Research Methods in Psychology

Understanding Science:
How Do They Know That?

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Preface

Understanding the method by which research is done is a core part of most scientific study. Psychological science, the science of human behavior, has its own characteristic set of methodologies and challenges in drawing robust conclusions. There are two major goals of instruction in research methods to university students. First is to prepare for participation in the process of science through collaborative research done as advanced students. Second, to develop a basic understanding of how inferences about the world are drawn through scientific study with a grasp of the strengths and weaknesses of specific scientific methodologies.

It has become particularly clear in recent years that this second point is an important element of critical thinking about science that has been a particular challenge for the general populace. People seek to better understand the world around them and are exposed to a wide variety of scientific claims and results, but are significantly hampered by a lack of understanding of the methodologies used to draw those claims. Without being prepared to critically review and understand how robust or reliable these claims are, misinformation spreads rapidly and dangerously. Attempts to combat misinformation directly have the unfortunate side effect of weakening confidence in the scientific process in general and shifting attention back to anecdotes and information personally observed.

Teaching methods and the process by which science is done is tricky. People naturally seem to like to learn facts and findings, but the ideas about the meta for how these findings were obtained does not appear to elicit the same natural curiosity. This is something that teachers of science need to work to overcome in order to generally increase the overall scientific literacy of the populace. That the method is interesting itself is something that can even surprise experienced scientests. Some years back in conversation with Kathleen Grady, Ph.D. (the author's mother), she remarked on her own surprise at being captured by interesting aspects of methodology framed as asking the question "How do they know that?" when encountering some brand new, unexpected result.

We may be able to inspire better understanding of how science informs us of the world around us by both encouraging asking this question and providing the tools to try to answer it. This question is taking as the sub-title for this text.

The structure of this text reflects an attempt to create a Research Methods textbook that aligns with the teaching style we use at Northwestern University. In a single 9-week quarter, we use a very hands-on approach to experimental research methods that incorporates both teaching the basic elements of design and significant APA-style writing assignments. We find this approach very effective for preparing undergraduates to understand research basics and be ready for both upper-level research oriented classes and opportunities to work directly within department research labs.

However, this requires an un unusual pacing of the class that does not align with most traditional research methods in Psychology textbooks. Rather than starting with a more gentle introduction to the importance of science, the philosophical ideas about drawing inferences from human behavior or even an overview of reserach ethics, Chapter 1 in this approach is plunging in to a basic research design through an active example. After many years of starting with Chapter 7 in traditionally structured texts, I decided to try to prepare a text that followed the pacing of our course design.

While our course pacing may be idiosyncratic to my institution, I have also come to believe that rapid engagement with hands-on examples may be an effective tool for overcoming the natural discinclination to learning about methodology. In the abstract, the rationale and statistical purpose of employing a two-group design with random assignment to conditions is a fairly dry and possibly boring idea. Perhaps we can inspire more engagement with the concepts by seeing the concepts in action and immediately facing the questions of what we learn from data obtained via this methodoolgy.

The content is aimed not to completely overlap with my classroom lecture slides content. I do not want the students to feel that classroom time is spent completely rehashing the text. I prefer to have different, novel examples illustrating the concepts and to use a very question-and-answer style in the classroom to maintain student engagement. I would like to work towards having that information available to students as well without minimizing the value of coming to class, but I'm not sure how to organize it.

We will start by reviewing methods of experimental psychology research. Typical textbooks for Research Methods start with a review of the scientific method, some history on psychological science, discussion of non-experimental methods and then the process of the design, implementation, and analysis of experimental research. Because this class is designed to be hands-on with active involvement in the actual course of research, we are starting immediately with experimental methodology.

Somewhat initially daunted by the prospect of preparing an entire textbook of content for the class, I started this project based on an open-source textbook made freely available by Rajiv S. Jhangiani, I-Chant A. Chiang, Carrie Cuttler, and Dana C. Leighton. This text was invaluable in motivating this process and the plan is to make this content similarly open-source and freely available.

You may notice some residual redundancy in the text, especially in areas where conceptual ideas are explained related to specific content for the Reber/NU class presentation and then explained again as presented by the original authors of the text. In some places these are left deliberately to help build a better understanding of complex or non-intuitive ideas by multiple explanations from slight different perspectives.

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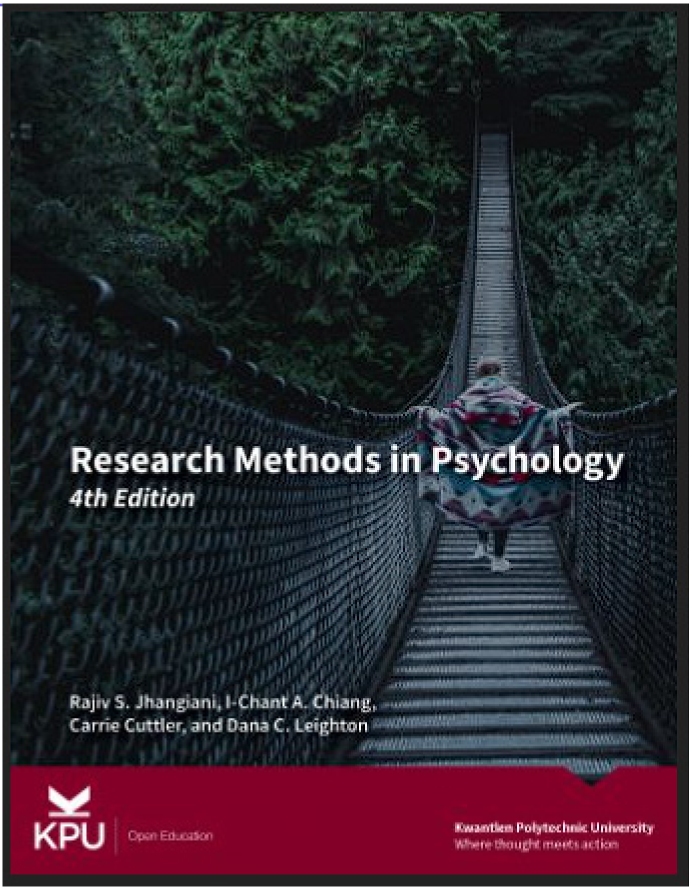
Research Methods in Psychology
4th Edition
Rajiv S. Jhangiani, I-Chant A. Chiang, Carrie Cuttler, and Dana C. Leighton
KPU
Open Education
Kwantlen Polytechnic University
Where thought meets action

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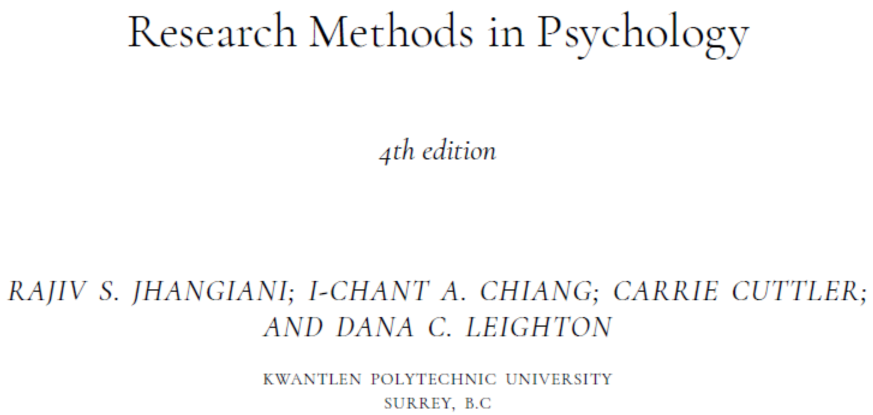
This adaptation constitutes the fourth edition of this textbook, and builds upon the second Canadian edition by Rajiv S. Jhangiani (Kwantlen Polytechnic University) and I-Chant A. Chiang (Quest University Canada), the second American edition by Dana C. Leighton (Texas A&M University-Texarkana), and the third American edition by Carrie Cuttler (Washington State University) and feedback from several peer reviewers coordinated by the Rebus Community. This edition is licensed under a Creative Commons Attribution-NonCommercial-ShareAlike 4.0 International License.

