QUASI EXPERIMENTS
Principal Differences Between Quasi Experiments (QE) and True Experiments (TE)

TE
- Where one has complete control over the who, what, when, where, and how of the experiment.
- Full experimental control can be accomplished only when ***all variables*** are under direct manipulation and there is a control group, condition, or means of comparison present.
- It permits ***greatest degree of control*** in ruling out alternative hypotheses, or alternative IVs, as being cause of difference between two groups or conditions.
- Thus, TEs permit the most ***powerful control for confounding*** because all other potential IVs have been eliminated by ***randomly assigning subjects to conditions***.
- If enough compromise of experimental control takes place, the research is considered a QE.

QE
- Where one ***does not*** have full experimental control.
- Q-E designs are for ***taking an experiment out of the lab,*** or***, cannot maintain total control of all variables in a lab.***
- In ***TEs we manipulate variables***, whereas in ***QEs we observe categories of subjects***.
- Whereas it is ***possible to assign subjects to conditions in a TE***, in a ***QE it is necessary to select subjects for the different conditions from previously existing groups***.
- QEs are sometimes called ***Ex Post Facto*** (after the fact) experiments because they are conducted after the groups have been formed, such as ***gender of participants***.
- QEs leave open possibility that other differences exist between experimental and control conditions and thus permits other potential differences to remain.
- IV in a QE is often called a ***Subject Variable*** if it is a characteristic of the subjects on which they have been selected, such as ***gender of participants***.
- In some cases, it would be unethical to induce the IV.
  EG: Experimenter is interested in psychological effects of child abuse. Unethical to induce child abuse on a sample of children. Or an experiment that must be conducted in a natural setting where planned or unplanned effects can occur. This pertains to situations where one ***cannot randomly assign subjects*** to treatment conditions or account for extraneous events.
- It is possible to have one experimental variable and one QE variable in an experiment.
  EG: Effects of two different teaching methods on classroom learning, interested in whether slow learners differ from fast learners in their response to the teaching methods. The two teaching methods would constitute a TE variable (assuming students randomly assigned to sections) and the classification into slow and fast learners would constitute a QE variable.
- Along with the “who”, in some instances in a QE one cannot completely control the “what, when, where, and how” of an experiment.
  EG: The “when”…Study of effects of changing work schedules on productivity must do so when management of the plant decides to make the changes. Problem is that productivity may already have been changing because of some outside variable.
- Always a trade off in External Validity (generalization to the real world) and Internal Validity (cause and effect through control of variables) between QEs and TEs.
- Other things being equal, one would choose a TE over a QE, and a QE over a nonexperimental method (no control over the presentation of the IVs and can only record what happens in a certain situation).
  EG: Many social-psych phenomena are difficult to bring into the lab in a realistic fashion. Therefore, a field study may be preferable to an TE because the advantage of realism outweighs the loss of control. The ***best method is the one that best answers your research question***. If a QE best answers your question, it is the best method.

NONEQUIVALENT-CONTROL-GROUP DESIGNS

- **Nonequivalent-Control-Group Design** - research design having ***both an experimental and a control group*** wherein subjects are ***not randomly assigned*** to groups; most typical QE.
- In example of a company that wanted to evaluate effect of a ***new work schedule*** (Lecture on Single-Factors Design), study would be ***improved by having a 2nd plant as a control group***.
- This example of a ***nonequivalent-control-group design*** with pre-test and post-test is a typical QE design and is shown below.

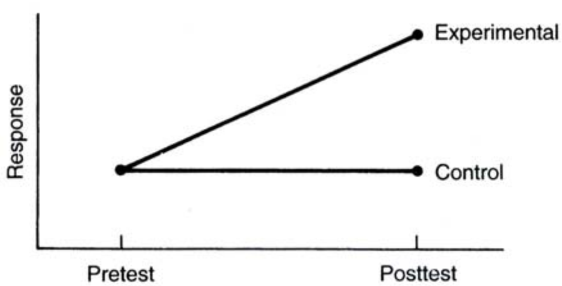
Nonequivalent-Control-Group Design with Pre-Test and Post-Test

	Allocation of Subjects and Groups	Pre-Test	Treatment	Post-Test
Group 1	Any method that is	Yes	Yes	Yes
Group 2	*not* random	Yes	No	Yes

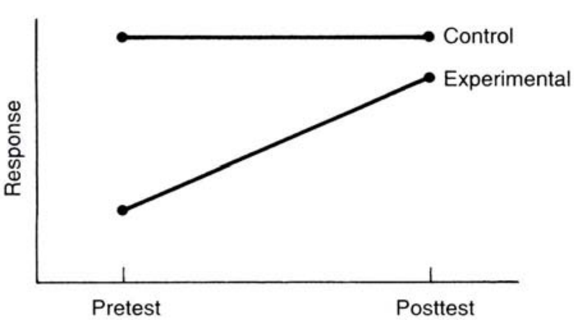
- Subjects were not randomly allocated to the two groups, so they may not be equivalent before the experimental manipulation was performed.
- Problem is in determining how to compare results between experimental and control groups when they were not equivalent to begin with (e.g., workers in experimental plant may have been less experienced on average than those in control plant).
- The increase in productivity in the experimental plant may have been caused by experience those workers gained between pre-test and post-test. In contrast, control subjects might already have been working at their maximum.

