Chapter 4

Quasi-experimental and experimental designs: more powerful evaluation designs

4.1 Introduction

In Chapter 3 we described the simplest type of evaluation design for intervention effectiveness evaluation, the before-and-after or pre-post design. We showed how its strength is inherently limited by several threats to internal validity.

In this chapter, we discuss several types of quasi-experimental and experimental designs. All offer some advantages over the simple before-and-after design, because some of the threats to internal validity are eliminated. In the first section we show how a quasi-experimental design evolves from the addition of one or more design elements to a before-and-after design. After this, we describe experimental designs. Although the latter offer the greatest strength of evidence, quasi-experimental designs are often more feasible in workplace situations. We close this chapter with the discussion of various threats to internal validity that arise with a control or comparison group.

4.2 Quasi-experimental designs

Design strategies which change a before-and-after design into a quasi-experimental design
Strategy 1: add a control group
Strategy 2: take more measurements before and after the intervention implementation
Strategy 3: stagger the introduction of the intervention among groups
Strategy 4: add a reversal of the intervention
Strategy 5: use additional outcome measures

There are five basic strategies to improving upon a before-and-after design. This section describes common approaches to adopting one or more of these strategies.

4.2.1 Strategy #1: Add a control group (e.g., pre-post with non-randomized control)

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O O

The *pre-post with non-randomized control design* mimics a simple experimental design. Like the experimental design, there is at least one group which receives the intervention (intervention group) and one group which does not (control group)[11]. The difference lies in the way participants are assigned to groups for the purpose of intervention implementation and evaluation. In an experiment participants are randomly assigned;[12] in quasi-experimental designs, they are not. Often assignment of participants to a group is predetermined by the work organization. For example, you might deliver an intervention to one company division. Another division, which is similar, acts as a *non-randomized control group* by not receiving the intervention. In the example below, the assignment of reindeer herders to intervention and control groups was determined by geographical location.

[11] The terminology varies regarding the use of the term “control group”. Some use it only in the context of experimental designs, in which the intervention and control groups are formed through randomization. Others, including ourselves, also use the term control group in the context of quasi-experimental designs, in which groups are formed through a non-random process. In this case, the quasi-experimental control group is referred to as a “non-randomized control group”. “Comparison group” is sometimes a synonym for “control group”, but in other cases is reserved to describe the non-intervention group in a quasi-experimental design.

[12] Random assignment of participants to groups is discussed in Section 5.4.

Advantages of the "pre-post with non-randomized control group" design

By adding a non-randomized control group to the simple before-and-after design, you automatically reduce some of the threats to internal validity discussed in Chapter 3. In particular, interference by external circumstances (i.e., history effects) is reduced, because they will often apply to both the control group and the intervention group. It therefore allows a separation of the effect of the intervention from that of other circumstances. The following example illustrates this.

Example of a pre-post with randomized control group design

Due to the high rate of injuries among reindeer herders, preventive measures were developed. In intervention group A, letters describing possible preventive measures were sent to district leaders and contacts, who were asked to pass on the information to herders in their district. In intervention group B, occupational health personnel trained in prevention measures passed on information during medical examinations. There was also a control group C, which received no intervention. Pre-post statistics for the three groups are shown below.

Statistical analysis confirmed that the groups did not differ in terms of a decrease in accident rate. The authors had to conclude that the intervention efforts were ineffective.

Number of accidents/working days for reindeer herder groups[13]

Year of accident data	Intervention groups		Non-randomized control group
	A	B	C
1985 (pre)	18.7	21	19.2
1987 (post)	15.1	14.9	14.6

The above example demonstrates that it is possible to conclude that an intervention is ineffective, even though fewer accidents are seen after the intervention. The control group showed the evaluators how much change to expect in the absence of the intervention. These changes were likely due to history, and possibly, testing and Hawthorne effects, according to the original report by Pekkarinen et al.[13] Thus, we see how the presence of the control group allowed one to examine the intervention effect, free from the influence of internal validity threats.

