end of the shift, these jobs may have increased in difficulty due to customer activity, and that same activity may have kept the managers too occupied to complete them.

Manager's interactions were observed every half hour with a

range of plus or minus ten minutes. The observers could not use a more formal sampling procedure because they were also working as co-ordinators and were thus sometimes involved in other activities. A possible limitation of this procedure is the chance that the experimenter-observers made their time samples contingent upon manager or worker behavior. This possibility exists because of the non-programmed variability in the time of occurrence of the observations. The observers were often present at times other than those being sampled. They noted workers and managers as either on-task or off-task, and, if they were interacting with a worker, as either praising or prompting. They observed the manager for up to an additional five minutes to obtain a sufficient number of interactions. The observation period for an interaction thus consisted of a momentary sample (the observer looked up and then immediately recorded what he or she saw) followed by up to five additional minutes of observation if an interaction had not occurred.

A second observer (the one co-ordinator of the two who was the off-duty co-ordinator) made frequent independent reliability checks for both job performance and interactions, at approximately the same time as the primary observer. The secondary observer's other schedule mainly determined when he or she would conduct those reliability checks. The plan was to check frequently at first, then less frequently after acceptable reliability was attained--but still check at least once per condition. The recording sheet divided jobs into areas of the store (customer-service area, kitchen, etc.). The observers would both complete

one area before moving on to the next; but they would not look at the same jobs at the same time in order to avoid seeing each other's recordings or marking gestures. For example, observers would be in the "customer service" area at the same time, checking to see if bins were properly restocked. But the observers would start at opposite ends of the area, most often with their backs to each other, or at worst side to side several feet away. It is possible that observers could have given each other cues about whether or not they had recorded a job as done; but this possibility had to be weighed against the likelihood that the job would be undone by a customer in the time between the two observations, if that time were very great. However, as experimenter-observers we never observed those cues. Also, since the store was small, it would have been possible for one observer to see the other no matter what areas they were in.

For the off-task, on-task data for each worker, observers assigned numbers to workers on the recording sheet prior to the observation. Observers went from one area to the next. If more than one worker were in an area simultaneously, the primary observer would then state or gesture the number of the worker to be observed to indicate the start of the momentary time sample (during which the observers merely looked up and immediately recorded what they say). Generally the primary observer would select the worker closest to him for the first observation. Observers remained several feet away from each other to insure independence was maintained during these observations. Observers took similar

precautions when observing manager on-task and off-task, and manager-worker interactions.

The establishment and maintenance of reliability for the observers consisted of an occasional rereading of the manager manual, with discussion between them of interpretations of the written descriptions. But no written descriptions were altered during the course of the study, as observers concluded that the descriptions were sufficiently clear and complete, though they obtained no independent confirmation from the managers. Also, after each reliability check, observers discussed results in terms of agreements and disagreements.

Over a 45 day period 399 observations of interactions were made on over 50 shifts. Manager interactions were observed starting with the third week of baseline, as the recording forms were not ready until then. From 3 to 102 reliability checks were made on various types of manager behaviors. Observations of job performance were made every time a manager and cashier were present for the whole shift. Occasionally data were obtained on the same manager during two shifts on the same day. Twenty-four reliability checks were made on job performance throughout the fifteen weeks of Cycle One.

#### Phase 4. Implementation--Approval by the Members

Implementation included an aspect not often encountered in this history of the analysis of behavior; namely, the introduction of experimental contingencies in a social system in which the clients participated in the design.



An attempt was made to explain the design of the behavioral systems to everyone involved. We used the rules of contingency management (Malott, 1972): start at the existing level of behavior, proceed in small steps, and consistently apply consequences as immediately as possible upon the emission of appropriate behaviors. We used words which were more a part of the manager's and other co-op members' existing repertoires than psychological jargon would have been. We did not use potentially aversive words such as "control," "manipulation," and "intervention."

It is important to ensure that those who are designing contingencies are subject to those same contingencies or to some equally effective form of counter-control. This is consistent with a system which is based upon the idea that behavior is modified by its consequences, and that people should be affected by the consequences of their own behavior. The co-ordinators were experimenters and observers in the present study, and were subject to many of the same contingencies as the managers in terms of accountability to the general meeting and having their work observed, evaluated and consequated.

A general co-op meeting approved the performance review system after discussing it at several meetings. The co-ordinators offered to do the observations and the performance reviews, as well as to make the additions to the manager manual. At this time managers were not particularly enthusiastic about the proposal, but other members were supportive, and no one voted against the proposal.

It is helpful to note here that several experiments had been conducted in the co-op prior to the current study, and much of the initial opposition to experimentation as such had diminished. However, this did not diminish our concern with the implementation of each specific new design feature that was proposed.

#### Phase 5--Evaluation: Part I. Social Interactions

Observers recorded managers and workers as either on-task or off-task. If an interaction occurred, they recorded managers as either praising or prompting. Although there were only three objectives for manager interactions (1. praise, 2. prompt, 3. work), we analyzed the recordings in five categories. The categories of 1) praise, 2) prompt, and 3) manager on-task correspond to the three objectives. We added data on interactions themselves (actually a subcategory of manager on-task) to see if managers' behavior changed with respect to frequency as well as quality of interactions. We added data for worker on-task to try to measure whether this aspect of worker behavior changed as a function of any change in manager behavior.

The five categories were:

1. Praise: Managers did not praise on-task workers more often after the treatment. Praise for on-task interactions was 15% of 22 interactions in baseline, and 10% of 26 interactions in treatment. There were no disagreements between observers on this measure for three sampled occurrences of praise on three days.

2. Prompts: Managers did prompt off-task workers more frequently after treatment. Prompts for off-task interactions was at an average of 33% of 36 interactions in baseline. Prompts for off-task interactions increased to an average of 65% for 20 interactions following treatment. Variability was moderate (range 20-50% in baseline and 60-70% in treatment). There were no disagreements between observers on this measure for three sampled occurrences of prompts on three days.
3. Managers on-task: There was no improvement in the percent of times the managers were on-task after the performance review was implemented. The group median was 82% for managers on-task for six weeks of baseline. For eight weeks following the performance review the group median was 77%. Data were highly variable with a range of 50% to 100% on-task, in each condition. The group median was computed as follows: a given manager might have from one to eight observations on a given shift. The percent on-task was then computed for the manager for that shift. For all of the managers combined, there might be up to twelve shifts in a week, and therefore up to twelve measures of percent on-task. The median of these percent-on-task measures was then computed. This figure was the group median. Reliability was 100% for 41 observations on 17 days.

4. **Interactions:** Managers did not interact with workers a greater percentage of the time after the performance review began. Manager interactions occurred during an average of 43% of the observations for the four weeks of baseline. Over the eight weeks of treatment, the group median was 40%. These data were also highly variable, ranging from 0-83% in baseline and 0-70% in treatment. Reliability was 100% on 18 days for 35 observations in which the manager was available (the manager was often in an area of the store not being observed).
5. **Workers on task:** There was no appreciable improvement in the percent of samples workers were on-task after the introduction of the performance review of the manager's activities. The group median was 69% for workers on-task for six weeks of baseline. The group median was 75% for eight weeks following the performance review. Again data were highly variable with a range of 40-100% in baseline and 40-90% in treatment. The reliability was 89% for 102 samples on 23 days, a sample being defined as any record of an individual worker being on-task or off-task, since more than one worker might be recorded in a single observation.

In sum, the performance review treatment for interactions produced no appreciable effects on manager or worker on-task, nor

on manager interactions per opportunity, nor on praise during on-task interactions. However, the treatment increased manager prompts during off-task interactions.

Phase 5--Evaluation: Part II. Job Performance

As Table 1 shows, the median non-priority job performance was 39.5% for four managers during the seven week baseline. The median non-priority job performance increased to 71% for one week when the performance review began, then it stabilized around 62% for the total eight week period of performance reviews. Each of the four managers improved considerably over baseline.

TABLE 1

Median Percent Non-priority Jobs Done  
by Individual Managers from Cycle 1

Manager	Baseline	Performance Review
1	32(9)*	71(7)
2	49(9)	66(6)
3	39(6)	58(5)
4	40(6)	53(7)
Median (Total Number of Shifts)	39.5(30)	62(25)

\*The numbers in parentheses indicate the number of shifts for which data were taken.

The variability of performance was very low in both conditions (with a range generally less than  $\pm 10$ ) excepting the first week of the performance review. There were almost no overlapping points between conditions. The basic time series analysis would thus suggest a strong relation between the introduction of the performance review and improved performance.

As in the remaining cycles of this study, data were collected in Cycle One on all shifts that met the criteria that both a manager and a cashier be present for the entire shift. All managers who met these criteria were included in the data.

Mid-shift performance remained consistently lower than end-of-shift performance after the performance review began. Therefore, our previously discussed concern was unfounded and only end-of-shift performance is reported.

We are not dealing with priority jobs in this cycle because the sample size was too small to get reliable measures of central tendency. That problem arose because there were only four priority jobs and only a small number of shifts for a relatively small number of subjects. (Data on priority jobs obtained from the third and fourth cycle are presented in the "Overall-Evaluation" section, pages 69 to 80. The combination of those two cycles provided enough data to obtain reliable measures.)

We can only speculate as to why performance in baseline was about 30% lower than in similar conditions in the previous study (Kent, 1974). Since over nine months had passed since the earlier

study, many factors could have contributed, including increased business and selection of poorer managers.

Twenty-four reliability checks throughout the fifteen weeks averaged 85% (range 67-97%) between independent observers using the formula  $[\text{agreements}/(\text{agreements} + \text{disagreements})] \times 100$ . For each reliability check, agreements and disagreements were determined as follows: agreement was measured for each individual task, and the total number of tasks on which agreement occurred was totaled, for a given manager on a given shift. The same was done for disagreements. Then those two totals were used in the reliability formula to compute the reliability for a given manager on a given shift. Observers made more reliability checks at first, with only two weeks (12 and 13) not containing at least one check. The reliability scores tended to improve slightly throughout the study.

#### Phase 6. Recycling

The treatment produced a substantial increase in work done that maintained throughout this cycle. However, even the modal gain in the first week after the introduction of the performance review fell far short of the specified objective. It should be noted that the observed increase could at this point be attributed to the performance review's direct affect on job performance, and/or to the increased prompting of workers by managers. While neither managers nor workers were on-task more after treatment

than before, an increase in the efficiency of either or both of their work could account for observed differences.