Interpretable and Uninterpretable QEs

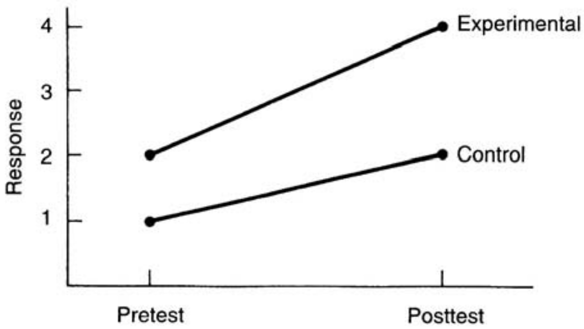
- Whether QEs that employ ***nonequivalent control groups with pretest and posttest*** can be interpreted depends on whether pattern of results can be accounted for by possible differences in groups or by something else in the experiment.



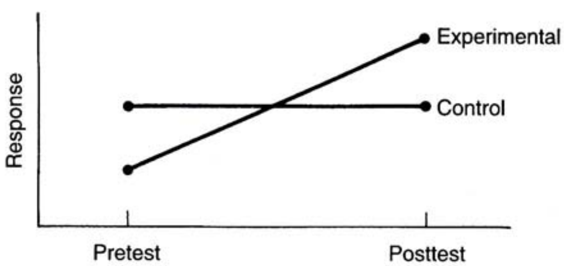
- Here 2 groups show same performance on pretest.
- Experimental group improved on posttest, but control group did not change. (***Interpretable***)
- Although experimental and control groups were not equivalent in all respects because they were not randomly constituted, their performances can be compared and results interpreted because their ***behavior was the same at the beginning***.



- Here, experimental group improved but control group did not and control group was superior to experimental group on both occasions.
- This difference could result from the operation of a ceiling effect. (***Uninterpretable***)
- If it was not possible for control group to perform any better, then we cannot attribute improvement in the experimental group to the experimental manipulation.



- These results could represent a learning experiment in which experimental group performed better than control group on pretest.
- Both groups improved on posttest, but experimental group showed twice as much improvement.
- Can difference in rate of improvement be attributed to experimental manipulation? ***No***, because, although experimental group improved more, both groups showed same proportional improvement.
- Possible that learning was twice as fast in experimental group, but could be due to some other variable that had nothing to do with experimental manipulation. (***Uninterpretable***)



- Here experimental group lower than control group on pretest but higher than control group on posttest. This is called the ***Cross-Over Effect***.
- Experimentals could have been as good as controls to begin with and did worse on pretest by chance.
- In that case, you would expect them to do the same as the control group on posttest if the experimental manipulation were not effective.
- However, there would be no reason to expect them to do better than the control group on posttest by chance alone - Therefore, safe to conclude that a pattern of results such as those here are ***Interpretable***.

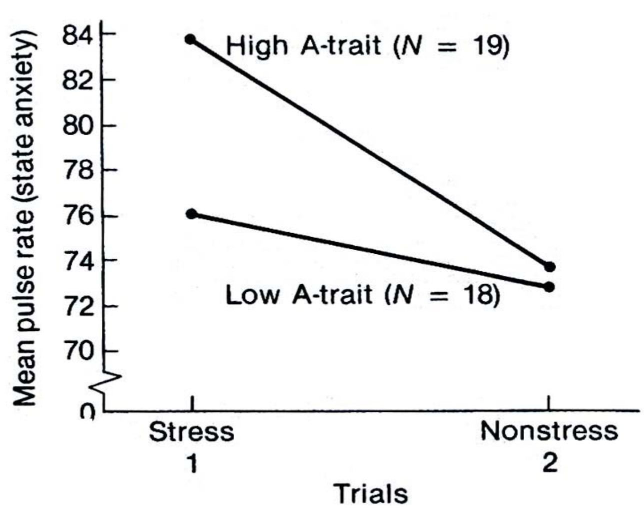
Example of a Non-Equivalent-Control-Group Design –
Mixed Factorial Design with One Non-manipulated Variable