On the other hand, a new threat to validity - selection effects - arises from using a non-randomized control group. This threat occurs when the intervention and control groups differ with respect to the characteristics of group participants and these differences influence the measures used to determine an intervention effect. Selection effects will be discussed further at the end of the chapter.

[13] Data from Pekkarinen et al. [1994] with permission of the Arctic Institute of North America.

4.2.2 Strategy #2: take more measurements (time series designs)

O O O X O O O

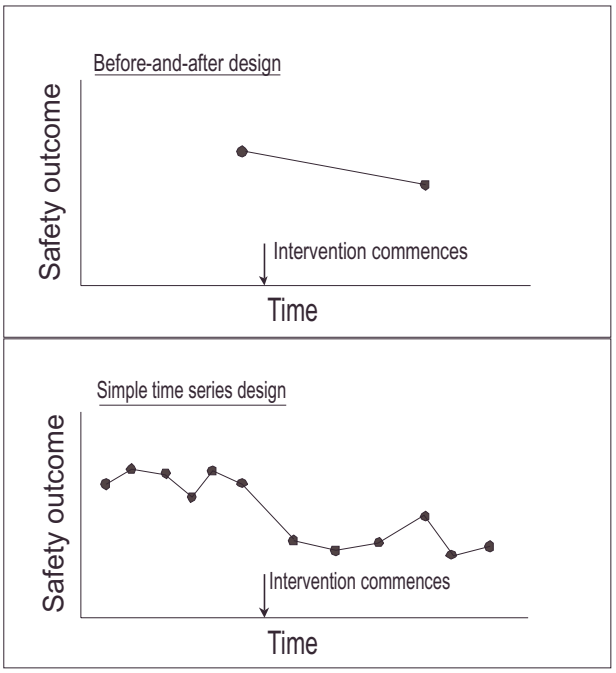
A *simple time series design* differs from the simple before-and-after design by taking additional measurements before and after the intervention. A baseline time trend is first established by taking several outcome measurements before implementing the intervention. Similarly, in order to establish a second time trend, several of the same measurements are made after introducing the intervention. If the intervention is effective, we expect to find a difference in outcome measures between the two time trends.

Advantages of simple time series design

Figure 4.1 illustrates how much easier it is to interpret the results of a time series evaluation design than a simple before-and-after design. In the first panel we see that there has been a drop in our safety measure from the period before the intervention to the one afterwards. As discussed in Chapter 3, several possible alternative explanations for this come to mind, e.g., history, maturation, instrumentation or Hawthorne effects. By adding measurements, as shown in the second panel, we can reduce the likelihood of some of these alternative explanations.

The maturation threat is eliminated because we observe that the change between the baseline time trend and the second time trend is abrupt. In contrast, changes due to maturation, such as increasing age or experience, are more gradual. Regression-to-the-mean or testing effects have also been eliminated as possible threats because we can see that safety outcomes are repeatedly high before and repeatedly low afterwards. Placebo and Hawthorne effects are less likely explanations because they tend not to be sustained once people have adapted to a change in their conditions. The threat of a history effect is somewhat lessened because the window of opportunity for a coincidental event is narrowed by the more frequent measures taken. Dropout and instrumentation both remain as threats, without consideration of additional information.

Figure 4.1 Comparison of before-and-after and time series designs



How many measurements are needed for a time series design?

The number of measurements you need for a time series design depends on the amount of random fluctuation (noise) that may occur in the outcome being measured and how much of an impact the intervention is expected to have. Somewhere between 6 to 15 measurements to establish a baseline and the same number again to establish the trend afterwards are typically required.[14]

Because of the necessity for many measurements,

[14] Several workplace examples can be found in Komaki and Jensen [1986].

the time series design is suitable for only some situations. For example, a time series design, using injury rate as the outcome measure would likely not be suitable for a small workplace. It simply takes too long - a year or more - for a small workplace to establish a reliable injury rate.