Jhangiani et al. (2022) Preface

Psychology, like most other sciences, has its own set of tools to investigate the important research questions of its field. Unlike other sciences that are older and more mature, psychology is a relatively new field and, like an adolescent, is learning and changing rapidly. Psychology



Research Methods in Psychology

4th edition

RAJIV S. JHANGIANI; I-CHANT A. CHIANG; CARRIE CUTTLER;
AND DANA C. LEIGHTON

KWANTLEN POLYTECHNIC UNIVERSITY
SURREY, B.C

researchers are learning and changing along with the emerging science. This textbook introduces students to the fundamental principles of what it is like to think like a psychology researcher in the contemporary world of psychology research.

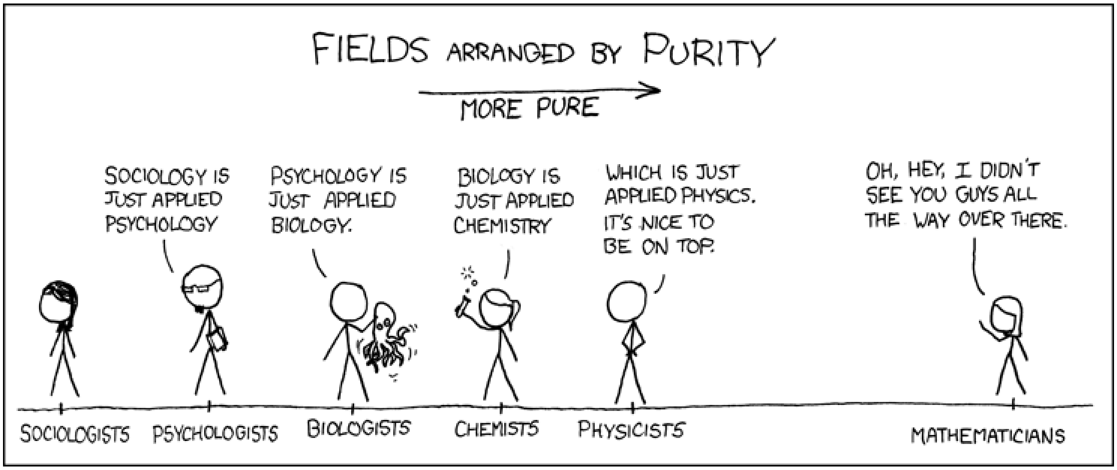
Historically, psychology developed practices and methods based on the established physical sciences. Unlike physical sciences, psychology had to grapple with the inherent variation among its subjects: people. To better account for this, we developed some practices and statistical methods that we (naïvely) considered to be foolproof. Over time we established a foundation of research findings that we considered solid.

In recent years, psychology’s conversation has shifted to an introspective one, looking inward and re-examining the knowledge that we considered foundational. We began to find that some of that unshakable foundation was not as strong as we thought; some of the bedrock findings in psychology were being questioned and failed to be upheld in fuller scrutiny. As many introspective conversations do, this one caused a crisis of faith.

Psychologists are now questioning if we really know what we thought we knew or if we simply got lucky. We are struggling to understand how what we choose to publish and not publish, what we choose to report and not report, and how we train our students as researchers is having an effect on what we call “knowledge” in psychology. We are beginning to question whether that knowledge represents real behaviour and mental processes in human beings, or simply represents the effects of our choice of methods. This has started a firestorm among psychology researchers, but it is one that needs to play out. For a book aimed at novice psychology undergraduates, it is tempting to gloss over these issues and proclaim that our "knowledge" is "truth." That would be a disservice to our students though, who need to be critical questioners of research. Instead of shying away from this controversy, this textbook invites the reader to step right into the middle of it.

With every step of the way, the research process in psychology is fraught with decisions, trade-offs, and uncertainty. We decide to study one variable and not another; we balance the costs of research against its benefits; we are uncertain whether our results will replicate. Every step is a decision that takes us in a different direction and closer to or further from the truth. Research is not an easy route to traverse, but we hope this textbook will be a hiking map that can at least inspire the direction students can take and provide some absolute routes to begin traveling.

As we wrote at the beginning of this preface, psychology is a young science. Like any adolescent, psychology is grappling with its identity as a science, learning to use better tools, understanding the importance of transparency, and is having more open conversations to improve its understanding of human behaviour. We will grow up and mature together. It is an exciting time to be part of that growth as psychology becomes a more mature science.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SOCIOLOGISTS
PSYCHOLOGISTS
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PHYSICISTS
MATHEMATICIANS

Table of Contents

Hands-on Research Methods

Students will complete 3 experiments over the course of the quarter. The chapters are ordered to support this process by covering as much information as possible needed for the next step.

Experiment 1

On the first day of class, students participate together completing a short memory experiment. This design and these data are used to illustrate basic experimental design, data analysis and to start the process of learning to write a formal, APA-style scientific report.

Chapters 1-4 introduce the basic concepts and terminology for experimental psychological research.

Chapter 5 explains how to analyze the Experiment 1 data.

Chapter 6 introduces APA-style scientific reporting to support students writing a brief overview of Experiment 1 as the first major writing assignment.

Chapter 7 extends simple research designs to include within-participant manipulations.

Chapter 8 introduces research ethics in Psychological science. The first midterm exam is typically given after this class.

Experiment 2

Chapter 9 and 10 introduce factorial design as a more complex variation of experimental research methods. We focus on 2x2 designs as the simplest version of this more complex design.

Experiment 2 is developed collaboratively with the students as a 2x2 design that extends Experiment 1. The students are responsible for recruiting participants and collecting data.

Chapter 11 discusses the issues of sampling, generalizeability and how this affects inferences from data.

Chapter 12 supports the tools needed to carry out the analysis of Experiment 2 by the students.

The second major writing assignment includes both Experiment 1 and 2 and is completed at this point.

Student-led research

Students then begin their final projects for the class, starting with submitting a proposal to carry out their own, short research project.

Chapter 13 goes over the development of a research proposal to support the final project process.

In parallel with the final project process, the remaining chapters cover material typically included in Research Methods instruction that does not immediately support the three hands-on research projects.

Chapter 14 covers non-experimental design. Chapter 15 provides an overview of survey research and related instruments. Chapter 16 rounds out basic statistical tools including correlation and chi-squared tests. Chapter 17 introduces field research. Chapter 18 extends ethics in research to Responsiebl Conduct of Research. Chapter 19 introduces quasi-experimental design. Chapter 20 reviews special methodological considerations for Developmental and Neuropsychological research.

1 Experimental Methods

Hands-on Approach

- Participate in a short psychology experiment using the QR code or the link below. When you have finished you will get a Completion Code to enter as the answer to the first assignment for the class.

- *Note: the experiment and questions/ discussion below will be covered on the first day of class. Review the Q&A below if you want a refresher for that discussion.*

Or use the following link:
https://tinyurl.com/Reber205

- Answer the following questions about that study. The following series of questions is based on the experiment but assumes some prior experience with psychological science. Since Research Methods typically follows and builds on classes *Introduction to Psychology* and basic *Statistics*, we therefore assume some familiarity with basic terms and ideas. Here we aim to reinforce understanding of these core ideas within the framework of what a simple experimental design looks like from the above hands-on example.

For answering the following questions it is useful as an exercise to cover the answers and try to answer the questions yourself before reading on. This will help you assess how much of the basic terminology and experimental approach you are already comfortable with. The terms will be defined in this chapter for general reference. The goal here will be to use the main terminology frequently enough that it simply becomes part of your understood vocabulary without need to look definitions up later on. The bolded terms below are ones to start becoming comfortable with.