- Endler (1977) interaction theory of anxiety - state of anxiety is a result of a trait for anxiety that interacted with anxiety-provoking situations.
- Individuals who are high on anxiety trait would not be in anxious state ***all the time*** but would respond more to anxiety-provoking situations.
- 2 groups of students selected on basis of who scored high and low on ***Pre-Test*** of anxiety.
- Then anxiety state measured (pulse rate) first in a threatening situation (exam), then 2 weeks later in a nonthreatening situation.
- Design is a mixed factorial with one between-subjects variable (anxiety trait of high and low) and one within-subjects variable (situational anxiety of threatening and non-threatening).
- Found students who were high or low on the anxiety trait (***Pre-test***), did not differ (***Post-test***) in anxiety state in non-threatening situation, but did differ in non-threatening situation

TABLE 14.2

Design of the Endler Study

	State Anxiety	
Trait Anxiety	Low	High
HIGH	S_1, S_2, •, •, •, S_{19}	S_1, S_2, •, •, •, S_{19}
LOW	S_{20}, S_{21}, •, •, •, S_{37}	S_{20}, S_{21}, •, •, •, S_{37}



DESIGNS WITHOUT CONTROL GROUPS

- Sometimes no control group is available to be considered comparable enough to be useful.
- Design that allows the same group to be compared over time must be used.

Two Designs Without Control Groups:

1. **Interrupted Time-Series Design**
2. **Repeated-Treatments Design**

1. Interrupted Time Series Design – allows the same group to be compared over time by considering the trend of the data before and after experimental manipulation.

- One way to improve on ***one-group before-after*** design is to ***consider trend of data before and after the manipulation***, rather than simply comparing the average data before and after.
  EG: Manager of the plant that is changing its work schedule might keep a weekly record of output for years preceding and following change. Management could then look not only for average differences between two periods but also for trends that might appear. Seasonal changes or other cyclical changes in output may be important, as well as any overall trend toward higher or lower productivity that occurred around time of the change. The ideal situation would be a flat and stable baseline before the change, followed by either an abrupt change to a new level or a gradual change to a new level.

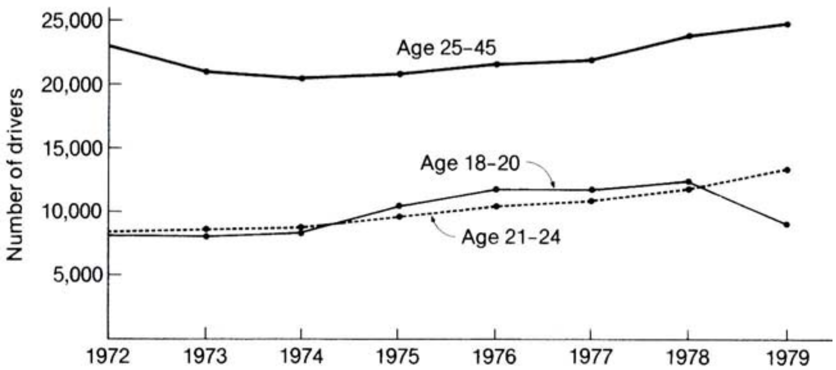
Example of a Multiple-Nonequivalent-Control-Group-Time-Series Design
Wagenaar (1981) Raising Drinking Age on Alcohol-Related Traffic Accidents in Michigan

- Michigan lowered drinking age from 21 to 18 in early 70s after voting age had been lowered to 18 in all 50 states.
- Change saw ***35% increase in alcohol-related crashes*** among Mich. drivers ***aged 18-20***.
- Therefore, in ***January 1979 the state returned the drinking age to 21***.
- Wagenaar wanted to know whether alcohol-related accidents had decreased following the change back to age 21 and whether this decrease could be attributed to the change.
- Wagenaar did his study one year after change back to age 21 and found that in ***1979*** drivers ***aged 18 to 20*** were involved in ***26% fewer accidents*** that were reported by police as "had been drinking" compared with 1978….lowest in five years.
- Several alternative hypotheses had to be ruled out before one could conclude that the change in drinking age caused the decrease in accidents.
  EG: Officers might have changed criterion of a driver in an accident and drinking or occurrence of an economic recession and higher gasoline prices both might have reduced amount of driving or fact that winter of 79 had relatively mild weather that made driving safer.
- To rule out rival hypotheses, he compared data on accidents reported as "had been drinking" with other similar data.

- ***First alternative hypothesis*** concerned change in police officers' criterion for reporting drivers who "had been drinking".
  EG: Compared original data (drivers aged 18-20 who were in accidents in which driver was reported as "had been drinking") with late-night, single-vehicle accidents involving drivers in same age group. Of such accidents, ***60% are known to be alcohol related***, which means that statistics on these accidents would not be affected by police officers' judgments of drinking. He found that these ***late-night, single-vehicle accidents had also decreased***.