On the other hand, the design could be quite suitable in a group of small workplaces, a bigger workplace, or if observed work-site conditions were measured instead of injury rate. These situations permit more frequent and reliable measurement.

Even when it is not possible to take as many measurements as are needed for a time series analysis, taking additional measurements over time is still a good idea. It gives you a better sense of the pattern of variability over time and whether the last "before" measurement is typical of the ones preceding and the first "after" measurement is typical of the ones following. You are better informed of potential threats to internal validity and the sustainability of the intervention's effect. It may allow you to better estimate the effect of the intervention more accurately by pooling data.

Multiple time series designs

O O O X O O O
O O O O O O O

Even better than using basic strategy #1 or #2 alone, you can strengthen the before-and- after design even more, by combining both approaches (adding a control group and taking more measurements).

4.2.3 Strategy #3: Stagger the introduction of the intervention (e.g., multiple baseline design across groups)

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O O O O O O O X O O O

A special type of multiple time series design is known as "multiple baseline design across groups". With this design, all groups eventually receive the intervention, but at different times. As a result, all groups also serve as a comparison group to each other.

Advantages of the multiple baseline across groups design

The advantage of the multiple baseline across groups design is that it markedly reduces the threat of history effects. When an intervention is given to only one group, you can never really be sure that something else did not coincidentally occur at the same time to cause the measured effect. Even when you are using a control group, something could still happen to only the intervention group (besides the intervention itself) that affects the outcome.

When the intervention's introduction is staggered, with the apparent effects correspondingly staggered, history effects are an unlikely explanation for the result. This is because one coincidence of the intervention and an extraneous event happening close together in time is plausible, but two or more such coincidences are much less likely.

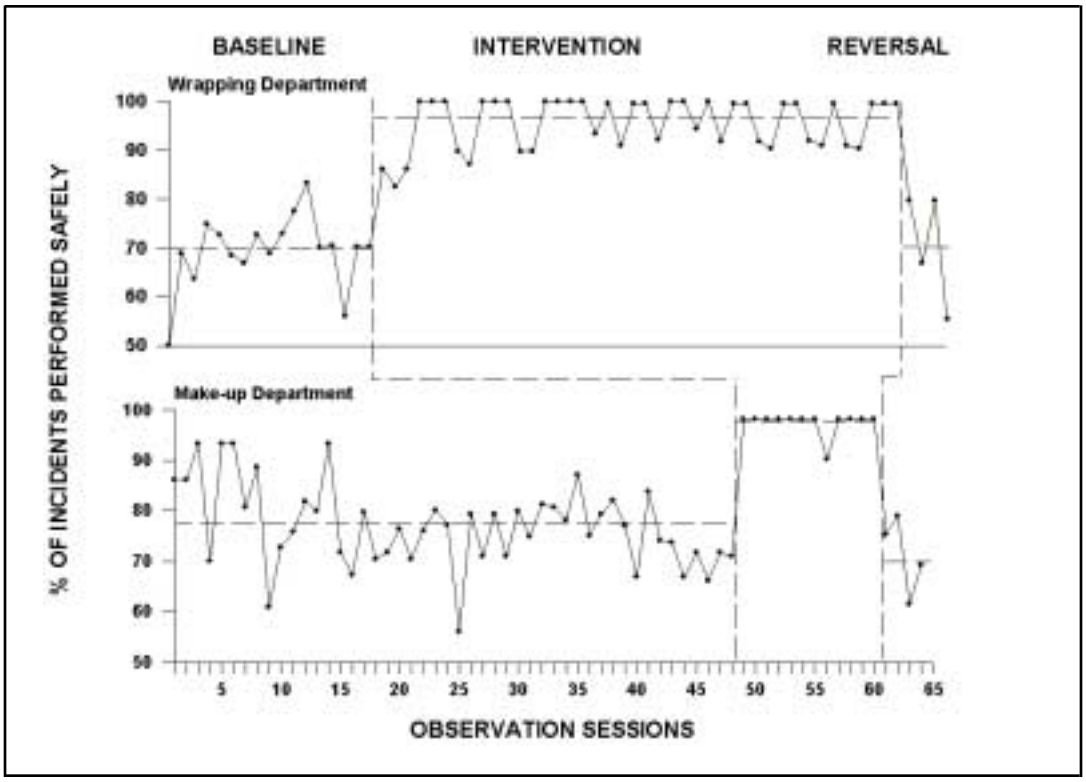
Whenever a workplace or jurisdiction has more than one division or group, a staggered introduction of the intervention should be considered as an alternative to introducing it to all divisions or groups at the same time. This staggered arrangement can also allow an interim assessment and, if appropriate, modification of the intervention or its implementation, before it is introduced into other divisions (though such modifications should be considered in the analysis and interpretation of results).