What was this experiment about?

The general temptation for the answer to this question is to give a lot of detail about your experience with the experiment and guesses about how this relates to the underlying hypothesis. However, after just going through the experiment, you actually do not know what the experiment is about because you have not seen enough of the design. This is a typical experience for a participant in an experiment that has an **independent variable** that is manipulated **between-participants**. You only experienced one of the conditions, so the underlying **hypothesis** is not visible to you.

Key Terms

The bolded terms in the answers are key concepts in experimental design that will be used daily in class and throughout the text. A glossary of definitions is provided below for general reference.

However, when we consider and evaluate research with examples as short summaries or drawn from published papers, we will always start with this question and the answer we are looking for in this very basic question is the highest-level **construct** that gives the overall domain of the aexperiment. Here, that is simply "memory."

As we will see, designing an experiment in psychology generally starts with something we are trying to learn about. In psychology, that will be a concept like memory, perception, anxiety, relationships, language, identity, etc. One of the specific challenges of experimental methods in psychology, as opposed to other areas of science (chemistry, physics, biology), is that while we intuitively understand each of those concepts, there is a significant amount of effort needed to turn that idea into things that can be used in research. That process is called identifying the **operational definition** of the **construct**, which is essentially, how are we going to capture that idea in a controlled study.

Answering the next questions will require being familiar with some technical terms that you may have encountered in prerequisite classes. If you are unfamiliar with the terms, they are defined below for your reference.

What was the independent variable?

To answer this question, you need some additional information. There were two different conditions used in this experiment. Half of the time, participants are given instructions to rate how much they like each word, on a 1-5 scale from "very much" to "not at all." The other half of the participants get instructions to count how many vowels there are in each word and also make a response on a 1-5 scale.

The **independent variable (IV)** is the conditions created by the experimenter and applied to the participants. Here it is the instructions given for how to read and engage with the list of words. A more interesting question is what **construct** is this **independent variable** an **operational definition** of? What is the construct that the experimenter is manipulating in this study? The answer is "depth of encoding" which refers to how much engagement the participants have with the meaning of the words in the study list. Understanding why this is an interesting factor to manipulate will require some background reading to become familiar with the theory (which we will get to later).

Here, “depth” is an **experimental operation definition**, which refers to turning this **construct** (concept) into conditions that can be applied to a research experimental design. Rating liking creates a higher level of depth by encouraging semantic engagement with the words. Counting vowels creates comparatively lower depth by focusing the participant on surface features of the word instead of meaning. The experiment is about how these conditions affect memory, which raises the next question.

What was the dependent variable?

The **dependent variable (DV)** in this experiment is a measured operational definition of memory, as in, how much memory did participants have of the word list after engaging with the work list in either of the experimental conditions. A measured operational definition turns a concept/construct into a quantitative number used to measure outcome. Here, the answer will be a numeric measure of performance on the recognition test that came at the end of the experimental protocol.

After going through the initial interaction with 30 words (“study phase”), you completed a short delay/distraction task based on answering trivia questions. Then you completed a recognition memory task in which you were presented with 60 works, the 30 you saw initially and 30 words that you did not see at the beginning. Note that you might be tempted to answer the question of “what is the DV?” with “the number of studied words you responded ‘old’ to on the test.” Here that is not quite correct as answering ‘old’ to all 60 words would not reflect good memory (because you called all the new words old). More accurate is to describe the DV as score on the recognition test, which we can count as the number of test items

Measuring Memory

If you are familiar with memory research, you might be familiar with more sophisticated ways to measure memory. A simple percent correct measure is enough for our simple study but not for all memory research.

responded to correctly (old called old, new called new).

State a hypothesis *relating the* independent variable *to the* dependent variable*.*

This is the first question that engages with the psychological science of the research study. The first few questions are just identifying the key terms as a basis for figuring out what the study might tell us about human thought or behavior. Stating **hypotheses** about experimental variables is a deceptively tricky task. It requires that the stated hypothesis be testable or falsifiable, which is not the same as correct.

Any statement relating the levels of the IV to scores on the DV are correct answers to a prompt like this. The hypothesis relating the experimental variables is: rating liking of words will lead to higher scores on the recognition test than counting vowels. Stating the opposite, that counting vowels will lead to higher recognition scores compared with rating liking is also an equally valid hypothesis, although we will see that it is false. That is, it is not supported by the data.

For the purpose of this question, stating the hypothesis in terms of the constructs would not be correct here. At some level, the experiment is about the hypothesis that deeper encoding of items being studied leads to better memory later. This is a perfectly valid hypothesis but in our analysis process we first focus specifically on how the experimental design tests a hypothesis about the experimental IV affecting the experimental DV.

An important part of analyzing an experiment is to find problems or errors in methodology. When we design studies, we need to consider our design critically to see if any errors have crept into our approach. And when we review research reports that we encounter and ask the question "how do they know that?" we should be looking for potential problems with the conclusions.

By explicitly framing the question in terms of the variables as asked here, we focus our attention on how the constructs of "deep processing," "shallow processing," and "memory" are implemented in this specific design. For example, memory here is operationally defined as a recognition test for the list of words. A statistically reliable result for this study allows us to make a confident statement about how this independent variable affected this dependent variable. However, extending the idea from this study to all other ways we might study memory is an additional step that we should consider carefully.

One of the important and unique aspects of psychological science is being aware of the difference between the experimental design and data, which are based on operational definitions, and the theoretical conclusions, which are based on constructs. In this design, the operational definitions led us to use lists of words as the things to be remembered and one specific approach to what we mean by "depth of encoding." These might be important **limitations** to consider about our conclusions, for example, do they apply to non-word stimuli, or how does depth influence other kinds of ways to measure memory?

The data obtained will tell us about the relationships of the variables we used in the experiment, pending the appropriate use of a statistical test to evaluate the relability of any effects observed. Following this, we hope to draw a theoretical inference about the constructs as the scientific conclusions about the study. Critically evaluating research requires being able to identify methodological issues that might limit those conclusions that arise at any step in the research process.

What statistical test would we use to establish a reliable relationship between our independent *and* dependent *variables that would allow us to test our* hypothesis?

Since this is a simple two group design with participants randomly assigned to one condition or the other, the most appropriate statistical test would be a **two independent samples t-test.** While other more powerful approaches could certainly be used, it is generally most effective to use the simplest test that effectively communicates the main findings.

Statistics are the bridge from your numeric, quantitative data to statements about the conclusions and meaning of your study. Our review of experimental methodology assumes prior familiarity with basic statistical methods from a prerequisite class. However, statistics will be used here in a potentially different manner than in prior classes.

For simple experimental design, where participants are randomly assigned to one or two conditions of one or two independent variables, questions of reliability are generally simple and often relatively uninteresting. Our use of statistics here will therefore be streamlined. We will focus on identifying the correct test to use from a constrained set of options and provide a recipe to carry out the analysis within the program R/RStudio. The result of the analysis will be reported in standard format (based on the American Psychological Association; APA) as part of the process of writing up the results of a study. While a strong foundational grasp of the underlying mathematics is always helpful, we will primarily focus on how statistics are used to test research hypotheses and how to report these in a result that is complete and comprehensible to other scientists.

In carrying out a research project, statistics are used to establish the **relability** of the effect of your IV on your DV. As we will see over the next several chapters, this is a separate question of the **validity** of your conclusions drawn from the study.

Psychology is a STEM field!