Observations of social interactions and of on-task/off-task behavior ended prematurely due to a major policy change at the co-op. A rule setting a maximum number of one worker per shift replaced the no-maximum policy. This rule was intended to improve efficiency. Under the circumstances, the experimenters then decided that the relevant behavior was work done, in most instances directly by the manager, not indirectly through supervision of workers. So, while the job performance dependent variable remained, we were no longer interested in the manager-worker interactions. In other words we were dealing with the person called "manager" though, in fact, they would most often no longer be managing the work of others.

Dissension and loss of subjects then began to be problematic. In the second week of feedback, some members of the co-op called a meeting to decide whether or not the study would continue. Out of 30 people attending, only four voted to stop the study, but two of the four were managers (all four managers were present). The two dissatisfied managers stated that they did not like being "forced" to work harder and that they did not like being studied. Within the next two weeks these managers quit. Another followed in two weeks. At least three of these four managers sometimes aggressed by punching holes in their work forms, refusing to fill out work forms, vocally criticizing the study, or attempting at



weekly meetings to vote an end to the study. So we redesigned the study while new managers were being recruited.

A logical way to redesign the study under the circumstances would have been merely to discontinue the performance review on interactions, because there were no workers to manage. However, the results as well as the policy change called for a new design. No matter the number of workers present, it did not look as though the performance review maintained consistent criterion performance. Something stronger (and more obvious) needed to be added. Money was not the only extrinsic consequence considered. We discussed the use of a more powerful social consequence than that included in the performance review. Perhaps group praise and criticism at a weekly meeting based on an earlier performance review would improve job performance. But due to problems in controlling levels of the independent variable, obtaining reliability on those levels, and the general problems involved with aggression and emotional side effects in group sessions, we ruled out this option. We would add a small monetary consequence, but only after trying the performance review again with the new set of managers.

CYCLE TWO: EVALUATION OF THE PERFORMANCE REVIEW USING  
A MULTIPLE BASELINE ACROSS SUBJECTS DESIGN

Overview

The planned bonus was not implemented in this cycle due to high turnover of managers. However, the performance review introduced at different times with each manager produced uniformly improved job performance.

Phase 2. Statement of the Behavioral Objective

Only the first objective of Cycle One remained in Cycle Two. Each shift under supervision of a manager would complete at least 100% of the priority jobs and 87% of the non-priority jobs if a cashier were present. However, only non-priority jobs are dealt with in this cycle due to the small sample size for priority jobs. In addition, since the maximum number of workers per shift was now one (instead of the unlimited number of Cycle One), it could be said that the behavior of interest is not "management" at all but rather the direct work of our "managers". So "manager" was to a certain extent a misnomer. But we will continue to call them "managers" for purposes of this study because they were still called managers in the co-op, and because the cashier and an occasional worker may have aided them. The dependent variable was the permanent product of job performance as observed at the end of each shift.

### Phase 3. Design

Basically, Cycle Two differed from Cycle One in the following ways: a) a bonus condition was added (with an accompanying shaping procedure), b) the design became a multiple baseline across subjects, and c) data were taken on job performance only, not on interactions. We planned to add \$.25 food credit per hour to manager pay contingent upon criterion performance. We chose \$.25 because it was the smallest amount we thought would affect job performance (based on the demonstrated effect of relatively small consequences in volunteer settings as cited earlier; however, we had no empirical evidence concerning the effectiveness of \$.25 in this situation). Managers could then earn \$1.00 per hour. This bonus was to be implemented with each manager individually after three baseline shifts and seven shifts with the performance review treatment package. ("Bonus" throughout this study refers to a contingent bonus, or food credit earned only after achieving criteria, while in later cycles "non-contingent bonus" will refer to food credit given to the managers regardless of their performance). In effect, we shortened baseline to a minimum number of three shifts and decided on a treatment phase to last as long as the previous baseline.

We used a changing criterion procedure to facilitate initial contact with the bonus. Managers would at first have to complete only 50% of the priority jobs and 75% of the non-priority jobs to earn the bonus, then 75% and 87%, and then the standard 100% for

priority and 87% for non-priority jobs. We chose the initial criteria based upon performance under the performance review in Cycle One. That is, almost everyone reached and exceeded 50% priority and 75% non-priority job completion under performance review conditions in Cycle One. We were thus more confident that they could achieve these levels early in the bonus condition. For priority jobs note that we had to start at a lower criterion (50%) than for non-priority jobs (75%), even though we ultimately required a higher criterion for priority jobs (100%) than for non-priority jobs (87%). We had to start at such a low criterion for the priority tasks to be sure that the managers would initially receive the bonus for those tasks before we started raising our criteria, because their baseline performance was so low. We thought that if they earned the bonus once, it would more likely control future behavior. The other previously mentioned treatment components remained; i.e., working one shift and completing the quiz over the manager manual.

We planned a reversal in the event that the bonus was effective. We would give the extra \$.25 per hour non-contingently, and continue to conduct the performance review. In this way we could separate the effects of the contingent pay from those of the performance review. For example, if performance decreased to below criterion when the bonus was no longer contingent, and then increased when the bonus was made contingent again, we could suggest that the contingency caused the improved performance.

Neither the \$.25 by itself (non-contingent), nor a mere coincidental improvement during review conditions, could reasonable account for such orderly variation. We used a non-contingent bonus (which of course is not a bonus at all) because of potentially disrupting side effects of withdrawing pay.

In summary, the Cycle One conditions were as follows:

1. Baseline.
2. Performance review.
3. Baseline.
4. Performance review on interactions only.
5. Performance review.

A basic time series design was used, with the group as the unit of analysis and no specific number of shifts was specified for a given condition.

In Cycle Two the design consisted of the following conditions.

1. Baseline (3 shifts).
2. Performance review (7 shifts).
3. Performance review plus bonus (undetermined number of shifts).
4. Performance review plus non-contingent bonus (undetermined number of shifts).
5. Performance review plus bonus (undetermined number of shifts).

Each manager was treated as a separate unit in a multiple-baseline, across-subjects design. Data were not taken on interactions.

### Observations and procedural details

One manager had remained from the first group of four, and others entered at different times. So now, each manager was treated as a separate unit, whereas in the first cycle all managers were under the same conditions concurrently. The design thus became a multiple baseline across subjects with within-subject reversals.

The multiple baseline design (Baer, Wolf, and Risley, 1968) has two basic advantages over the basic time series. First, it adds a control baseline that should be affected by any coincidental variable that might have caused a change in the experimental baseline (Campbell and Stanley, 1966). Second, it replicates the intervention of the independent variable. Because the superiority of the performance review over baseline had only been demonstrated in Cycle One with a basic time series, we were still interested in replication of this effect in Cycle Two. For this purpose, the standard three shift baseline of Cycle Two had the disadvantage that stability may not be obtained and thus comparison to another condition made difficult.

But we thought an advantage of the new design outweighed this disadvantage. Several subjects would be used in the new Cycle Two multiple baseline across subjects design. Based on our Cycle One data, we could predict that for most subjects, most data points in the performance review condition would be higher than those in baseline. A few exceptions among several subjects, and a few overlapping points between conditions for a few subjects would not

detract much from the basic conclusion that the performance review was superior to baseline. In a sense, then, the new design combined features from single subject and group designs.

We eliminated the mid-shift job performance observation because, as noted previously, end-shift performance was consistently higher. And also at the start of this cycle we explained to each manager the rationale that jobs should be completed at the end of each shift, both to best help the next shift and to make evaluation of the current shift possible. This rationale was approved at meetings and was received fairly well by the managers.

#### Phase 4. Implementation--Approval by the Members

Many of the details of implementation included in Cycle One apply to this cycle as well. The proposed changes were approved at a general meeting after being discussed at a previous meeting. Some managers and other members expressed reluctant approval, saying, in effect, "do we have to resort to this?" The co-ordinators noted that in the meeting called to discuss the continuance of the study a mandate had arisen from the membership for improved job performance. The members approved each job on the list and the specified criteria. The co-ordinators pointed out that the performance review did not appear to have been effective in getting performance to criterion (though it produced considerable improvement), but in the event that it proved effective with the new managers, the bonus would not have to be used.

We thought at this time that a multiple baseline design could be implemented without complaints about unfairness or arbitrariness. Managers would be entering the experiment at different times, thus producing a natural multiple baseline. Further changes would be based on seniority. This would tend to reduce possible accusations such as "So and so is getting a bonus and I'm not."

#### Phase 5. Evaluation

Contrary to the Cycle Two design we did not implement the bonus in this cycle, as only one manager progressed to the sixth shift of the performance review condition. Also, absences and lack of cashiers for many shifts slowed the study, as we took data only when a cashier was present.

As in Cycle One, all managers improved following the introduction of the performance review. The median, baseline, non-priority job performance for all five managers started out at 50%, increasing abruptly to 70% for four managers at the beginning of the performance review phase, and falling off to about 55% during the last four shifts of that phase, with a median for the whole phase of 57.5%. Table 2 shows these data. Reliability remained at 85% for five observations. Manager 1 does not appear in Table 2, even though his performance review condition was in Cycle Two, because this condition overlapped with Cycle One and is included in Table 1.



## Phase 6. Recycling

Unlike Cycle One, managers did not complain of hard work or of being studied. However, these factors cannot be ruled out in explaining high absenteeism and turnover.

As in Cycle One, there was the phenomenon of a peak followed by a plateau in the performance review condition. Because turnover was so rapid, managers never received the bonus; thus, in effect, Cycle Two was a replication with a multiple baseline of Cycle One. And the next step clearly seemed to be the redesign of our system to reduce the turnover rate and thereby give the existing bonus system a chance to affect the job performance of the managers.

TABLE 2

Median Percent Non-priority Jobs Done by  
Individual Managers from Cycle Two

Manager	Condition	
	Baseline	Performance Review
5	73(3)*	80(4)
6	47(3)	56(2)
7	53(3)	59(6)
8	41(3)	53(3)
9	57(2)**	--
Median (Total Shifts)	50(12)	57.5(15)

\*The numbers in parentheses indicate the number of shifts for which data were taken.

\*\*These data are not included in the group median, as this manager did not continue to the performance review condition.