Example of a multiple baseline across groups design[15]

A safety behavior training intervention was undertaken at a food manufacturing plant. The intervention was first introduced in the wrapping department and then in the make-up department. The intervention started with an educational session on safety behaviors, after which a list of safety behaviors was posted. From then on the group was given feedback by posting the results of behavioral observations.

Safety behaviors were measured by a trained observer (three to four times a week). The observer used a checklist which gave an estimate of the percentage of incidents performed safely. Baseline measurements of safety behaviors were taken prior to introduction of the intervention.

You can see how, in each department, the change in safety behaviors followed implementation of the intervention. Having this sequence of events happen not only once, but twice, bolsters the causal link between intervention and behavior change. Further, because implementation occurred at different times, we really end up with two separate estimates of the amount of change caused by the intervention. [The reversal part of the intervention will be discussed in Section 4.2.4]



[15] Example from Komaki J, Barwick KD, Scott LR [1978] A behavioral approach to occupational safety: pinpointing and reinforcing safety performance in a food manufacturing plant. Journal of Applied Psychology 63:434- 445. Copyright © 1978 by the American Psychological Association. Adapted with permission.

4.2.4 Strategy #4: Reverse the intervention

O O O X O O O -X O O O

One way of strengthening a before-and-after or even a time series design is to follow the introduction of an intervention with another phase of the project in which the intervention is removed. In the simplest case, you end up with three phases: a baseline phase; an intervention phase; and a reversal or withdrawal phase. The rationale here is that if you remove the intervention conditions, you should correspondingly see a change in the outcome back towards the baseline condition.

Of course, this design is clearly not suitable for all situations, because it is hoped that the effect of an intervention will last and therefore is not easily reversed. However, as the Figure in section 4.2.3 shows, it has been found useful when behavior is the safety outcome being measured. In this case, the intervention was "reversed" by no longer giving the posted feedback.

Advantages and disadvantages of designs with a reversal phase

If you can demonstrate the effect of a reversal phase, you will have markedly reduced several of the internal validity threats discussed in Chapter 4 - in particular history, maturation, testing, dropout and Hawthorne (assuming researchers / outsiders are still present during reversal phase). Instrumentation and placebo effects may still remain as issues and should be considered. After demonstrating the effect of intervention reversal, you are then free to reinstate the intervention.

The downside to the reversal design feature is that repeated changes in safety programming could create confusion, stress and resentment among those affected. As well, if an intervention has looked promising following its introduction, subsequent removal could be considered unethical. Thus, use this design feature with caution.

4.2.5 Strategy #5: Measure multiple outcomes

O_1/O_2 X O_1/O_2

The final strategy for increasing the strength of an evaluation design is to use more than one type of outcome measure. We describe two approaches to doing this.

4.2.5.1 Add intervening outcome measures

We pointed out, using models in Chapter 2, that there can be a number of outcomes intervening between an intervention and the final outcome. We should ideally try to measure as many of these different intervention outcomes as is feasible, in order to bolster the strength of evidence provided by the evaluation design. This includes measurement of the intervention's implementation, as well as short- and intermediate-term effects of the intervention.