Research Methods is about using the scientific method to understand human behavior, attitudes, cognitive processes, social interactions, personality and mental health. It is fundamentally quantitative even though advanced math skills are not strictly necessary for basic design

We will consider these ideas of reliability and validity of research from two different perspectives as well. When carrying out research, scientists spend a great deal of time worrying about whether their study will produce a reliable result where reliability is defined as meeting specific statistical criteria. This is because for a study to have scientific impact, it needs to be reported and published in a peer-reviewed journal. The review process always involves an element of evaluating the reliability of the data, so if the results do not meet the field standard criteria, the result will not be published and available to other scientists to read and learn from. For experimental research, obtaining a reliable result requires a well-designed study much more than any statistical sophistication so techniques for effective design will be our main concern.

An additional important goal of the study of research methods is to be able to read and understand results that have been published. As a reader/consumer of science, reliability of the published results is generally less of a concern because the authors and reviewers have already judged that the data and analysis meet the standards for reliability. However, we will observe that when we ask "how do they know that?" about published research that we will identify validity challenges that may weaken or limit the conclusions drawn even from published research.

Much of your ability to identify weaknesses in scientific methodology will come from your understanding of human behavior as a human. In this class, we will augment this with some practice applying critical thinking skills systematically to these questions.

Psychology is the science of human behavior.

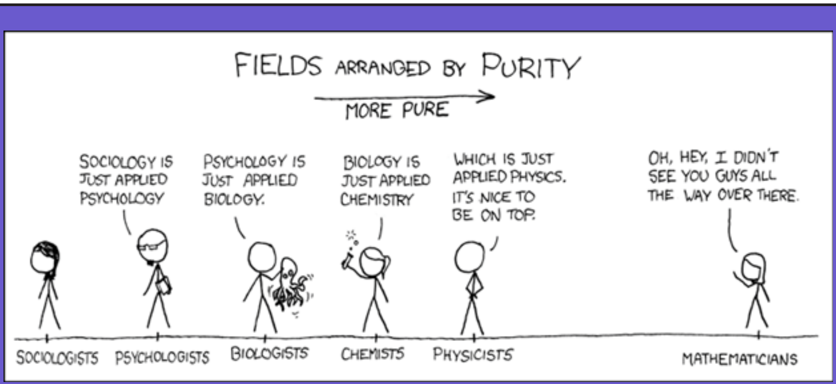
As a human, you have pretty good intuitions about how humans behave. Personal knowledge and experience can be a good starting point for the tricky problem of coming up with the operational definitions for psychological constructs.

Experiment 1

Our first experiment is based on some fairly old ideas in memory research but which hold up well for a simple demonstration experiment. The underlying ideas are described in Craik & Lockhart (1972), which lays out a **framework theory** for thinking about memory. Craik & Tulving (1975) reports a series of experiments that establish that manipulations designed to vary the "level of processing" or "depth of processing" have robust and reliable effects on measures of memory. While the core terminology and theoretical framing presented in these older papers is slightly out of date by more modern theories of memory function, the procedure still serves as an excellent example of a simple design that consistently produces a measureable effect.

Experiment 1 will be used to illustrate the typical path from theory through experimental design, data collection and analysis. We start with constructs like memory and a hypothesis, does deeper engagement with material lead to better memory? These are then turned into an experimental operational definition (liking and vowel counting) and measured operational definition (recognition test). Data are collected and will be analyzed. The statistical test will be used to allow us to support (or not) a statement about whether the IV reliably affected the DV. From there we will draw a final conclusion about how we think the original concepts are related and whether the data support the original hypothesis (or not).

XKCD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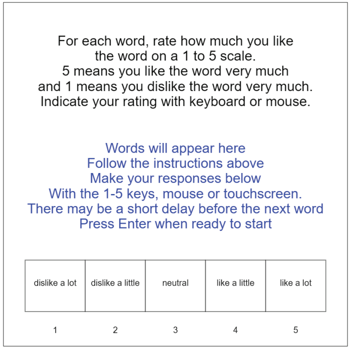
drawn by Randall Monroe, is an exceptionally sharp source of perspectives on science, https://xkcd.com/435/

Experiment 1 Design

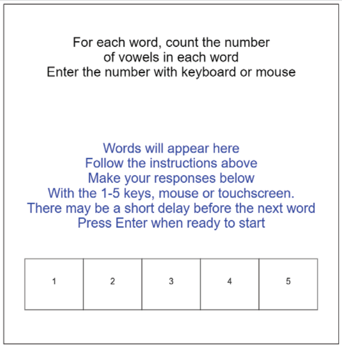
Random Assignment to Conditions either Deep or Shallow, which implements the study IV



For each word, rate how much you like the word on a 1 to 5 scale.
5 means you like the word very much
and 1 means you dislike the word very much.
Indicate your rating with keyboard or mouse.

Words will appear here
Follow the instructions above
Make your responses below
With the 1-5 keys, mouse or touchscreen.
There may be a short delay before the next word
Press Enter when ready to start

dislike a lot	dislike a little	neutral	like a little	like a lot
1	2	3	4	5

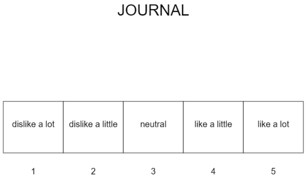


For each word, count the number
of vowels in each word
Enter the number with keyboard or mouse

Words will appear here
Follow the instructions above
Make your responses below
With the 1-5 keys, mouse or touchscreen.
There may be a short delay before the next word
Press Enter when ready to start

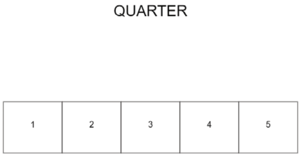
1	2	3	4	5

Although the instructions differ, every word is shown for 4s to maintain matched viewing time



JOURNAL

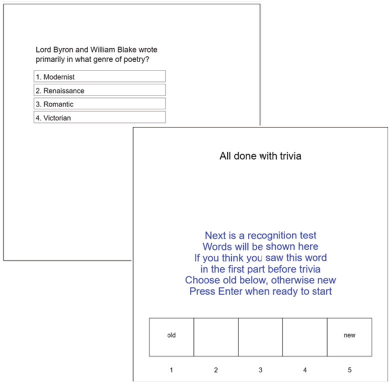
dislike a lot	dislike a little	neutral	like a little	like a lot
1	2	3	4	5



QUARTER

1	2	3	4	5

After performing the rating task, the rest of the study procedure is the same for both groups



Lord Byron and William Blake wrote primarily in what genre of poetry?

1. Modernist
2. Renaissance
3. Romantic
4. Victorian

All done with trivia

Next is a recognition test
Words will be shown here
If you think you saw this word
in the first part before trivia
Choose old below, otherwise new
Press Enter when ready to start

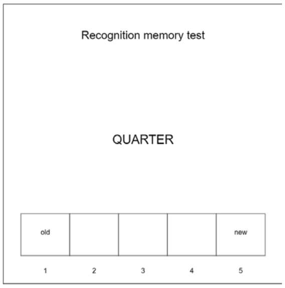
old				new
1	2	3	4	5

Everybody completes 3 minutes of trivia between seeing the words and the recognition test

Both Groups See the Same 30 Words

POCKET, PAINT, PRISON, QUARTER, CITIZEN, VEHICLE, ROUGH, BRAIN, TEMPLE, PRINCE, MEDICINE, FILLING, GUARD, JOURNAL, ENGINE, PALACE, GRAVE, BRANCH, CONCRETE, DANCER, SALARY, BASEMENT, MATCH, NATIVE, STABLE, FENCE, SWIMMING, QUEEN, OCEAN, FACTORY

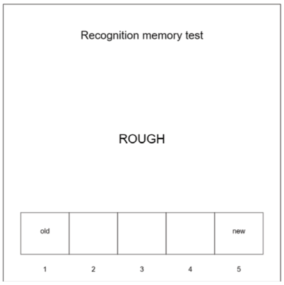
The study DV is obtained from the recognition test. In addition to the 30 studied words, 30 unstudied (new) words are shown.
All participants get the same test



Recognition memory test

QUARTER

old				new
1	2	3	4	5



Recognition memory test

ROUGH

old				new
1	2	3	4	5

Materials

A set of 60 words was used for the study and test stimuli. Words were seleted to have a written frequency of 30-80 per million and to be 5-8 letters in length.