CYCLE THREE: EVALUATION OF THE PERFORMANCE REVIEW PLUS BONUS  
USING A MULTIPLE BASELINE ACROSS SUBJECTS

Overview

High turnover of managers again prevented the use of reversals. But each manager performed better following the introduction of the performance review plus the small bonus. In this third replication, the performance review alone exceeded baseline by a considerable amount.

Phase 2. Statement of the Behavioral Objective

The objective remained 87% and 100% for non-priority and priority jobs, respectively. The dependent variable remained the permanent products of job performance, in most cases jobs probably done directly by the manager. Data on priority jobs obtained from this cycle are presented in the "Overall Evaluation" section, except for managers 19, 20, and 21, who had too few data points to include.

Phase 3. Design

In an attempt to decrease the turnover rate, we increased pay from \$.75 to \$1.25 per hour for those managers who would work eight or more hours per week. We also reduced the number of shifts in the performance review phase from seven to four, so the entire pre-bonus "apprenticeship" was seven instead of ten shifts.

In the bonus condition, all the managers received notice at the end of each shift as to whether or not they had earned their bonus. The managers who worked less than eight hours were still paid at the end of each shift. (However, the managers who worked eight or more hours were now paid weekly instead of at the end of each shift.)

The Cycle Three design was thus exactly the same as Cycle Two in its sequence of conditions, while it differed from Cycle Two in that the second condition, performance review, consisted of four, not seven, required shifts, so that sequence was:

1. Baseline (3 shifts).
2. Performance review (4 shifts).
3. Performance review plus bonus (undetermined number of shifts).
4. Performance review plus non-contingent bonus (undetermined number of shifts).
5. Performance review plus bonus (undetermined number of shifts).

#### Phase 4. Implementation--Approval by the Members

Many of the details of implementation included in Cycle One apply to this cycle as well. The co-op membership approved these changes at a meeting. The co-ordinators emphasized the data from preceding attempts to improve performance in appealing to the membership for authorization to make necessary changes. We also stressed the difficulty of constantly training managers. The proposal to increase food credit pay was accompanied by financial

data showing that funds were available for this purpose.

#### Phase 5. Evaluation

Table 3 shows that the performance review plus the bonus produced consistent, median, criterion, non-priority job performance, for the first time in the study. The changing criteria were rarely used, as managers usually made immediate improvements to levels above the standard criteria. The performance review again showed higher performance than did baseline, but not criterion performance. The medians for baseline, performance review, and performance review plus bonus were 65%, 78%, and 95%, respectively. Reliability was 87% for four observations, at least one in each condition.

These data included four managers who worked more than eight hours per week and six who worked eight or less. The higher paid eight-hour managers sometimes met criteria in baseline and in the performance review condition. However, the two members of this group who did advance to the performance review plus bonus condition showed the same improvement in performance as the other managers.

Three out of eighteen times managers completed fewer than the planned number of shifts in a particular condition. This occurred due to experimenter error (adding the bonus too early by mistake) or due to attempts to obtain data for more than one condition for managers who were expected to leave the co-op. The data in a given condition were often stable after only three shifts, allowing us to advance to the next condition.

TABLE 3  
Median Percent Non-priority Jobs Done By  
Individual Managers from Cycle Three

Manager	Baseline	Performance Review	Performance Review + Bonus
1*	32(9)**	71(7)	89(6)
10	67(6) $\Delta$	75(3) $\Delta$	95(5)
11	80(2)	79(4)	--
12	62(3)	63(2)	--
13	63(3)	78(3)	92(4)
14	67(3)	84(4)	96(4)
15	72(3)	84(4)	--
16 $\Delta\Delta$	70(3)	73(4)	96(3)
17 $\Delta\Delta$	70(3)	87(4)	92(6)
18 $\Delta\Delta$	93(3)	91(4)	100(5)
19	50(2)	--	--
20	53(1)	--	--
21 $\Delta\Delta$	80(2)	79(1)	--
Median for those managers in all three conditions	67(30)	78(29)	95(33)
Median for those managers in both of the first two conditions	67(10)	79(11)	

\*The first two conditions for this manager are also included in Cycle One (Table 2). They are included here to facilitate comparison with the performance review plus bonus condition.

\*\*The numbers in parentheses indicate the number of shifts for which data were taken.

$\Delta$ These data include two separate Baseline (A), and Performance Review (R), conditions for this manager, who left the experiment and then reentered at a later date.

$\Delta\Delta$ Indicates those managers who worked more than eight hours per week. These managers were paid \$.50 more per hour and received this pay weekly instead of at the end of each shift.

But turnover remained a problem, since only seven of the first thirteen managers stayed until the first shift and only two stayed past the fifth of the seven bonus shifts. During this cycle only manager 18 entered the performance review and non-contingent bonus phase (although these data are presented in Cycle Four). The data for the performance review and non-contingent bonus condition, and the second performance review plus bonus condition for manager 1 are included in Table 5 rather than Table 3 because manager 1 was the only manager in this cycle who completed those two conditions.

#### Phase 6. Recycling

Turnover of managers still prevented our evaluation of the effect of the bonus using a reversal design.

Performance for eight-hour managers was not typical of managers in earlier cycles. Their baselines were much higher, and they did not show the temporary gain in the performance review. Higher pay, selection of better performers and the shortened apprenticeship might account for these data. Both the higher pay and the fact that the co-op had grown and was becoming more popular could account for its attracting people with greater initial skills. Skills related to specific jobs were not relevant, as everyone could do them. But self-management skills could help a manager perform better without the programmed contingencies.

Also, two of the managers who worked more than eight hours were declining in performance on non-priority jobs during the

bonus phase, and they were not picking up their bonus when they had earned it. The performance review may not have made contact with them, either; as they often left immediately at the end of the shift before the cross-check could be completed and summarized.

Observers had moved the crosscheck to the very end of the shift because of requests by managers that they be given the last fifteen minutes to finish the jobs. If we could not conduct the performance review, we put the written feedback in the manager's personal folder. But some managers did not refer to these forms, either on their next shift or when making out their pay reports. Support for this notion comes from one of the two managers whose performance declined during the bonus phase. She stated that she had not recorded a bonus on her weekly self-report because it was not clear to her whether she had earned it.

CYCLE FOUR: EVALUATION OF THE PERFORMANCE REVIEW PLUS BONUS  
USING A REVERSAL, ALONG WITH AN EVALUATION OF THE  
BONUS COMPONENT USING A MULTIPLE BASELINE ACROSS SUBJECTS

Overview

In this cycle, we continued the multiple baseline, across subjects design. First we shifted to a non-contingent bonus from a contingent bonus (managers' performance decreased markedly). We then re-introduced the performance review plus bonus. Following that, we again used a non-contingent bonus but this time leaving in the performance review component. Again performance decreased. Because of manager turnover, we were unable to again reintroduce the performance review plus bonus to confirm the advantage of the contingent bonus. However, each manager showed decreases in performance following the removal of the contingency aspects of the bonus, even though the performance review was retained.

Phase 2. Statement of the Behavioral Objective

The objective remained 87% and 100% for non-priority and priority jobs, respectively. The dependent variable remained the permanent products of job performance, in most cases jobs probably done directly by the managers. Data on priority jobs obtained from this cycle are presented in the "Overall Evaluation" section.



### Phase 3. Design

In Cycles Two and Three we had planned a reversal design described in the following three conditions:

1. Performance review plus bonus.
2. Performance review plus non-contingent bonus.
3. Performance review plus bonus.

The reversal design was to have followed preliminary sessions which, by Cycle Three, consisted of a three shift baseline and a four shift performance review.

For Cycle Four, we planned the reversal design shown in the following four conditions:

1. Performance review plus bonus (5 shifts).
2. Non-contingent bonus (without performance review)(5 shifts).
3. Performance review plus bonus (5 shifts).
4. Performance review plus non-contingent bonus (undetermined number of shifts).

No preliminary sessions were included. We discontinued the preliminary sessions (three shifts of baseline and four of performance review) since a shortened experimental duration would increase the chances that managers would go through all the conditions of current interest. We omitted the simple performance review part of the preliminary sessions since our data indicated that the performance review alone was not powerful enough to consistently produce criterion performance.

The current reversal design differed from the previous one in

two ways: First, condition two was different. In the current design, condition two consisted of a non-contingent bonus without a performance review while in the earlier design, condition two consisted of a non-contingent bonus with a performance review. We did this to enhance the probability that we would demonstrate an effect with the whole treatment package.

Secondly we added a fourth condition to the new design to test the independent effects of the contingent pay component, (the bonus, or food credit earned only after achieving criteria, as opposed to "non-contingent bonus", or food credit given regardless of performance), in the event that the entire treatment package proved successful.

As with other cycles, we planned the number of conditions and the number of shifts in each condition with an eye toward two objectives; a) getting the data necessary to demonstrate differences between conditions with the multiple baseline across subjects design, while b) keeping the total number of shifts as low as possible to minimize the loss of critical data through turnover.

We posted pay report forms on a bulletin board so managers could list their hours of work daily rather than weekly as before. Managers also agreed that the crosscheck would be done at the optimal time; namely during, not after, the last fifteen minutes of each shift. These changes made it possible for the co-ordinator to do the cross-check, summarize it, give the performance review, and then to record the payment of the bonus on the manager's report

form before the manager left.

#### Observations, reliability and procedural details

In this cycle, one of the cashiers assisted in conducting a reliability check along with the co-ordinator. Since the primary observer had been the on-duty co-ordinator, and the reliability observer had been the other co-ordinator, we thought having an "outside" observer who was familiar with the manager jobs and with the co-op but who was by no means an "expert" would be valuable.

Prior to this cycle we had only reintroduced a contingent bonus (completed the reversal) with one manager. Removing the contingent bonus by giving the "free bonus" was not hard to implement. However, reintroducing the contingency presented more problems. It was understood by each manager that the meeting had approved a design in which the bonus would be contingent, then "free", and then reinstated at the discretion of the co-ordinators. The individual scheduling aspects of the multiple baseline design helped considerably. We informed the manager that it was time to bring back the "earned bonus" and asked if that was okay. If they asked if this was due to poor performance, we said that overall, it looked like the earned bonus worked better. If any had said bringing back the earned bonus was not okay, then the co-ordinators would probably have taken the issue to a general meeting for resolution. This circumstance never arose. We did not reveal individual data.