Measures of intervention implementation, such as the documentation of equipment purchases and work task modification in the following example, are especially important. In instances where a program has failed, you want to be able to distinguish between an inherently ineffective program and a flawed implementation. If an intervention has not been implemented as intended, measuring effectiveness by measuring changes in outcome will likely underestimate the intervention's potential impact. Thus, if inadequate implementation is found by the evaluation, you might try first to improve this part of the intervention, instead of discarding the intervention altogether.

Example of adding intervening outcome measures

A company plans to implement a participatory ergonomics program. Plans involve forming a labor-management committee, assessing employee needs, purchasing new equipment, modifying work tasks and providing worker education. The health and safety coordinator plans to measure the ultimate impact of the program by comparing self-reported symptoms and injuries before and after the intervention is implemented.

However there are concerns that a change in symptom and injury rates could have a number of alternative explanations, such as staffing changes, the business cycle, management changeover and Hawthorne effects, etc. To deal with this concern, the health and safety coordinator plans some additional measurements: records of equipment purchases; and self- reports of work tasks, practices and stressors. These all measure outcomes intervening between the intervention and the final outcome of changes in symptoms and injuries.

Illustration of the value of measuring intervention implementation

Mason [1982] tried to evaluate the effectiveness of a train-the-trainer kinetic handling training course, by looking at the change in the rate of back and joint injuries in the companies of instructors who had taken course. When practically no change was found after a year, it was valuable to know that this was probably because few of the instructors had organized and carried out in-company courses based on their own training during that year. Furthermore, those who did run courses had failed to retain most of their training and therefore could not pass on the handling techniques. The lack of any measurable effect of the intervention on injuries was therefore no proof that the kinetic handling technique itself was not effective, but rather that an improvement in the training methods for trainers were needed.

4.2.5.2 Add a related but untargeted outcome measure

The second approach to adding outcome measures involves measuring an outcome which is similar to the main outcome measure, but not targeted by the intervention. The additional outcome measure should be similar enough to the main outcome measure so that it is susceptible to the most important threats to internal validity. However, it also needs to be different enough that it should be unaffected by the intervention. The following examples show how this approach works.

Examples of adding related but untargeted outcomes

1)[16] The effect of new equipment on oil-drilling platforms was primarily evaluated by changes in the rate of tong-related injuries, a type of injury which should have been reduced by using the new equipment. The rate of non-tong-related injuries, a related but untargeted outcome measure, was also tracked. Although this second type of injury should have been unaffected by the intervention, it would likely be similarly susceptible to any history or reporting effects threatening the internal validity of the evaluation. Thus, including this untargeted injury measure in the evaluation reduced these threats, since any history or reporting effects on tong-related injuries would also be detected by changes in the non-tong-related injuries.

2)[17] An ergonomic intervention among grocery check stand workers was primarily evaluated by measuring self-reported changes in musculoskeletal discomfort. The intervention appeared successful because of significant change in reported symptoms in the neck / upper back / shoulders and lower back / buttocks / legs, the two areas predicted to benefit from the ergonomic changes. This conclusion was bolstered by a finding of no significant changes in symptoms in the arm / forearm / wrist, which were not targeted by the intervention. This made history, maturation, instrumentation, placebo, Hawthorne and instrumentation effects a less likely explanation for the improvement in the upper extremity and lower back areas.

4.3 Experimental designs

Two key features of an experimental design are 1) the use of a *control group* and 2) the assignment of evaluation participants to either intervention or control groups through *randomization*, a process in which participants are assigned to groups in an unbiased manner.[18] Thus, an experimental design uses an approach similar to strategy #1 in quasi-experimental designs (Section 4.2.1).

The use of randomization gives the experimental design greater strength. We can be more certain that any differences between the intervention group and the control group, with respect to the apparent effect of the intervention, can be attributed to the intervention, and not to group differences. Although it is often not feasible to use an experimental design, it has been used in several occupational safety situations.