A second test of hypothesis that police officers might have changed their criterion for reporting drunk driving was possible by comparing single-vehicle nighttime crashes to similar daytime crashes for the 18-20 year olds. It is known that fewer daytime crashes than nighttime crashes are alcohol related. He found that ***both categories of crashes decreased after the change in the drinking age, but the nighttime crashes decreased twice as much as did the daytime crashes***.

- ***Second alternative hypotheses*** concerned recession, price of gasoline, and winter weather.



EG: He compared accident data for drivers aged 18-20 with data for drivers in age groups who were not affected by change in law.

The other age groups actually showed an increase in alcohol-related crashes, as shown below

- It was still possible that results were caused by some factor other than a change in drinking age, so he compared Michigan results with data from another state that also raised drinking age, Maine, and two that did not, New York and Pennsylvania.
  EG: Results in Maine paralleled Michigan results, whereas no change occurred in accident rate in New York or Pennsylvania at the time that Michigan changed its law.

- Together, the various comparisons make it ***reasonably certain that the change in the Michigan law did produce the decrease in number of traffic accidents***.
- Because Wagenaar compared his group of 18- to 20-year-olds with several different groups that were not randomly assigned to conditions, his study is a ***multiple-nonequivalent-control-group-time-series design***.

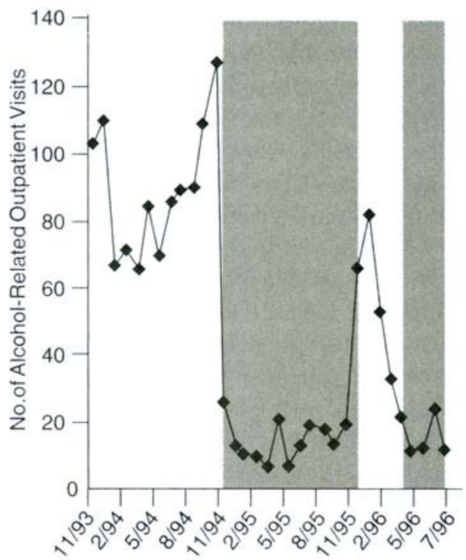
2. **Repeated-Treatment Designs -** design in which a treatment is withdrawn and then presented a second time - improves validity by presenting the treatment more than once.

- Subject's response is measured before and after the introduction of a treatment, then the treatment is withdrawn and the whole process is begun again.
- ***Limitation:*** treatment must be one that can be withdrawn without causing complications in analysis of data.
- Whatever change is found between Pretest-1 and Posttest-1 should be in the same direction as that between Pretest-2 and Posttest-2.
- A reversal in any previous trend of response between Posttest-1, when treatment is withdrawn, and Pretest-2 is desirable to rule out possibility that there would have been a continuous change in performance over the 4 tests regardless of treatment.
- Like the interrupted time-series design, the repeated-treatment design is one that is ***used in single-subject experiments***.

Repeated-Treatment Design

PreTest-1	Treatment	PosTest-1	Withdraw treatment	PreTest-2	Treatment	PostTest-2

Example of a Repeated-Treatment Design
Chiu, Perez, & Parker (1997) Ban on Alcohol Consumption in an Alaskan Community



- Barrow, Alaska, isolated town of ~ 4000 people, most indigenous to the area.
- When oil was discovered near Barrow in 1967, prosperity increased and many nonnative Alaskans came to the area.
- Although previously rare, alcohol became widely available with the increased cash economy.
- Many residents disapproved of increase in drinking, and in 1994, possession of alcohol was made illegal in town of Barrow.
- Ban was repealed in 1995, and then reinstated in 1996 after lawsuit alleging flaws in referendum that ended ban.
- To assess impact of alcohol policy changes on medical problems related to alcohol consumption, number of outpatient alcohol-related visits to Barrow's hospital was examined over a 33-month period, spanning 1993-1996.
- Data demonstrates the number of alcohol-related outpatient visits were ***much higher during periods of time alcohol could be legally possessed*** than when it was illegal in Barrow.

QUASI-EXPERIMENTS in a NUTSHELL (6 points)

1. The boundaries between true experiments, QEs, and nonexperi-ments are not sharp; the distinctions are based on the relative amount of control that the researcher is able to maintain.
2. QEs may be performed when a true experiment would be impossible or when the advantages of a QE outweigh its disadvantages.
3. The most common QE situation is to have nonequivalent control groups. Such experiments are sometimes uninterpretable, depending on the pattern of results.
4. An example of a QE factorial design was the study of anxiety. Trait anxiety was a QE variable, and state anxiety was a true experimental variable.
5. Interrupted time-series designs consider the trend of the data before and after some manipulation in a study with no control group. The ideal situation is to have a stable baseline before the manipulation, followed by an abrupt or gradual change to a new stable level.
6. Repeated-treatment designs improve on the validity of an experiment by presenting the treatment more than once. The ideal result is for each presentation of the treatment to produce a change in the same direction, with a reversal of the effect when the treatment is removed.