#### Phase 4. Implementation--Approval by the Members

Many of the details of implementation included in Cycle One apply to this cycle as well. The general meeting approved all of the above design changes. Again the co-ordinators stressed the data concerning the relative ineffectiveness of the performance review (but at no time did they reveal individual data).

#### Phase 5. Evaluation

As Table 4 shows, the bonus had a clear cut effect: about 20% higher than the non-contingent bonus, and 14% higher than the performance review with the bonus made non-contingent. In all individual cases these trends were apparent. The changing criteria did not appear to have been necessary. Four managers completed a median of 97.5% of the non-priority jobs during the first bonus condition. Data from Cycle Three are reported here to facilitate comparisons, and because these transitional conditions are really in both cycles. The four managers who remained dropped to a median of 75% when the bonus was made non-contingent, and then returned to 95% when the bonus treatment was reintroduced. Manager 13 is not in the group medians because he was in fewer conditions and went through a different sequence of conditions. Two managers completed less than the five planned shifts in the reversal condition because their performance decreased immediately (as predicted) and we wanted to speed their progress through the experiment. When we added the performance review and the non-contingent bonus condition,

TABLE 4

Median Percent Non-priority Jobs Done by Individual Managers from Cycle Four

Manager	Condition (Number of Shifts)			
	Performance Review Plus Bonus	Non-contingent Bonus	Performance Review Plus Bonus	Performance Review Plus Non-contingent Bonus
10	95(5)*	77(4)	92(5)	82(2)
17	92(6)	70(3)	96(4)	75(2)
18**	100(5)	--	--	95(3)
22	100(5)	75(5)	93(5)	78(5)
23	100(5)	87(5)	100(5)	95(13)
Median for those in four conditions	97.5(21)	76(17)	94.5(19)	80(22)

\* The numbers in parentheses indicate the number of shifts for which data were taken.

\*\* These data are not included in the group median due to different number of conditions, and different sequence of conditions.

performance decreased and became more variable, with a median of 80%. Reliability was 91% (range 88% to 94%) for four observations; one observation in each condition. Reliability with the "outside" observer was 90% for non-priority jobs. More will be said of these results in the following chapter "Overall Evaluation".

All four managers left the co-op during this last condition. Two had stated earlier that they would be leaving town at that time. And the co-op was changing policy again, requiring managers who worked more than eight hours to assume more responsibility, so the other two managers chose not to remain under those conditions.

## OVERALL EVALUATION

Figures 1 and 2 present an overall evaluation of the effects of the performance review and bonus.<sup>1</sup>

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<sup>1</sup>We used the following considerations in determining the data to be analyzed in this overall evaluation:

We did not include data for people who terminated prior to the third cycle (subjects 2 thru 9), because they would not be able to provide any information about the effects of the bonus. And we did not include data for people from cycle three or four who did not contribute at least two data points from at least two conditions (subjects 19, 20, and 21), because they would not be able to provide any information about between-condition effects with a single subject. We also did not include data from the bonus condition for subjects 11 and 12, because the procedure at that time did not insure that the subjects would collect their earned bonus, and thus these two subjects did not make contact with the independent variable. As noted earlier, this procedure was changed before the other subjects entered the bonus condition.

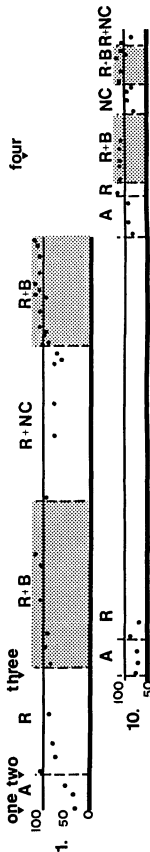
However, we did include data from the baseline and performance review for subjects 11, 12, and 15 even though they did not contribute data from a bonus condition; we did this because they had provided at least two data points for each of two conditions after the start of the third cycle, and thus it seemed undesirable to exclude reliable data obtained during the time that the bulk of the data for this final analysis was being collected--in other words, it seemed desirable to prevent a potential attrition bias during these last two conditions.

And we also included some data from the first and second cycles for manager 1 since this allowed us to start the bonus condition at the beginning of the third cycle and to avoid the repetition of two conditions for that subject. So for this subject, we included the last three data points from the baseline condition of the first cycle, and we also included the immediately-following, first-four data points from the performance review conditions of the third cycle; in that way the number of data points from these two conditions was comparable to the maximum number of data points allowed from those comparable conditions in the remaining cycles.

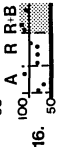
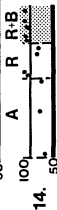
There was a sufficient amount of data on priority tasks in the combination of cycles three and four to allow for reliable assessments of the effects of the bonus and performance review, so these data are also included in this overall evaluation.

Figure 1. Percent non-priority jobs done per shift. There were 28-32 non-priority jobs. Data are included for each manager who completed at least two shifts in each of at least two conditions since the introduction of the bonus conditions on the 11th week (the start of Cycle Three). The horizontal solid lines represent the 87% criterion for non-priority jobs. Bonus conditions are shaded. The vertical arrows indicate the earliest date on the graph that a manager could be subject to the conditions of a new cycle. The numbers for managers correspond to the numbers on Tables 1 through 4. Week #1 for Figure 1 is the first week from which data were included in this overall evaluation, it is not the first week of the first cycle.





**PERCENT NON-PRIORITY JOBS DONE**



- A = BASELINE
- R = PERFORMANCE REVIEW
- R+B = PERFORMANCE REVIEW & BONUS
- R-NC = PERFORMANCE REVIEW & NONCONTINGENT BONUS
- NC = NONCONTINGENT BONUS


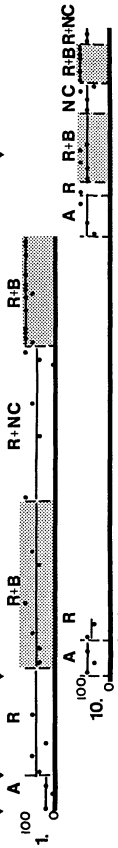


Figure 2. Percent priority jobs done per shift. There were four priority jobs. Data are included for each manager who completed two shifts in each of two conditions since the introduction of the bonus condition on the 11th week (the start of Cycle Three). No criterion line was used, as the criterion was 100% for priority jobs. Unlike Figure 1, the horizontal lines represent the median performance in each condition. Bonus conditions are shaded. The vertical arrows indicate the earliest date on the graph that a manager could be subject to the conditions of a new cycle. The numbers for managers correspond to the numbers on Tables 1 through 4. Week #1 for Figure 2 is the first week from which data were included in this overall evaluation, it is not the first week of the first cycle.

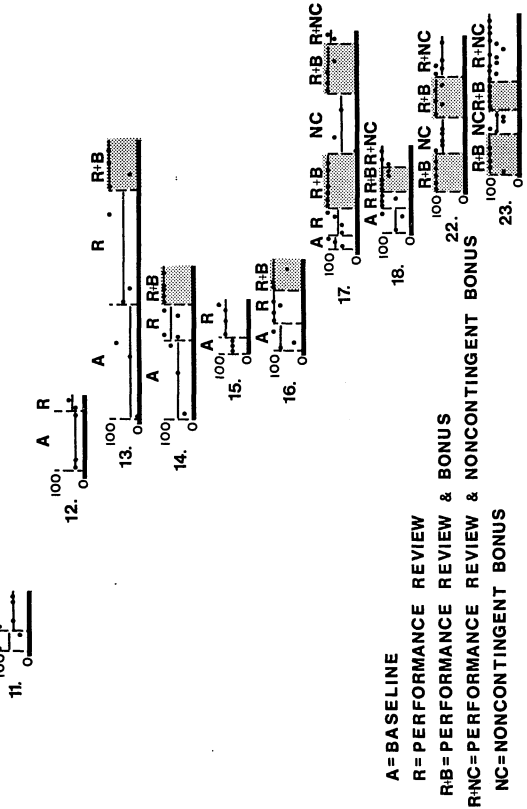
four

three

two



PERCENT PRIORITY JOBS DONE



- A=BASELINE
- R=PERFORMANCE REVIEW
- R+B=PERFORMANCE REVIEW & BONUS
- R+NC=PERFORMANCE REVIEW & NONCONTINGENT BONUS
- NC=NONCONTINGENT BONUS

1 11

WEEKS

Figure 1 shows that for non-priority jobs, managers generally performed above criterion levels (the horizontal solid lines) only in the bonus plus performance review condition (the shaded condition). It also shows how long each manager was in the study and in each condition, and when each condition was introduced relative to its introduction with other managers.

Figure 2 shows that for priority jobs managers generally performed best in the bonus plus performance review condition (the horizontal solid line depicts median performance in each condition)(the shaded condition). Like Figure 1, it also shows when each condition change occurred both in relation to individuals and to other managers.

As this is the first reporting of priority job data, reliability of observations should be mentioned. There were 37 reliability checks for non-priority and priority jobs during this study; 24 in the first cycle, five in the second, four in the third and four in the fourth cycle. There was always at least one observation in each condition of each cycle. The median reliability for priority job performance throughout the study was 100%. The reliability for priority jobs with the "outside" observer in Cycle Four was 100%.

Table 5 shows individual data from Figure 1. In all cases with non-priority jobs, the bonus plus performance review was higher than any other condition. In all but two cases, the performance review (with or without the non-contingent bonus) was higher than

TABLE 5  
 Median Percent Priority and Non-priority Jobs Done By  
 Individual Managers from Figure 1 and Figure 2

Manager	Baseline	Performance Review (R)	Condition			
			R + Bonus 1	Non Contingent Bonus	R + Bonus 2	R + Non-contingent Bonus
1*	32(25)**	71(50)	89(50)	--	92(100)	66(50)
10	67(75)***	75(75)***	95(75)	77(75)	92(100)	82(75)
11	80(75)	79(50)	--	--	--	--
12	62(25)	63(37.5)	--	--	--	--
13	63(25)	78(50)	92(100)	--	--	--
14	67(50)	84(75)	96(100)	--	--	--
15	72(50)	84(75)	--	--	--	--
16	70(75)	73(100)	96(100)	--	--	--
17	70(75)	87(62.5)	92(100)	70(50)	96(100)	75(87.5)
18	93(50)	91(100)	100(100)	--	--	95(100)

TABLE 5 (Continued)

Manager	Baseline	Performance Review (R)	Condition			
			R + Bonus 1	Non-Contingent Bonus	R + Bonus 2	R + Non-contingent Bonus
22	--	--	100(100)	75(75)	93(100)	78(75)
23	--	--	100(100)	87(75)	100(100)	95(100)

\*For this manager, conditions are not presented in the order in which they were implemented. He received the performance review and noncontingent bonus prior to reentering the performance review and bonus condition.