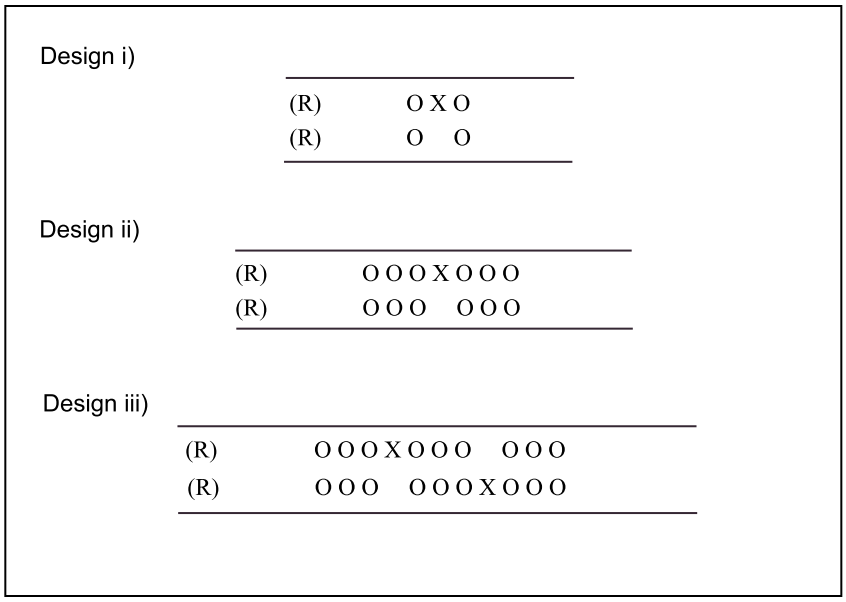
4.3.1 Experimental designs with "before" and "after" measurements

Earlier, three types of quasi-experimental designs were discussed that use non- randomized control groups: pre-post with non-randomized control group (Section 4.2.1), multiple time series (4.2.2) and multiple baseline across groups (4.2.3). These same design approaches can turned into experimental designs by using randomization to create the groups.

The first design shown in Figure 4.3, "pre-post-with-randomized-control" has been used in the subsequent examples. The first example involves randomizing work-sites into groups, and the second, randomizing individuals into groups.

[16] Based on Mohr and Clemmer [1989]
[17] Based on Orgel et al. [1992]
[18] Randomization is discussed in Section 5.4.

Figure 4.3: Experimental designs with "before" and "after" measurements



Example of an experimental design (1)[19]

An intervention for principle farm operators and their farms consisted of an on-site farm safety check with feedback and a one-day educational seminar. Potential participants in the intervention were identified from a list of all farms in the Farmers Association, using a random selection process. Of these, 60% of farm operators agreed to participate in the study. They were then assigned to either an intervention or control group, using a *randomization* procedure. To evaluate the intervention, these groups were compared on measures taken before and after the intervention: self-reported injuries and near-injuries (final outcome) and safety perceptions, practices and attitudes (intermediate outcomes).

Table 4.3 Example of an evaluation of a farm safety intervention using an experimental design

Group	Pre-intervention measures	Intervention	Post-intervention measures
	Injury self-report + perceptions, practices, attitudes questionnaire	Safety check + education	Injury self-report + perceptions, practices, attitudes questionnaire
A (intervention)	O	X	O
B (control)	O		O

[19] Adaptation of intervention described in Glassock et al. [1997]

Example of an experimental design (2) [20]

Two interventions for the prevention of back injury were evaluated with an experimental design involving warehouse workers for a grocery distribution center. Ninety workers with the same job classification were randomly selected from among the 800 employees at a warehouse. The ninety workers were then randomly assigned to one of three groups. One group was given one hour of training on back injury prevention and body mechanics on the job. A second group was also given the training, as well as back belts to wear. The third group served as a control group, receiving neither training, nor back belts. Both "before" and "after" measurements were taken: knowledge (short-term outcome); injuries and days absent as reported in health records (final outcomes). Abdominal strength was also measured in case it decreased as a result of wearing the belt (unintended outcome).