30 Unstudied Words For Memory Test

CLOTH, DRIVER, LIQUID, SOLDIER, GALLERY, CHAIN, LUMBER, STEEL, MISSILE, SMOKE, AUTHOR, OWNER, PORCH, FISHING, SMELL, SHADOW, TRACK, GUEST, DISPLAY, MUSCLE, SPEAKER, WEDDING, WORKER, GUIDE, BRUSH, HIGHWAY, NAVAL, CARBON, PARTNER, PASSAGE

The recognition test is scored as percent correct of responses out of 60 possible. Calling a word seen in the first phase “Old” is correct. Calling a word not seen “New” is also correct.

Each participant obtains a score on the test and the average performance across the two groups is compared with a two independent samples t-test.

Some key experimental design terms

- **Experimental research**: The experimenter manipulates an independent variable and measures a dependent variable to observe whether the manipulation has an effect.
- **Construct**: The high-level concepts we aim to do research about. Typically, these things we have an intuitive understanding of but that have to be translated into specific experiment elements.
- **Operational definition**: Turning an intuitive but imprecise concept into something that can be measured quantitatively, or controlled categorically. For example, we have a sense of what "memory" is, but for experimental design we need a numerical implementation of the amount of memory a participant has in order to measure how memory is affected in a study.
- **Measured operational definition**: A quantitative measure of a construct, essentially turning an idea into something that can be characterized as a number. For example when we turn "memory" into a number, or a construct like anxiety, impulsiveness, attention. These measures are used as dependent variables in experimental design.
- **Experimental operational definition**: A controlled method of implementing a specific definition of a construct into levels or categories that can be manipulated by an experimenter in order to create the independent variable(s) for an experiment protocol.
- **Independent variable (IV)**: Often referred to by the acronym IV, this is factor manipulated by the experimenter to see if or how it affects the measure being collected in an experimental design. Controlled manipulation of the IV is the defining feature of experimental research.
- **Dependent variable (DV)**: Frequently referred to by the acronym DV, this is the measurement collected by the experimenter. The core idea in experimental reasearch is to see how the scores on the DV change across the manipulation of the IV. If they do, we can conclude that the IV affected the DV.
- **Experimental Hypothesis**: A statement about the relationship between experimental variables that can be tested and importantly, falsified. If there are no data that would render a statement false, then it is not a falsifiable statement and is typically a description rather than a hypothesis. Typically the hypothesis is that the IV

affects the DV, and we use statistics to reject the **null hypothesis** (that the IV does not affect the DV). Note that hypotheses can be stated about the specific IV and DV used in an experiment but also stated separately about the constructs from which the IV and DV were operationally defined. Experimental data gives us confidence to make statements about about the specific IV affecting the implemented DV but the goal of research is to draw inferences about the relationship among the constructs.

- **Limitations**: Concerns that conclusions about the underlying constructs might not be true in all cases and conditions other than the specific operational definitions used in the experimental design. Generally these are not issues with the fundamental validity of the experiment, e.g. if there were a confounding variable (Chapter 3), but questions about how widely the results can be applied. Identifyinf what limitations should be considered often requires some knowledge of the underlying theoretical ideas for a research study and can also indicate directions for future research. Using Experiment 1 as an example, we might note that the results used memory for word lists measured with a recognition test a few minutes later. We might wonder if deeper encoding similarly affects memory for pictures or if the effect might change with another measure of memory like recall. Studies examining those questions would reflect different operational defintions of memory to use a different DV and different operational defintions of deeper encoding as IV.
- **Statistical reliability**: We will evaluate whether the IV has a robust effect on the DV using standard statistical tools. Our focus here will be selecting the correct tool and reporting the use of the tool accurately. Statistics are often presented as a simple binary outcome: did the IV affect the DV reliably, can we reject the null hypothesis, was the probability of the null less than the criterion of .05 (these three statemetns are essentially synonymous). However, we will see that Psychological Science is moving towards a model of reporting **effect size** rather than relying on these binary descriptions. The effect size is helpful both with understanding the reliability of the statistics and also communicating the results. For Experiment 1, we might want to be able to say not just that deep encoding improved memory, but how much did this study approach increase our measure of memory?

Experimental vs Non-Experimental Research

A useful approach for understanding the definition of something complex, like experimental research, is to define what isn't experimental research. In non-experimental research, we also look for a relationship between an independent variable and a dependent variable, but the independent variable is not manipulated or controlled by the experimenter. For example, we could look for a correlation between your GPA and the score on the memory test in the demonstration experiment.

Non-experimental research is a powerful tool for psychological science as well as fields such as epidemiology, economics and sociology. However, the methods of the design of research studies and tools for analysis of data for non-experimental methods are quite different. The current approach focuses on experimental methods first, followed by some discussion of contrasting these methodologies for general reference in Chapters 9-11.

Experimental research has a significant advantage in drawing conclusions about how a manipulated variable (IV) affects a measured variable (DV). If we manage the challenge of adequate experimental control (Chapter 3-4) we can be fairly confident that changes in our DV were caused by our manipulation of the IV. However, experimental design is limited by needing conditions where we can create effective and accurate operational definitions of the constructs we want to study so that we can implement a protocol for a well-controlled laboratory experiment. There are a lot of important and interesting questions in Psychology that rely on data collected from the world in imperfectly controlled conditions.

Non-experimental research typically fights against the “correlation is not causation” problem and frequently uses more advanced quantitative analytic tools to improve our ability to draw causation from these data.

Experimental research uses simpler methodology and simpler analytic tools, making it an effective introduction to the design of psychological research.

Experimental Analysis

The following questions will be asked regularly about example designs and findings from psychological research. These will train your intuition to identify strengths and weaknesses of designs from short research descriptions. Later we will see how to read and write formal research reports following APA guidelines. Most of the research that you encounter will be in more informal context, but you can still ask the question: **How do they know that?**

- What is the experiment about?
- What is the dependent variable?
- What is the independent variable?
- What is the hypothesis or finding about how the IV affects the DV?
- What statistical test is used to establish a reliable effect?
- What is the conclusion drawn by the researcher?
- Do we see any problems with this inference?

Trying to identify the hypothesis and potential problems with the inference are the hardest but most important questions from this list. If there was a tried-and-true approach to always identify inference errors, professional researchers would never make mistakes about their findings (spoiler alert: they do).

The first three questions depend on the operational definitions used by the researchers and how well they capture the intent of the research. When there is a mismatch, this often reflects differences in how people understand common phrases. For example, we might want to test a hypothesis related to an adage like “time flies when you are having fun.” One of the first challenges we would face is how to define the constructs of “time flies” and “having fun.” Different researchers would likely define these ideas in different ways and rather than saying that some operational definitions are right or wrong, it is important to understand that the different definitions reflect different design ideas. Experiments with different definitions might be quite properly constructed, but the conclusions drawn from carrying out the study might end up being very different.

Chapter 2 will discuss operational definitions as an example of Measurement Theory Mistakes in operational definition are one important source of error in experimental design. These can lead to studies where the results are quite robust, the IV clearly strongly affects the DV, yet the main conclusion of the study is inaccurately stated because the variables are ineffective operational definitions of the constructs they were intended to capture.

The question of what statistical test is appropriate for the research is necessarily more technical. As noted above, this class assumes background in basic statistics. In Chapters 5 and 10, we will review the process of selecting and carrying out the appropriate statistical tests for common experimental designs. The focus here is knowing which analysis to use, how to carry out the basic analysis procedure and most importantly, accurately state the inferences the analysis supports.