\*\*The numbers in parentheses indicate the percentage of priority jobs done by this manager in this condition.

\*\*\*These data include two separate Baseline (A), and Performance Review (R), conditions for this manager, who left the experiment and then reentered at a later date.

baseline (with or without the noncontingent bonus). Managers 10 and 18 showed slightly higher performance in baseline than in the performance review condition, but otherwise conformed to the group trend. With priority jobs, the bonus plus performance review was higher than any other condition with the exception of six individual instances of equality with a performance review condition (with or without the non-contingent bonus). These exceptions occurred in the first performance review plus bonus condition with managers 1, 10, 16, 18, and 23, and in the second performance review plus bonus with manager 23. Four of these six ties occurred because of perfect performance by managers in a performance review condition (managers 16, 18, and two ties with manager 23). In no case did individual performance in baseline equal or exceed that in the performance review plus bonus condition.

In all but five cases with priority jobs, the performance review was higher than baseline (with or without the non-contingent bonus). Three of these instances were ties (manager 10 with 2; manager 22), while in two cases managers performed better in a baseline condition than in the performance review (managers 11 and 17).

Figures 1 and 2 show high performance in the bonus condition, but they also reveal the invariably short work spans of managers. Perhaps we should not have expected managers to work at the co-op for prolonged periods with such stringent requirements and such low pay. The one manager who stayed through most of the experiment

(manager 1) was partially employed elsewhere. He had said that he worked at the co-op to develop social skills so that he might get a teaching job. At one point he contracted with the co-op to meet twelve social and grooming criteria or be suspended from his job. Keeping his job thus appeared reinforcing to him for reasons other than pay and other than the variables investigated. However, his performance was under the control of experimental variables, as Figures 1 and 2 show.

The last cycle demonstrated the superfluity of the seven-shift "apprenticeship". The required conditions of baseline and performance review gave us valuable information about job performance, but were not needed in training competent managers as we had once thought.

#### Limitations

The following are problems, discussed at various points in the text (including the conclusion section); problems that might limit the extent to which we can rely on the summary data.

1. Data were inconsistently obtained due to cashier absences. This created problems in that managers would sometimes work several shifts and yet contribute only a few data points (or no data points). Along with high turnover, cashier absences thus helped prevent managers from completing conditions as designed.
2. Some instructions were given vocally. For instance, we



never wrote down exactly when jobs had to be completed. The stated rule was that observations of job performance would take place during the last fifteen minutes of each shift. Early in the study this caused problems because managers would dispute the rule. The co-ordinators then brought it up at a co-op general meeting, and had the rule formally approved. After that there were no such disputes.

3. The changing criteria were rarely used. For example, even though this shaping procedure was in effect during the initial bonus condition for each manager, only three out of 44 possible times did a manager receive a bonus at less than the final criterion (100%) for priority jobs. Still, the question is raised as to whether the data obtained while the preliminary criteria were in effect are comparable to those obtained under final criteria conditions.
4. The turnover of managers was high. This interfered with detailed within-subject designs so that we could not do all of the comparisons we wanted. It may also make it unwise to generalize too hastily to voluntary settings with lower turnover.
5. Some managers received more base pay starting in Cycle Three. The \$.50 per hour more received by managers who worked at least eight hours per week could have affected

their absolute and relative levels of job performance.

6. We were never sure who completed the jobs. We could not determine whether the manager did a job, or whether he or she asked the cashier or a worker to do it and perhaps then checked to see that it was done.
7. The possibility of experimenter bias arose in many instances, as the experimenters were often present in the setting, they were the observers, and they delivered the independent variables.
8. Baseline and performance review conditions were not exactly replicated within-subjects due to the use of the non-contingent bonus in the reversals.
9. Baselines for some individuals were not stable before condition changes.

## POSTSCRIPT - CURRENT APPLICATIONS

Several problems remained at the conclusion of this study. If the system were to maintain, a) it would be more cost-effective to reduce turnover of managers, b) the co-op would have to provide some inducement for managers to assume more responsibility for critical tasks on the co-ordinator level, and c) the co-ordinators' observation of jobs would have to continue without the added incentive of university credit they received for some of their work on this study.

In response to those problems we made several improvements.

- a) Turnover of managers: We raised the base pay to \$2.00 per hour plus \$.75 per hour in bonuses, plus special bonuses. We arranged the special bonuses so as to equally distribute critical co-ordinator tasks and to pay each worker as close to \$3.00 per hour as possible. These changes more than doubled the pay for managers.
- b) Co-ordinator tasks: The increased pay was made contingent upon increased responsibility. We essentially changed the manager's job to include those tasks previously done by the co-ordinators. The special bonuses were applied to ordering and receiving goods, doing the books, and so on. The bonus for job performance remained in effect, and we added two more \$.25 per hour bonuses--the first additional bonus for doing important but irregular tasks like paying bills and handling mail.
- c) Observation maintenance: The second additional \$.25 per hour

bonus was for doing the observations of the previous shift's work-- including whether or not they made the observations of their previous shift (or at least recorded having done so). An experienced manager-auditor checked each of the other managers each month on at least one observation of all bonus activities to maintain accuracy of observations. The auditor received special bonuses for this job.

At the end of the present study there was also some dissatisfaction among the workers about various aspects of the performance system. A minority opposed the observation procedure, which was essential for both the performance review and the bonus. In effect, they preferred the baseline performance of 67% to the performance of 95% with the bonus plus performance review. They described the observations as "surveillance" and as demonstrating a "lack of trust." One dissenting person suggested as an alternative more selective hiring, in order to get trustworthy workers, and more firing of workers who did not perform "adequately," defining adequate performance intuitively. This person evidently thought it worse to evaluate a person's performance than to fire them on subjective grounds.

By this time no one objected to the bonus by itself. However, some people expressed discomfort with the social aspects of the performance review. Essentially they were saying that losing the bonus was enough punishment, and they could get corrective feedback by looking at the observation form--so why add personal criticism?

Anecdotal evidence cited earlier concerning managers leaving prior to the performance review may support the contention that the personal interaction in the performance review had aversive aspects.

So in the main we have eliminated the social aspects of the performance review, making it more like a standard visual feedback procedure. In other words, the people who do the crosscheck do not meet with the managers after the shift. They merely post the results--often after the previous shift managers have left. However, all social aspects are not absent, as the crosscheck is invariably started while the previous shift managers are present, and the crosscheckers are identified by name on the feedback sheet. This change combined with the peer-observation system appears to have considerably decreased countercontrolling measures against the performance system.

Eight months after implementation, performance remains very high in this peer-management system (very few bonuses have been lost for job performance and reliability remains over 80%), indicating that social interaction may not have been a necessary component of the performance review plus bonus condition. The cost of wages has increased slightly as a percent of sales, but sales have increased so much over fixed expenses that net income has improved. Also, turnover has decreased markedly, with only one manager leaving since implementation, and none of the seven full or part-time managers have left in the last seven of the eight months since implementation.

Pay has not stifled the spirit of service or co-operation in the co-op. Service in terms of goods delivered at low cost has increased. Workers co-operate extensively in completing jobs and monitoring each other. Customers seem to be more satisfied and often volunteer to help the co-op on special projects as well as regular shift work at \$.50 per hour and packaging work at \$1.00 per hour. More workers than ever are assuming critical responsibilities, which has led to a more equitable distribution of power. Perhaps those who opposed pay and specialization do not volunteer as often at the food co-op. But pay and specialization, although conspicuous in institutions some would like to change, may not be the features they should oppose.

## GENERAL DISCUSSION

The performance review generally increased job performance over baseline levels, but did not sustain criterion levels. This held true whether the performance review preceded or followed conditions in which a bonus was added to the performance review. The performance review plus bonus succeeded in maintaining criterion performance for all managers (and an eight-month follow-up indicates that the bonus even without a performance review maintained criterion performance).

In a setting such as this new co-op, any threat to its "voluntary" status is a threat to its existence. Each introduction of an obvious contingency in the present study met with opposition from workers or other co-op members. Although the performance review met the main requirement of "voluntariness," namely, the absence of money, it was an obvious contingency in other respects. The performance review was regularly scheduled and was made explicitly contingent upon performance other than just being present. It also brought much attention to the previously obscure observation system, a critical component of each intervention.

The managers, though not the general membership, opposed the use of the performance review after it was first introduced. Opposition to the performance review decreased considerably after three of the four initial managers left. Perhaps opposition to the use of systematic social consequences would not have been so great if

it had been an integral part of the co-op setting since its founding. Nineteenth century communistic societies such as Oneida may have had this advantage (Nordhoff, 1966). Criticism has been widely used and accepted in the Peoples Republic of China since at least the late 1950's, whereas immediately after liberation in 1948 it was not so easily implemented (Hinton, 1967). In the present setting, opposition to the performance review was not as vehement after it had become standard procedure.

Several potential problems arose because the experimenters, the observers and the co-ordinators were the same.

The observers might have been biased in their recording of the results, since they were not blind to the experimental conditions or their rationale. However the item by item agreement required for the reliability measures may indicate that such a bias did not occur. It would be very improbable that two independent observers (observers not looking at each other's recordings at the time of recording) would maintain the high reliability obtained in this study if they had simply increased or decreased the percentage of correct or incorrect items completed as a function of mutual observer bias.

Also, the observers' presence as such might have differentially affected the managers' and workers' performance in the various conditions. But, if observer presence were an independent variable, it is likely to have added a constant to all conditions, that is, if observer presence improved performance in the performance review



condition, it likely improved performance equally as much in baseline, and in the other conditions.

Additionally, there may have been a loss of generality to a non-experimental maintenance system, but probably none occurred because formal observation is a necessary part of such a system.