Table 4.4 Example of an evaluation of back belt and training interventions using an experimental design

Group	Pre-intervention measures	Intervention components		Post-intervention measures
	(Abdominal strength + questionnaire + injuries + absenteeism)	Training in body mechanics	Black belt use	(Abdominal strength + questionnaire + injuries + absenteeism)
A (intervention 1)	O	X		O
B (intervention 2)	O	X	X	O
C (control)	O			O

4.3.2 Experimental designs with "after"-only measurements

(R)	X O
(R)	O

One advantage of randomization is that in some situations it may allow for not having "before" measurements. This can be especially advantageous if you are worried about the measurement influencing the outcome of interest ("testing effect", section 3.5.4). It is also advantageous if taking a before measurement is costly (e.g., the administration of a questionnaire).

The disadvantage of not obtaining "before" measurements is that it will not be possible to see if the groups differed initially with respect to the outcome measure. You would therefore not be able to make any allowance in the analysis for these group differences.

[20] Based on Walsh and Schwartz [1990]

4.4 Threats to internal validity in designs with control groups

We discussed how designs that use control groups can markedly reduce the threats to internal validity discussed in Chapter 3. However, using control groups introduces some new threats to internal validity which we consider below. In spite of these, control groups are still strongly recommended. On balance, they strengthen the evaluation design far more than they weaken it.

4.4.1 Selection threats

A *selection threat* occurs when the apparent effect of the intervention could be due to differences in the participants' characteristics in the groups being compared, rather than the intervention itself. For this reason, control and intervention groups should be similar, especially with respect to any variables that can affect the measured outcome(s).[21]

Whenever you compare groups created through a non-random process, as in the quasi-experimental designs, you must consider how selection could affect your results. In what way do the people in the groups differ? Do they differ in their initial value of safety outcome measure or other characteristics (e.g., age, level of experience, level of education, etc.) which could influence the way groups respond to the intervention? If so, you need to collect information on these differences and make allowances for these differences in your statistical analysis.

Even by using a randomization procedure to create groups, as in a true experiment, you can have a selection threat.

4.4.2 Selection interaction threats

We just described how it is important for groups to be similar in their characteristics at the outset of an evaluation. It is also important that they remain similar and are treated similarly over the course of the evaluation. Otherwise, *selection interaction-effects* threaten the legitimacy of your evaluation conclusions. Recall that there are a variety of threats to internal validity in before-and-after designs, e.g., history, instrumentation, dropout, etc. In many cases having a control group - especially a randomized control group - can reduce or eliminate these threats to internal validity. The exception to this situation is when something happens to one group (e.g., history, instrumentation, maturation, etc.) and not to the other, resulting in selection interaction threats; i.e., selection-history, selection-instrumentation, selection-maturation, etc.

For example, a *selection-history effect* could occur if you are comparing two different divisions in a "pre-post with non-randomized control group" design. What if the supervisor of only one of these divisions changed during the course of the evaluation? You could not be sure whether between-group differences in the "before" to "after" changes was due to the effect of the intervention on the intervention group - or due to a change in the leader in one group. Selection-history interaction threats to internal validity are often beyond the evaluator's control, as in the example above. If they should arise, they are dealt with as was described for history threats (Section 3.5.1).

A *regression-to-the-mean-interaction threat* to internal validity arises if you deliver an intervention to units with high injury rates and compare their results to units with lower injury rates. Even if there was no intervention effect, the high injury group would tend to have a decrease in rates, and the others might have even shown an increase. The proper control group

[21] Depending on the type of evaluation design and the context, these characteristics or variables are sometimes called *confounders;* other times they are called *effect modifiers* or moderating variables.

would be a second group with similarly high injury rates.