Understanding the hypothesis and conclusions that are tied to the IV and DV, the specific operational definitions used in an experimental design is the key to ensuring you understand how to read and interpret scientific findings. Being an effective reader of science and understanding what is confidently learned from the data obtained in a psychological study is a major goal of this class and text.

Experimental Analysis Practice Examples

Practicing experimental analysis and learning the common types of research design will give you critical thinking tools to help strengthen your understanding of science. We will practice via example throughout class meetings with a daily example to evaluate and analyze.

Example 1

Time flies when you're having fun, but what is it about pleasant experiences that makes time seem to go by faster? In one experiment inspired by prior work (Gable & Poole, 2012), researchers tested the hypothesis that approach motivation causes perceptual shortening of time during pleasant experiences. That is, it isn't just positive affect (fun), time goes quickly when you are specifically motivated to obtain a reward. Thus, they predicted that time spent viewing pictures of "delicious desserts" would appear to go by particularly quickly if you expected to get to eat one of the desserts after the experiment.

Participants were randomly assigned to either be told they would get to eat a dessert after the experiment or not. Then they each looked at 36 pictures of desserts each presented for a 12s and rated a scale of 1 (time dragged) to 7 (time flew), how long the picture had been presented.

Go through the Experiment Analysis questions for this example

What is the experiment about?	The subjective experience of time passing
What is the dependent variable?	The numerical scale rating from 1 to 7 of whether time dragged or time flew
What is the independent variable?	Told they would get a dessert after the study or not
What is the hypothesis or finding about how the IV affects the DV?	Participants told they would get dessert would score higher on the DV reflecting a feeling that time flew
What statistical test would be used to establish a reliable effect?	Two independent samples t-test

If the data were consistent with the hypothesis such that scores on the time-passing rating scale were higher for the participants who expected a reward, the researchers would like to conclude that expecting reward makes time feel like it is passing more quickly.

We should always consider limitations of the broad level conclusion. We might note that the task is particularly dull but also intrinsically linked to the reward (both are related to eating dessert). We might also note that the conclusion does not argue against the idea that time flies when you are having fun, but only suggests time might also fly when you expect dessert.

Example 2

Martin hypothesizes that self-esteem affects snacking behavior. He thinks that low self esteem will leads to increased opportunistic eating. He conceives of the following experiment. A group of 50 participants is recruited. All are given the opportunity to play a game of chance. They are all told that the odds are in their favor and that 90% of the people who play win the game. However, they are really assigned randomly to two groups: half win and half lose. The winners are congratulated and the losers are told, "Wow, that's really unlucky. You must be a really unlucky person. Do you lose a lot of games like this?" Afterwards, all participants are then left alone in a room with a full bowl of peanuts for 15 minutes. The average weight of peanuts eaten during this period is compared for the 2 groups.

What is the experiment about?	Snacking behavior, self-esteem
What is the dependent variable?	Weight of peanuts eaten
What is the independent variable?	Whether the participants were made to feel that they were lucky or not
What is the hypothesis or finding about how the IV affects the DV?	Being told they were unlucky would lead to lower self-esteem and increase the number of peanuts eaten
What statistical test would be used to establish a reliable effect?	Two independent samples t-test

If the data were consistent with the hypothesis, the group randomly assigned to lose and be told they were unlucky would have consumed more of the peanuts left with the participants. This result could be statistically reliable but we might still have concerns about the broader conclusions. We would want to be confident that the experimental manipulation really did affect self-esteem. An **alternate explanation** for the results would be that feeling unlucky leads to greater snacking, without involving perceptions of the self that incorporate self-esteem. The existence of this alternate account for explaining the result does not mean the conclusion is wrong, it simply means that there is more than one way of understanding the data from the experiment and we do not yet know which is correct. These situations are often good opportunities for future research with novel operational definitions of the underlying construct. Note that such an **alternate hypothesis** for the data do not imply the results were not reliable, but that there is a question or limiation about the validity of the conclusion about the constructs. To highlight this different, it is best to separately consider the results of the experiment both in terms of the actual variables (IV, DV) and then the inference in terms of the intended constructs.

We've dived into experimental design and analysis very rapidly here and introduced a fairly large vocabulary of critical terms and concepts very quickly. If that seems daunting, don't worry! We will be going back over the concepts in detail to ensure a solid foundation of methodology design principles across a range of common approaches and research areas. If that seems too easy because design is straightforward, don't worry! While simple designs are easy, it gets complicated fast. If it were really easy, trained and professional researchers wouldn't make mistakes in their research conclusions (spoiler alert: they do).

Exercises

Read Craik & Lockhart (1972) to orient you to the background theory behind our hypothesis for Experiment 1.

It is worth noting that this is a fairly old paper that reflects the theoretical understanding at that time. The "levels of processing" theory is presented as an alternative to "multistore models." In modern memory research, elements of both theoretical ideas turn out to be true and the two approaches are not seen as inconsistent with each other.

The description and data of the multistore models reflects studies done prior to 1972. It is a useful overview, but if you are interested in the general topic of studies of memory, be aware that is a historical overview from a very long time ago. Characterization of the new ideas related to 'levels of processing' comes after this review in the paper.

Answer the following questions from the reading:

1. What is 'depth of processing' and why might it lead to better memory?
2. In our study, how would our definition of 'deep encoding' connect to this theoretical idea?
3. In our study, how does our definition of 'shallow encoding' provide a control comparison?
4. From the prior work cited (e.g., p 677), give an example of how researchers have implemented a different procedure to create shallow encoding.
5. Give another example of a procedure to create deep encoding from the briefly reviewed prior work.

2 Psychological Measurement

Researchers Tara MacDonald and Alanna Martineau were interested in the effect of female university students' moods on their intentions to have unprotected sexual intercourse (MacDonald & Martineau, 2002). In a carefully designed empirical study, they found that being in a negative mood increased intentions to have unprotected sex—but only for students who were low in self-esteem. Although there are many challenges involved in conducting a study like this, one of the primary ones is the measurement of the relevant variables. In this study, the researchers needed to know whether each of their participants had high or low self-esteem, which of course required measuring their self-esteem. They also needed to be sure that their attempt to put people into a negative mood (by having them think negative thoughts) was successful, which required measuring their moods. Finally, they needed to see whether self-esteem and mood were related to participants' intentions to have unprotected sexual intercourse, which required measuring these intentions.

To students who are just getting started in psychological research, the challenge of measuring such variables might seem insurmountable. Is it really possible to measure things as intangible as self-esteem, mood, or an intention to do something? The answer is a resounding yes, and in this chapter, we look closely at the nature of the variables that psychologists study and how they can be measured.

Do You Feel You Are a Person of Worth?

The Rosenberg Self-Esteem Scale (Rosenberg, 1989) is a common measure of self-esteem and the one that MacDonald and Martineau used in their study. The goal of this scale is to take the construct "self-esteem" and turn this into a number that reflects a quantitative measure of a participant's subjective rating of this idea.

To obtain this measure, participants are asked to respond to each of the 10 items that follow with a rating on a 4-point scale: Strongly Agree, Agree, Disagree, Strongly Disagree.

1. I feel that I'm a person of worth, at least on an equal plane with others.
2. I feel that I have a number of good qualities.
3. All in all, I am inclined to feel that I am a failure.
4. I am able to do things as well as most other people.
5. I feel I do not have much to be proud of.
6. I take a positive attitude toward myself.
7. On the whole, I am satisfied with myself.
8. I wish I could have more respect for myself.
9. I certainly feel useless at times.
10. At times I think I am no good at all.