The possibility of experimenter influence also exists. Perhaps the co-ordinators somehow differentially reinforced better performance in each successive condition from baseline to performance review to performance review plus bonus. It is best to use people "blind" to the experimental conditions and their rationale to administer the independent variables. However there is some reason to suspect that the present experimenters did not introduce systematic variation into the data. The experimenters did not include the bonus in the original design. If they were intent upon using their influence as co-ordinators, or somehow delivering more social reinforcement during experimental conditions, it seems plausible that they would have done so during the performance review condition. That is, the original hypothesis was that the performance review alone would improve performance up to criterion levels. It would therefore have been in the experimenters' "best interest" (as opposed to the interest of science and society) to favorably influence the performance review condition so that criterion level was attained. However, criterion level was not attained. So even if the experimenters had tried, they might not have been able to have any more contaminating

influence in subsequent bonus conditions.

Experimenter presence in itself might have affected the performance of managers, separate from their role as observers or as deliverers of the independent variable. However, just as with the presence of observers, experimenter presence was a constant, and per se would probably not differentially affect performance in a given condition.

In all of the cases of potential experimenter bias (as opposed to observer bias), the following argument holds. If a functional relationship was demonstrated, it may have occurred due to experimenter influence, and not to the independent variable as described in the design; but it occurred nonetheless. As with all experimental results, it remains to replicate them externally--in several similar systems and settings. Then, if the independent variable as described and replicated does not produce the predicted effect, we may suspect some extraneous variable that was presented along with the original intervention. However, it would have improved the present study to have utilized independent observations of experimenter-observer bias and of the experimenters as deliverers of the independent variables.

The high turnover rate of managers throughout this experiment raises some questions: 1) Did the experimental procedures influence the turnover rate, and if so, how does this affect the interpretation of the results? 2) Does the fact that no individual received all experimental conditions weaken the conclusions made? 3) Can the results be generalized to systems with less turnover?

1. While no data were taken on turnover prior to the experiment, it is possible that turnover rate did increase as a result of the experiment (as opposed to the planned independent variables in the experiment). This is particularly evidenced by the managers quitting in Cycle One, because, according to them, of the introduction of the performance review and the fact that they were being observed. This could be interpreted as a "reverse Hawthorne effect" in that subjects responded adversely to being in an experiment. The experimenters had hoped that since the co-op was recently relocated, that its sales would increase sufficiently to pay managers and cashiers enough to keep them on the job. These hoped-for raises never materialized (other than the raise from \$.75 to \$1.00). Perhaps we expected turnover to be diminished or at least be unaffected by the introduction of the performance review. At any rate, Cycle One could have been better designed and eventually was. This is the nature of the systems approach.
2. Any time results are not replicated with a within-subject design, the possibility arises that individual differences are responsible for observed differences in behavior. In the current experiment, no condition was presented to all managers. However, each effect was replicated either within or across managers. The within-subject demonstration

of the relative effectiveness of the performance review plus bonus versus the non-contingent bonus was the strongest--as it was a reversal design. The other within-subject comparisons were not reversals. However, the various comparisons were repeated with regularity across several subjects each using a multiple baseline design to minimize confounding by coincidental variables whether they be time or subject related. For instance, from Table 5 94% (34/36) non-priority data points conformed to the direction of their respective group median changes from one condition to another.

3. The overall effect of experimental conditions on turnover and hence on job performance does not affect the internal validity of the results. It always remains to replicate experimental results widely in similar systems to externally validate them. Only in this way can we say whether or not "experimentation" or some other unsuspected variable contributed to the turnover or other results in the particular experiment.

The present results are consistent with studies in applied behavior analysis showing improvements in voluntary settings with the addition of small extrinsic consequences. Comparisons of obvious, material forms of reward with social consequences were not made in these previous studies. However, systematically delivered social reinforcement and punishment may have been more

cost effective in some settings where token or monetary reinforcement was eventually used. Or token reinforcement may have worked better and faster than social reinforcement, perhaps in a more cost-effective manner.

We are interested in comparative analyses for purposes of improving prediction and control. But we also might compare the costs of two procedures to weigh these against their benefits. In the current study we demonstrated a bonus procedure, costing about \$12.00 per week, that resulted in an improvement in non-priority job performance of about 13%, and in priority jobs of 25% (from Table 5: conditions with the bonus versus both those with the performance review and no contingent bonus). The net gain in the bonus condition was one additional priority job done per shift, and four additional non-priority jobs done per shift. There is good reason to argue that the opportunity cost of not completing the one additional priority job and the approximately four additional non-priority jobs was considerably greater than the cost of the bonus. For example, a common priority job not completed under the performance review alone was restocking the dairy cooler. If one forty-pound block of cheese with a retail value of \$80.00 was not stocked during a week in which it would have sold, the actual cost to the co-op would be 25% (the gross margin) of \$80.00 or \$20.00--already more than the cost of the bonus. Similarly but perhaps less obviously, the cost of losing customers due to uncleanliness could be very great. If

the average customer buys \$4.00 worth of food per week (at the same 25% margin) from the co-op, we would be paying for the four non-priority jobs (usually the hardest ones--dusting bins, cleaning dispensers and coolers, sweeping) if we did not lose 12 out of our average 1200 customers per week (a loss of 1%). Comments from ex-customers indicate that many did not like the level of sanitation in the co-op. We suspect that we would lose more than 1% of our customers if we left an additional four cleaning jobs undone each shift. We consider the above figures to be underestimates.

A majority of co-op members approved the continuation of the bonus system when these benefits and costs were revealed. But the multiple baseline design prevented analyses of actual dollar sales or number of customer differences among conditions, because in any given week some managers were in the bonus condition performing well, and other managers were in less optimal conditions.

By no means do these results show conclusively that social consequences are less cost-effective than monetary ones in all voluntary settings. For one thing, we did not directly contrast the bonus alone with the performance review alone (although the follow-up approximated such a comparison). For another, social consequences can vary considerably from one voluntary setting to another. Individual histories of social reinforcement with respect to voluntarism and co-operation in general may make a considerable difference, and social variables outside the setting may also make a considerable difference. Doubtless in a highly

co-operative society we would prefer using social consequences to using systems involving monetary rewards with their associated costly delivery mechanisms.

Nevertheless, this study may have implications for voluntary settings other than the food co-op. It would be interesting to explore the effectiveness and feasibility of adding small extrinsic consequences in settings such as: hospitals, boy scouts, political groups, university departmental participation of faculty, civic groups, and fraternities. Within those settings we might study the effects of adding small extrinsic consequences and/or social consequences contingent upon behaviors such as the following: meeting attendance, committee participation, paper drives, visiting residents, fund raising, and distributing literature.

It could be that, ironic as it may seem, making the setting less voluntary is the solution. The food co-op became less voluntary when food credit pay increased from \$.75 to \$1.25 per hour, and then, by the time turnover was no longer a problem, pay was up to about \$3.00 per hour and the managers were no longer volunteers.

On the other hand, many behaviors in many settings may remain voluntary. That is, they could be controlled by social consequences. It might be culturally advantageous to have as many behaviors as possible under the control of voluntary consequences.

The staff-management literature in non-voluntary settings, in the standard, applied behavior analysis literature, shows a similar absence of comparisons of social consequences with obvious, material

consequences. Many studies compared written feedback with monetary consequences, but written feedback contains a minimal and often unspecified social component. One study (Pomerleau, Bobrove, and Smith, 1973) compared "supervision" plus a bonus with a bonus alone, but supervision appeared to include mostly social antecedents (modelling, instruction) not consequences. Other studies used treatment packages; that is, combinations of social consequences and one or more additional independent variables, but did not attempt to separate effects (Pommer and Streedbeck, 1974; Iwata, Bailey, Brown, Foshee, and Alpern, 1976).

Clearly, more work needs to be done in this socially important area. And this work needn't wait for the U. S. culture to become highly co-operative. Small living and working settings with special populations, such as group living and the food co-op, may be used to study the contribution of social and monetary rewards to the maintenance of behavior.

The behavioral systems approach made explicit the stages that many experiments in applied behavior analysis go through in attempts to gain control over the dependent variables. This approach is often preferable to one which relies on elaborate planning and later statistical analysis, as events in applied settings are often unpredictable (Michael, 1974).

Similarly, the behavioral systems approach may be preferable to one which employs pilot studies in an artificial setting. Often only by implementing variables in the setting of interest can we



assess their final effects. The setting contributes variables which have effects of their own and which interact with experimentally introduced variables. Also, it is best to avoid pilot studies which employ less than adequate numbers of subjects (particularly in studies using group designs), poor reliability procedures, different independent variables than the ones to be used later, and so on. Data from such pilots are supposed to guide future research and yet suffer from inadequacies that made them unacceptable for sound research in the first place (Sidman, 1960). Again, we should plan our applied research, and the plan should include recycling.

The present study included a time series design (Cycle One) with other features added as their use became necessary or opportune. The first cycle, Management of Managers, involved a time series analysis of the performance review which demonstrated a substantial, although unsatisfactory, increase in job performance for those four managers. The design subsequently became a multiple baseline across subjects, not so much because the time series had proved inadequate in separating the effects of the performance review from those of baseline conditions, but because turnover of managers necessitated the second design. Later, we planned the reversal because we did not expect the bonus to have an obvious incremental effect over the performance review with all managers. We felt it would be possible to implement the reversal using the "free bonus" (non-contingent bonus) and the individualized condition

durations that co-ordinators discussed personally with each manager. Further, experimental evaluation of the effects of the bonus with no performance review was not attempted due to time limitations. Hopefully, later applications in this or a similar setting will isolate the effects of the bonus.

So the behavioral systems approach included methods of assessing the relative contributions of several major independent variables. In this case, the evaluation evolved out of necessity and opportunity into a design with replications both within and across subjects. Rigorous observation combined with an understanding of the principles of behavior and reasonable control over the dependent variables are the essentials of a systems approach. This study demonstrated that such an approach can be used in a setting in which the clients participate in design, evaluation, and redesign.

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## APPENDICES



## APPENDIX A

### WHAT TO AND HOW TO IN THE PEOPLES CO-OP

#### Job Performance

This is intended to supplement the daily job checklist. If a job does not need doing, it should be marked complete in the M or manager column. This goes for jobs that can't be done, too; if a necessary supply is missing or if there is a physical obstacle to performance, for example.

Jobs that are not detailed here are thought to be self-explanatory.