A *dropout interaction threat* arises if one group has a greater rate of dropout than the other, especially if it results in the two groups having different characteristics. Characteristics of particular concern are those which could affect how the study participants respond to the intervention (e.g., age, level of experience, level of education), as well as differences in the initial value of the safety indicator used to measure outcome. While these differences are sometimes taken into account in the statistical analysis, it is preferable to avoid selection-dropout threats to internal validity altogether by taking steps to ensure that people continue participating in the intervention project and its evaluation.

Most other selection interactions, i.e., selection-instrumentation, -testing, -placebo, - Hawthorne, or -maturation effects can be minimized by treating the control group as similarly as possible to the intervention group with the exception of the intervention itself. Ideally, the evaluators should have just as much contact with individuals in the control group as those in the intervention group. In practice, such an arrangement may not be feasible.

4.4.3 Diffusion or contamination threat

A *diffusion threat* to internal validity (also known as a contamination threat) occurs when the intervention delivered to one group "diffuses" to the other. This can easily happen when the intervention is educational in nature, since workers naturally share information with one another. It is even possible for new equipment given to the intervention group to be shared with the control group. Diffusion is most likely to occur when the intervention is perceived as beneficial. It is undesirable for an evaluation because it reduces the differences observed between groups in their "before" to "after" changes. Thus, you might conclude that an intervention was ineffective when it really was not. The best way to reduce the threat of diffusion is by keeping the intervention and control groups as separate as possible.

4.4.4 Rivalry or resentment threat

Finally, threats to validity can arise when people in the control group react to not receiving the intervention. Suppose a safety incentive program has been introduced to encourage safe behaviors. The control group could react by not reporting injuries so its safety performance ends up looking good compared to the intervention group. Or the opposite might be done. Injuries could be "over-reported" to demonstrate that the group needs an incentive program as well. In both cases we could say that the control group has changed its behavior due to rivalry. Resentment effects are also possible. The control group, for example, could resent not being given the opportunity to participate in an incentive program. This souring of labor-management relations in the division could cause an increase in injury rates.

Rivalry or resentment threats can affect the evaluation's conclusions in either direction. Depending on the situation, they can either increase or decrease the differences between groups in "before" to "after" changes. The effects just described can sometimes be avoided by communicating well with groups or promising that if the intervention is shown to be effective, the control group will receive the intervention afterwards. If interventions are conceived and introduced through a participatory process, unexpected reactions are less likely. However, it is impossible to anticipate every reaction to a program. This is one area where qualitative investigation can be very helpful. Interviews with a few knowledgeable people in the control group should give insight into whether rivalry or resentment dynamics are an issue. As with the diffusion threat, the rivalry or resentment threats might be avoided if groups in different locations are compared and communication between the groups does not occur.

4.5 Summary

A quasi-experimental or experimental design is more likely to give a truer estimate of the effect of an intervention than a non-experimental design. You can change a (non-experimental) before-and-after design into a quasi-experimental one through one or more of the following design strategies: adding a control group; taking more measurements; staggering the introduction of the intervention; reversing the intervention; or using additional outcome measures. By adding these design elements you can increase the strengthen the design and reduce or eliminate the threats to internal validity discussed in Chapter 3.

Experimental designs differ from quasi-experimental designs by always involving a control group and by assigning subjects to intervention and control groups under a randomization scheme. Otherwise, many of the elements of quasi-experimental and experimental designs are the same. Although some new threats to internal validity need to be considered when using designs with control groups - selection, selection interactions, diffusion, rivalry, resentment - the use of control groups is almost always recommended whenever feasible.

Key points of Chapter 4

- Improve upon a simple before-and-after design, and use a quasi-experimental design, through one or more of five strategies:
    - adding a control group
    - taking more measurements
    - staggering introduction of the intervention among groups
    - adding a reversal of the intervention
    - using additional outcome measures.
- Improve upon a quasi-experimental design, and use an experimental design, by assigning participants to intervention and control groups through randomization.
- Check that intervention and control groups receive similar treatment throughout the evaluation period, apart from the intervention itself.
- Avoid (but check for) diffusion, rivalry or resentment effects.