The responses are then use to calculate a total score based on the responses to each item. A response of Strongly Agree is counted as 3 points, Agree is 2 points, Disagree is 1 point and 0 for Strongly Disagree. Items 1, 2, 4, 6 and 7 are scored this straighforward way. Notice that items 3, 5, 8, 9 and 10 have statements that are conceptually backwards, that is, agreeing reflects less self esteem. For these items we reverse the scoring before calculating the total across all the items. The final number is a value that is higher for participants who have greater self-esteem and we have turned this relatively absract construct into a quantitative value we can use for scientific research.

In the previous chapter, we introduced the idea of using quantitative measusres as variables for experimental design. In this chapter we will focus on the the measurement process of creating or identifying these quantitative measures to use in those designs. For a measure like this example, the score could be used in design as a dependent variable. For example, participants could be asked to complete the scale after manipulating an independent variable that was thought to have a temporary effect on self-esteem. We could also use this measure as an independent variable where it would be a special type of IV, called a **participant variable**, where we would test a hypothesis about participats with relatively higher or lower self-esteem on some other dependent variable (as in the cited study above). For a measure like this, we often use a technique called a median split to sort our participants into groups with higher or lower scores.

Participant variables are fairly commonly used and act like independent variables in experimental design and drawing inferences, but should be noted that these cannot be manipulated by the experimenter. They often reflect intrinsic characteristics of the participants that are hypothesized to affect the dependent variable of interest.

Surveys

Surveys are a familiar methodology by which we turn concepts into numbers. While they look deceptively simple to construct, there is a lot of work that goes into establishing that a specific survey is an effective measure of the intended construct.

Developing a new, robust scale that reliably measures a construct is beyond the scope of what can be covered in basic research methods. A overview of this process is provided in Chapter 15. For student research, use of an existing scale from published research is strongly recommended.

Learning Objectives

1. Define measurement and give several examples of measurement in psychology.
2. Explain what a psychological construct is and give several examples.
3. Distinguish conceptual from operational definitions, give examples of each, and create simple operational definitions.
4. Distinguish the four levels of measurement, give examples of each, and explain why this distinction is important.

What Is Measurement?

Measurement is the assignment of scores to individuals so that the scores represent some characteristic of the individuals. This very general definition is consistent with the kinds of measurement that everyone is familiar with—for example, weighing oneself by stepping onto a bathroom scale, or checking the internal temperature of a roasting turkey using a meat thermometer. It is also consistent with measurement in the other sciences. In physics, for example, one might measure the potential energy of an object in Earth's gravitational field by finding its mass and height (which of course requires measuring those variables) and then multiplying them together along with the gravitational acceleration of Earth (9.8 m/s2). The result of this procedure is a score that represents the object's potential energy.

This general definition of measurement is consistent with measurement in psychology too. Psychological measurement is often referred to as psychometrics. Imagine, for example, that a cognitive psychologist wants to measure a person's working memory capacity—their ability to hold in mind and think about several pieces of information all at the same time. To do this, she might use a backward digit span task, in which she reads a list of two digits to the person and asks them to repeat them in reverse order. She then repeats this several times, increasing the length of the list by one digit each time, until the person makes an error. The length of the longest list for which the person responds correctly is the score and represents their working memory capacity. Or imagine a clinical psychologist who is interested in how depressed a person is. He administers the Beck Depression Inventory, which is a 21-item self-report questionnaire in which the person rates the extent to which they have felt sad, lost energy, and experienced other symptoms of depression over the past 2 weeks. The sum of these 21 ratings is the score and represents the person's current level of depression.

The important point here is that measurement requires some systematic procedure for assigning scores to individuals or objects so that those scores represent the characteristic of interest.

Psychological Constructs

Many variables studied by psychologists are straightforward and simple to measure. These include age, height, weight, and birth order. You can ask people how old they are and be reasonably sure that they know and will tell you. Although people might not know or want to tell you how much they weigh, you can have them step onto a bathroom scale. Other variables studied by psychologists—perhaps the majority—are not so straightforward or simple to measure. We cannot accurately assess people's level of intelligence by looking at them, and we certainly cannot put their self-esteem on a bathroom scale. These kinds of variables are called constructs (pronounced CON-structs) and include personality traits (e.g., extraversion), emotional states (e.g., fear), attitudes (e.g., toward taxes), and abilities (e.g., athleticism).

Psychological constructs cannot be observed directly. One reason is that they often represent tendencies to think, feel, or act in certain ways. For example, to say that a particular university student is highly extraverted does not necessarily mean that she is behaving in an extraverted way right now. In fact, she might be sitting quietly by herself, reading a book. Instead, it means that she has a general tendency to behave in extraverted ways (e.g., being outgoing, enjoying social interactions) across a variety of situations. Another reason psychological constructs cannot be observed directly is that they often involve internal processes. Fear, for example, involves the activation of certain central and peripheral nervous system structures, along with certain kinds of thoughts, feelings, and behaviors—none of which is necessarily obvious to an outside observer. Notice also that neither extraversion nor fear "reduces to" any particular thought, feeling, act, or physiological structure or process. Instead, each is a kind of summary of a complex set of behaviors and internal processes.

Ethics

By diving straight into experimental design, we have taken on the idea of measurement without establishing a foundation for how researchers need to consider ethical aspects of psychological science.

There are many measures of individuals that are invasive of the *privacy* of the participant in research. These must be administered with great care for the rights of participants in human research. Questions about constructs related to mental health are often relevant to scientific hypotheses in psychological science. These questions must be administered within the framework of ethical research and with oversight from the Intritutaionl Review Board.

Chapter 8 will review ethical research procedures in detail.

Conceptually Defining the Construct

Having a clear and complete conceptual definition of a construct is a prerequisite for good measurement. For one thing, it allows you to make sound decisions about exactly how to measure the construct. If you had only a vague idea that you wanted to measure people's "memory," for example, you would have no way to choose whether you should have them remember a list of vocabulary words, a set of photographs, a newly learned skill, an experience from long ago, or have them remember to perform a task at a later time. Because psychologists now conceptualize memory as a set of semi-independent systems, you would have to be more precise about what you mean by "memory." If you are interested in long-term episodic memory (memory for previous experiences), then having participants remember a list of words that they learned last week would make sense, but having them try to remember to execute a task in the future would not. In general, there is no substitute for reading the research literature on a construct and paying close attention to how others have defined it.

Example: Personality and The Big Five

The Big Five is a set of five broad dimensions that capture much of the variation in human personality. Each of the Big Five can even be defined in terms of six more specific constructs called "facets" (Costa & McCrae, 1992): Openness, Conscientiousness, Extroversion, Agreeableness, and Neuroticism.

The conceptual definition of a psychological construct describes the behaviors and internal processes that make up that construct, along with how it relates to other variables. For example, a conceptual definition of neuroticism (another one of the Big Five) would be that it is people's tendency to experience negative emotions such as anxiety, anger, and sadness across a variety of situations. This definition might also include that it is hypothesized to have a strong genetic component, is required to remain fairly stable over time, and is positively correlated with other types of measurements, such as the tendency to experience pain and other physical symptoms.

Students sometimes wonder why, when researchers want to understand a construct like self-esteem or neuroticism, they do not simply look it up in the dictionary. One reason is that many scientific constructs do not have counterparts in everyday language (e.g., working memory capacity). More important, researchers are in the business of developing definitions that are more detailed and precise—and that more accurately describe the way the world is—than the informal definitions in the dictionary. As we will see, they do this by proposing conceptual definitions, testing them empirically, and revising them as necessary. Sometimes they throw them out altogether. This is why the research literature often includes different conceptual definitions of the same construct. In some cases, an older conceptual definition has been replaced by a newer one that fits and works better. In others, researchers are still in the process of deciding which of various conceptual definitions is the best.