#### Health Regulations

1. Put belongings in the basement--there are hooks to hang your coats on and shelves for other belongings.
2. Apron on--you will find aprons in the basement, too. And put it back in the basement when you're done.
3. Tie hair back. Tie it back if it reaches your shoulders. Use hair band or hair net if desired. This is so it won't fall into the food and gross people out.
4. Wash hands. Use the kitchen hand sink. Use soap.
5. No smoking. It's a fire hazard, the ashes contaminate food, odor destroys delicate flavors, and it is dangerous to everyone's health.

#### General and Housekeeping

6. Complete work record (all jobs ✓ or X). List names of workers--those signed and not signed, hours signed, and hours present. Mark all jobs complete or incomplete according to these descriptions. Check the list as you go. Start filling out the form at the beginning of the shift. Let the workers use the form to see who is to do what, and to see what's been done so far in the shift.
7. "Open-closed" sign turned at opening and closing. The one on the screen door at the front of the store.

8. Clean check-out area. There should be an on-site description for this one (and for many other of these jobs). Throw away loose papers. Wipe the scale with a sponge dipped in a chlorine solution. For chlorine solution use at least 9 parts water to 1 part chlorine bleach. This is our sanitizer. If it's not by the sink it should be in the broom closet in the southwest corner of the very back room. Wipe the counter off, too, as needed. But don't drop the dirt on the floor--catch it and put it in the waste basket. There should be no visible food or trash or dust on the counter. Sweep the floor around here on both sides of the check-out counter. There should be no garbage or trash on the floor (dust is O.K. in this area because it's so busy).
9. Clean dispenser area. Clean as you go (whenever you make a mess). The table and floor must be clean to sight and touch at all times. If you see any food outside of its proper dispenser, wipe it up, off the lids, off the table, off the floor. Use a sponge and warm soapy water (in an empty peanut butter tub, for instance). Rinse and sanitize the table (using that 9+water/1 chlorine bleach solution).
10. Shelves, bottles, and bins dusted in the front room. Don't worry about the kitchen, the bakers are supposed to do the shelves in there. Besides, this is the toughest job on the list. Use a damp sponge from the kitchen. Don't forget the tops of jars and bottom shelves. Don't forget the tops and lids of the bins. Do it 'till you can't get an obvious streak with your finger.
- 11A. Shelves of produce cooler cleaned Tues., Thurs., Sat. Remove the food from a section. Lift the shelf. Wipe the top of the shelf and under it with a sponge dipped in warm soapy water. Rinse. Sanitize with the 9+water/1 chlorine bleach sanitizer. Replace shelves. Then replace the food, filling the cooler to capacity. Finish by wiping off the out-side and the top of the cooler.
- B. Reach-in cooler cleaned Tues., Thurs, Sat. Remove the food from a shelf. Wash the shelf and the sides of the cooler with the warm soapy water. Rinse. Sanitize with 9+water/1 chlorine bleach solution. Replace food and fill to capacity. When all the shelves are done, do the bottom of the cooler. Empty the pitcher. Replace it. Finish this job by washing the outside and top of the cooler.
- C. Bathroom cleaned Mon., Wed., Fri. Put bowl cleaner into the toilet. While waiting for that to work, use cleanser and a sponge to clean the sink. The cleanser is in the corner closet next to the bathroom. Wipe off the mirror if it's

dirty or spotted. Wipe off the tank, the toilet seat, the top of the bowl. Rinse the sponge thoroughly. Then sanitize the sponge using the 9+water/1 part chlorine bleach solution. Then take the toilet bowl brush from the corner behind the toilet, and wipe the inside of the toilet bowl. Put the brush back in the corner. Empty the waste paper basket into the green dumpster outside. You'll find a broom in the closet next to the bathroom. Sweep the floor. There should be no visible dust, trash, or garbage. Put the broom back when you're done and return the waste paper basket to the bathroom.

12. Floor in front swept. The co-op should be kept as clean and nice looking as possible. It is more attractive to members, nicer to work in, and healthier. Besides, it's your store and it follows that you should want to keep it clean. The peoples broom can be found in the peoples broom and mop closet in the southwest corner of the back room. It is best to use the small broom first to sweep around and under the bins and shelves, and into the corners before bringing the larger broom into play. Be sure to get the flour. Anything smaller than a spice bag (# 1/2, 2 inches by 4 inches) should be swept and put in the trash. Anything larger than a spice bag should be picked up, separated and put in the compost or trash. No flour dust on the floor should be visible from the customer side of the counter. There should be no visible garbage or trash.
13. Floor of back room swept. See above paragraph. Get close to the pallets. There should be no visible dust, garbage, or trash.
14. Floor of walk-in cooler swept. See paragraph # 12. Especially get the wheat germ swept up. Pick up pieces of food. Everything except milk should be on the shelves. Milk stays on the floor. There should be no visible dust, garbage, or trash. Wipe up water.
15. Food garbage in compost and trash in bin. The compost station is the grey pail with a white lid and handle marked "COMPOST" near the hand sink in the kitchen. Only food garbage should be put here. Always cover the compost pail after using it. Any other trash that is not recyclable food or heavy paper should be put in the trash bin next to the compost pail.
16. Full (within 2 inches of the top) compost and trash out. Take them to the big green dumpster. Do this by the end of the shift. We only throw the compost away when no one will recycle it.

17. Bags, cardboard boxes, crates, and can (except honey and oil) stacked by the back door. We want them to go outside when enough have accumulated (see # 18 below). Put the honey and oil cans downstairs in the room with the outside door. We send these back to the supplier.
18. After 6 items take to back fence (except take crates and cans other than honey and oil to the dumpster--we can't recycle them with the cardboard). This is so the pile by the back door doesn't get too high. Flatten the boxes and separate the crates from the boxes. There should never be 7 items by the back door. Take all items out at the end of each shift no matter how many or few have accumulated.
19. Clean sink and contents. Leave nothing except sponges in the sink. Wash everything, starting with the counter, including the P.B. tubs. Wash everything in hot soapy water. Put washed items in rinse water. Empty soapy water and fill sink (or P.B. tub--rotating solution to next tub for reuse) with 9+parts water/1 part chlorine bleach solution. Now sanitize the rinsed items in the solution. Then set to air dry, by the sink if possible. Put things away when they're dry. You are not responsible for cleaning something that is obviously a baking item (such as baking pans, mixing bowls, measuring cups, pots). Get the P.B. tubs, jars, spoons, and bagging utensils for sure.
20. Nothing left on the wooden tables in the kitchen. This includes the bakery table. Don't even put mail on it.

Customer Service and Stocking (includes dispensers)

21. All customers waited on--fill order and weigh it. No customer should be unattended if a worker is not busy waiting on somebody else. Only emergency or priority jobs should take precedence over waiting on a customer. Finish what you're doing later if at all possible. Ask the customer if you can get something. Suggest things.
22. Empty containers cleaned. This has to be done because in the past food has become infested with worms when bins were not cleaned. For wooden bins use a damp sponge. Wipe down the inside with the sanitizer (9+parts water/1 part chlorine bleach). Put the new bag next to the drying bin so that the food may still be sold. For the dispensers wash them and dip them in the chlorine solution. Also let air dry.
23. Empty containers refilled if stock available. Change the price on the price board. Use the small green chalkboard to note price changes if you don't have the time right away to

change the price board.

You know if the stock is available by looking at the out-of-stock list for the item whose label is on the empty bin. If a bin or container is empty and the item is not on the out-of-stock (OS) list, it should be available in back. If not see job # 24.

To find out where an item is in back, refer to the Main Supply List which should be near to the OS list. Or ask someone. Or hunt.

If you take the last of anything from the back room or down-stairs, put it on the out-of-stock list.

#### How to Stock:

a. The easiest way to stock the binned items is to take the sack and drape it over the edge of the bin (once the clean-bin is dry). Get help or use the industrial dolly if the sack is too heavy for you. Then cut the end of the strings binding the top of the sack and pull or cut the string and the sealing strip off. Carefully dump the contents of the sack into the bin. Then put the bag by the back, or take it outside if more than 6 items have accumulated by the door.

BE SURE THAT YOU HAVE DRAPED THE SACK OVER THE PROPER BIN BEFORE CUTTING IT OPEN.

- b. Stocking dispenser items is also an art. Follow the guidelines for bins for cleaning and restocking. But there are some differences. Don't try to restock honey if you can't lift 60 pounds very high. Have someone help you. When pouring liquid from one container into another, remember to allow for the Coanda effect. This is the physical property of liquids that makes them stick to the sides of containers rather than pour neatly out of them. Be careful.
24. Out-of-stock items put on OS (out-of-stock) list by door to bakery. You are not responsible to know everything that's in or out of stock. If, however, there is an empty bin, and the item is not on the OS list, you are responsible for finding that stock or for writing that item down on the OS list. Note other items that are already on the list so you don't put an item down twice in a week.

Don't forget the dates for items that run out entirely in front so that we can get turnover rates. If we know that

something ran out 3 days after we got it in, we should probably order twice as much for the next 6 days.)

25. Tops on all containers and dispensers when they're not in use. This keeps foods from mingling. We don't want any unauthorized mingling going on! Also sometimes the top is the only identification for the food for a naive worker (what does Tahini look like, anyway? Ta-what? etc.)
26. All bags on flats (pallets, wooden things on the floor). We need to sweep under there. Also it protects the bags from possible water damage. As on the main supply list, note that most grains, flours, seeds and other 60-100 lb. bags are stored on the pallets in the back room. Perishable things like wheat germ go in the walk-in cooler on the shelves. Beans, oats, and other 25-59 lb. bags go in the basement. Except no open bags are kept in the basement no matter what they weigh, they are put upstairs on the shelves to the left of the back room. The heavy things are upstairs so we don't have to carry them so far.
27. Baked goods stocked to capacity. No less than 2 of an item if stock available. All baked items that should be available should be on the stock list near the baked goods in front. If an item becomes unavailable it should be crossed off the list. You should proceed something like this:
  1. Look at the list of items that should be available.
  2. See if all of those items are on the shelf.
  3. If a listed item is not on the shelf, or if there are 2 or less of that item, check on the racks in the bakery to see if it's there. Restock to capacity if it's there.
  4. If it's not in stock, cross it off the list.
  5. If you take the last of anything from the back, cross it off the list. Out-of-stock items should be crossed off so others won't have to look in the back for them.
28. X bakery item off list if not available in kitchen. See above paragraph, numbers 4 and 5.
29. Reach-in cooler stocked to capacity--no less than 2 of an item if stock available. All reach-in items that should be available should be listed on the door of the reach-in. You should proceed something like this:
  1. Look at the list of items.
  2. See if all those items are in the reach-in.
  3. If a listed item is not there, or if there are 2 or less of an item, check the OS list.

4. If it's not on OS check the walk-in for it. Restock to capacity if it's there.
  5. If it's not in stock, put it on the out-of-stock list and date it (or, in the case of a stock list on the door, like the one for flavors of yogurt, cross the missing flavor off the list and date it).
  6. If you take the last of anything from the walk-in, put it on OS or cross it off the stock list. ROTATE STOCK.
30. Above (reach-in) items on OS list. See above paragraph, numbers 5 and 6.
31. Produce stocked to capacity. No less than 2 of an item if stock available. All produce that should be available should have a sign somewhere on the produce cooler, or on the table by the door to the kitchen for less perishables like potatoes or onions. You should proceed something like this:
1. Look at the signs for what should be available.
  2. See if all those items are on the shelf.
  3. If an item with a sign is not on the shelf or if there are 2 or less of that item, check the out-of-stock list.
  4. If it's not on out-of-stock, check the walk-in (and the back room for less-perishables) for it. Restock to capacity if it's there.
  5. If it's not in stock, put it on the out-of-stock list.
  6. If you take the last of anything put it on the out-of-stock list. This will save others the trouble of looking in the walk-in.
32. Above (produce) out-of-stock items on OS list. See numbers 5 and 6.
33. Cheese stocked to capacity--no less than 2 of an item if stock available. All cheeses that should be available should be listed on a stock list by the cooler. If a type of cheese becomes unavailable it should be crossed off the list. You should proceed something like this:
1. Look at the list of items that should be available.
  2. See if all those items are on the shelf.
  3. If a listed item is not on the shelf, or if there are 2 or less of that item, check on the shelves in the walk-in to see if it's there. Look for already packaged cheese first, then find unpackaged cheese (the ones with the oldest date first) and package it.
  4. If it's not in stock, cross it off the list and date it.
  5. If you take the last of anything from the back, cross it off the list. ROTATE STOCK.

34. X cheeses off list if not available in walk-in. See above paragraph, numbers 4 and 5.
35. X dried fruits, nuts, and seeds off list if not available in walk-in. See instructions for crossing cheeses off the list.
36. All packaged and self-service items rotated (oldest dates in front and on top). Do this even with eggs and milk in spite of the fact that you have to lift the old out, put the new in, then put the old back in.
37. Cutting and bagging things cleaned and on labeled shelf. There should be a list on or near the shelf of what these things are. There are even marked places to put certain things. Clean things according to the washing instructions on the sink. Clean the shelf, too. No crumbs.
38. Work tables sanitized after packaging done. Take a sponge and dip it in the sanitizer (9+ parts water/1 part chlorine bleach). Wipe the table thoroughly, let it air dry. The tables should be clean to sight and touch.



APPENDIX B  
MANAGER DUTIES

1. Say something nice to workers who are doing a job right, who are improving, or who are working but screwing up (but tell them how to do it right).

The idea behind this job is simply to encourage work in the co-op. We need efficient work to make the co-op an economic success. We do not want to take good work for granted. We can't expect the worker to continue to do things right when there is no encouragement. We also should not encourage everything a worker does, including goofing off. We can show that we really care for the workers as people by helping them to help others. This maximizes the total amount of caring. We are assuming that we are helping others by doing good, hard work in the co-op.

There may be more problems with this job than we think. How many of us were taught to be really nice, appreciative, and yet discriminating? Either we are nice indiscriminately or we are a hard ass. Right? Wrong. Anyway, be nice to people when they're nice to YOU (when they're working co-operatively).

Although we are saying that the manager should say something nice to a worker who is screwing up, this does not mean that the manager should continue to be encouraging if the worker does not eventually improve. The worker in this case is exerting energy but is not helping anybody. If a worker does not improve after attempts to help by the manager, then we may have to go through our grievance procedure which allows the worker to explain at a meeting why he or she should continue to work at the co-op. The by-laws state that if the manager and the co-ordinator each ask a worker twice to do something and the worker does not comply, then the worker may be asked to not work until the meeting approves it. In the past this has not happened until after many alternatives were tried, such as finding the easiest and most enjoyable jobs for the worker to do, and training the worker better to do them right. These alternatives may be attempted at the manager's and co-ordinator's discretion.

Here are some examples that may help:

Example # 1. Rita Restock

Situation: The worker is walking past with a crate of celery on the way to the produce cooler.

Manager says: "That's really great, Rita. Restocking is about the highest priority job in the co-op."

Manager doesn't say: "Rita, how about restocking the dairy cooler as soon as you get a chance?"

Why? It's not really encouraging to give a person more to do like that. If you must tell a worker what to do when they're already busy, at least tell them they're doing a good job first. Chances are that later a worker will be looking for something to do and then you can assign the dairy cooler.

Manager doesn't say: "Rita, are you the meter maid down at the ISB Building on weekends, etc., etc. B.S.. B.S.?"

Why? It's encouraging idleness.

#### Example # 2. Lerna Lerna

Situation: The worker writes down the price of the item on the bag, but forgets to put the price of the bag on the bag so that the cashier will remember to add it in. Last time you saw the worker filling an order, not even the price was on the bag.

Manager says: "Hey, Lerna, that was good to put the price of the item on the bag. Next time, if you remember to put the price of the bag on there, too, like this, then the cashier will be sure to add it in."

Manager doesn't say: "That's the second time I've asked you to put the prices on the bag--come to the meeting and defend your job."

Why? It's punishing.

Manager doesn't say: "That's far out, perfect!"

Why? It's encouraging incorrect work.

#### Example # 3. Goop

Situation: The worker is packaging cheese and is all wrapped up in the Reynolds film. Nothing seems to be going right.

Manager says: "Goop, you've got the cheese cut to the right size and the label is right and it's on the cheese ready to go. Good. Now, let's see about this here problem with the wrapping."

Manager doesn't say: "Let's deal with your plastic psychosis."

Why? There's always something someone is doing right when they're working--if only trying. Build on strengths. Don't emphasize weaknesses.

Manager doesn't say: "Ha! That's hilarious! That reminds me of Woody Allen as a plastic tit."

Why? Encourages incorrect work.

## 2. Ask idle worker to do a specific job.

An "idle" worker is one who is not doing a job on the work form or a job that the manager or co-ordinator has specifically assigned or approved.

This includes workers who are talking too much or walking around as well as those who are just standing.

Again, the assumption behind this manager job is that the best way for workers to serve the people is to contribute to the economic success of the co-op.

Some managers may have difficulty asking workers to do a specific job. They do not like to "give orders" to people. First of all, managers should understand that being nice to people is supposedly what the co-op is all about. Being nice to people involves building an economy in which all people can be happy. Our assumption is that this requires economic success, which requires hard work, being on time, being efficient, and occasionally following orders.

If a particular worker's idea of an ideal economy is that people should rap at length with each other rather than do more immediately productive labor, then this argument should be dealt with: If customers want to talk about politics this is great and should be encouraged. Ask them to come to the political meeting before the general meeting. Ask them to come to the political meeting and the general meeting. Ask them to meet you after work. The same goes for customers who want to talk about nutrition or any other beneficial topic. But don't rap to them for more than a couple of minutes while you're working at the co-op if this interferes with your work. Don't talk to other workers or managers about topics other than work if this interferes with your work. Also don't gossip if this interferes with your work. Leisure time depends upon economic success. True, ultimate economic success depends upon correct politics. But political discussion (as opposed to political action) should interfere with the process of production only in emergencies, like a cultural revolution, for instance.

In as nice a way as possible, managers should ask idle workers to do a specific job. Here are some examples:

Example # 1. Idle Idele

Situation: The worker has hands washed, apron on, hair back, and isn't smoking. Worker is standing next to the bakery table, rocking from heel to toe, singing "Taking Care of Business" by Bockman Turner Overdrive.

Manager says: "Howdy, Idele. How would you like to job ? Do you know how to do that?"

Manager doesn't say: "Idele, why are you standing around?"

Why? It's punishing and doesn't tell the worker what to do.

Manager doesn't say: "Idele, you're right on."

Why? It's encouraging idleness.

Potential Problems: The worker may not know how to do any of the jobs in the co-op. If the worker tells you this, refer the worker to the work form to get a general idea of what-all needs to be done. Try to assign the worker to a job that is presently being done by an experienced worker. Also refer the worker to this job description booklet or to on-site cards.

Example # 2. Mao Tse Tung Mike

Situation: The worker is talking to a customer about how the co-op's voting system has some advantages and some disadvantages compared with the Revolutionary Committee system in Peoples China. The customer is leaning-hand on the door to the dairy cooler-and is obviously impressed with the worker and the co-op.

Manager says: "Excuse me, Mao. That's a good topic to bring up at the meeting on Thursday. But would you please job as soon as you can?"

Manager doesn't say: "You are not to talk while working at the Food Co-op."

Why? It's punishing and doesn't tell the worker what to do.

Manager doesn't say: "Yeah, but the Revolutionary Committee system depends upon party members being too many places at once. The differential voting system allows the party to exert influence without being a stranger to the business. As Vice-Chairperson Frazier says, Know the Theory, Know the Setting...etc., etc., B.S. B.S."

Why? It's encouraging idleness.

Potential Problems: The manager may not know the worker's name. In that case just ask. Also, the worker may have finished a shift, and is not officially working. In that case, ask the worker to turn in the outfit. No work, no outfit.

Example # 3. Olympic Walker

Situation: The worker moves swiftly past with a deliberate pace. To the discerning eye of the Tuesday Second Shift Manager of the Peoples Food Co-op, however, there seems to be particular purpose to the worker's movements.

Manager says: "Can I talk to you for a second, Olympic? Are you doing something on the work form that needs doing? Are you doing something that the co-ordinator asked you to do? O.K., why don't you do job?"

Manager doesn't say: "You're like a chicken with its head cut off, Olympic, do job."

Why? It's punishing. There are better ways to tell the worker what to do.

Manager doesn't say: "Good luck in Zurich!"

Why? It's encouraging idleness.

3. Work when all workers are doing jobs right.

The idea here is that you are a worker as well as a helper of workers.