Operational Definitions

Once you have a conceptual definition of the construct you are interested in studying, it is time to operationally define the construct. Recall an operational definition is a definition of the variable in terms of precisely how it is to be measured. Since most variables are relatively abstract concepts that cannot be directly observed (e.g., stress), and observation is at the heart of the scientific method, conceptual definitions must be transformed into something that can be directly observed and measured. Most variables can be operationally defined in many different ways. For example, stress can be operationally defined as people's scores on a stress scale such as the Perceived Stress Scale (Cohen, Kamarck, & Mermelstein, 1983), cortisol concentrations in their saliva, or the number of stressful life events they have recently experienced. As described below, operationally defining your variable(s) of interest may involve using an existing measure or creating your own measure.

An operational definition is a definition of a variable in terms of precisely how it is to be measured. These measures generally fall into one of three broad categories. Self-report measures are those in which participants report on their own thoughts, feelings, and actions, as with the Rosenberg Self-Esteem Scale (Rosenberg, 1965). Behavioral measures are those in which some other aspect of participants' behavior is observed and recorded. This is an extremely broad category that includes the observation of people's behavior both in highly structured laboratory tasks and in more natural settings. A good example of the former would be measuring working memory capacity using the backward digit span task. A good example of the latter is a famous operational definition of physical aggression from researcher Albert Bandura and his colleagues (Bandura, Ross, & Ross, 1961). They let each of several children play for 20 minutes in a room that contained a clown-shaped punching bag called a Bobo doll. They filmed each child and counted the number of acts of physical aggression the child committed. These included hitting the doll with a mallet, punching it, and kicking it. Their operational definition, then, was the number of these specifically defined acts that the child committed during the 20-minute period. Finally, physiological measures are those that involve recording any of a wide variety of physiological processes, including heart rate and blood pressure, galvanic skin response, hormone levels, and electrical activity and blood flow in the brain.

For any given variable or construct, there will be multiple operational definitions. Stress is a good example. A rough conceptual definition is that stress is an adaptive response to a perceived danger or threat that involves physiological, cognitive, affective, and behavioral components. But researchers have operationally defined it in several ways. The Social Readjustment Rating Scale (Holmes & Rahe, 1967) is a self-report questionnaire on which people identify stressful events that they have experienced in the past year and assigns points for each one depending on its severity. For example, a man who has been divorced (73 points), changed jobs (36 points), and had a change in sleeping habits (16 points) in the past year would have a total score of 125. The Hassles and Uplifts Scale (Delongis, Coyne, Dakof, Folkman & Lazarus, 1982) is similar but focuses on everyday stressors like misplacing things and being concerned about one's weight. The Perceived Stress Scale (Cohen, Kamarck, & Mermelstein, 1983) is another self-report measure that focuses on people's feelings of stress (e.g., "How often have you felt nervous and stressed?"). Researchers have also operationally defined stress in terms of several physiological variables including blood pressure and levels of the stress hormone cortisol.

When psychologists use multiple operational definitions of the same construct—either within a study or across studies—they are using converging operations. The idea is that the various operational definitions are "converging" or coming together on the same construct. When scores based on several different operational definitions are closely related to each other and produce similar patterns of results, this constitutes good evidence that the construct is being measured effectively and that it is useful. The various measures of stress, for example, are all correlated with each other and have all been shown to be correlated with other variables such as immune system functioning (also measured in a variety of ways) (Segerstrom & Miller, 2004). This is what allows researchers eventually to draw useful general conclusions, such as "stress is negatively correlated with immune system functioning," as opposed to more specific and less useful ones, such as "people's scores on the Perceived Stress Scale are negatively correlated with their white blood counts."

Experiment 1

For example, in the in-class experiment, we measured 'memory' by score on a recognition test where you saw a list of words and for each one responded whether you had seen it before. This produces a numerical measure of memory in the number of correct answers. However, it is fair to also say that there are lot of other ways to think about memory. Memory can refer to being able to recount the events of an experience you had yesterday. Another common way to measure memory is via tests of recall, e.g., asking participants to report all the words they had seen during the original study phase. This would also produce a quantitative measure of memory for the word list. In more advanced memory research, there are theoretical questions about how recognition and recall memory may be influenced by underlying mechanisms that might be specific to those processes. Recalling words seems to depend on something like "searching" our memories that might not be part of the process of deciding if you recognize a word seen before.

It would also be fair to say that any measure of memory for a list of arbitrary, unrelated words fails to capture important ideas that people are interested in that relate to the concept of "memory." One of the most common complains about memory is memory failures, such as the challenging issue of remembering somebody's name after you meet them. People will also have the experience of walking into a room and forgetting why you went into the room, which is also described as a failure of memory. Understanding factors that affect memory for lists of words may inform our understanding of these kinds of memory failures, but the distance from the operational definition employed in our experiment to those applications should be noted in considering the meaning of our findings.

All forms of science employ measurement, but the idea of the distance from the operational definition to the underlying concept is somewhat unique to psychological science. In other areas like biology, chemistry, or physics it is more commonly the case that there is less debate about what is being measured exactly. Because psychology is the science of people, we have the advantage of intuition and a basic understanding of the high-level concepts. We all know what words like 'memory' or 'anxiety' mean. However, when we design experiments or read about others' experimental work, we need to identify more precise definitions that turns these conceptual ideas into numbers. This also highlights the complexity of a word like "memory" and the associated challenge of indicating exactly what aspect of memory is being incorporated into the operational definition. This complexity is also why much modern psychological research uses increasingly specific and precise terminology to capture sub-areas of interest. For example, if you are interested in research aimed at understanding the phenomenon of forgetting why you walked into a room, you will want to look for research on "prospective memory," which is built around operational definitions based on memory for intentions to carry out actions and when that process surprisingly fails.

The process of establishing operational definitions applies to the process of setting up both the independent and dependent variables for a study. Many of the terms used to describe the key ideas in "measurement" apply more obviously to the dependent variable. For our basic experimental design, we expect the dependent variable to be a measured operational definition, which is a quantitative number that changes in a direction that can be conceptually connected to the construct. For our Experiment 1, more words recognized is clearly associated with more memory. It is also fine to consider measures that move the other direction, such as a measure like reaction time (speed to make a response) which tends to go down as a reflection of more knowledge. In communication about research, it is necessary to be clear about the details of the type and direction used for measurement.

Levels of Measurement

The psychologist S. S. Stevens suggested that scores can be assigned to individuals in a way that communicates more or less quantitative information about the variable of interest (Stevens, 1946). For example, the officials at a 100-m race could simply rank order the runners as they crossed the finish line (first, second, etc.), or they could time each runner to the nearest tenth of a second using a stopwatch (11.5 s, 12.1 s, etc.). In either case, they would be measuring the runners' times by systematically assigning scores to represent those times. But while the rank ordering procedure communicates the fact that the second-place runner took longer to finish than the first-place finisher, the stopwatch procedure also communicates how much longer the second-place finisher took. Stevens actually suggested four different levels of measurement (which he called "scales of measurement") that correspond to four types of information that can be communicated by a set of scores, and the statistical procedures that can be used with the information.

The **nominal** level of measurement is used for categorical variables and involves assigning scores that are category labels. Category labels communicate whether any two individuals are the same or different in terms of the variable being measured. For example, if you ask your participants about their marital status, you are engaged in nominal-level measurement. Or if you ask your participants to indicate which of several ethnicities they identify themselves with, you are again engaged in nominal-level measurement. The essential point about nominal scales is that they do not imply any ordering among the responses. For example, when classifying people according to their favorite color, there is no sense in which green is placed "ahead of" blue. Responses are merely categorized. Nominal scales thus embody the lowest level of measurement.

The remaining three levels of measurement are used for quantitative variables. The **ordinal** level of measurement involves assigning scores so that they represent the rank order of the individuals. Ranks communicate not only whether any two individuals are the same or different in terms of the variable being measured but also whether one individual is higher or lower on that variable. For example, a researcher wishing to measure consumers' satisfaction with their microwave ovens might ask them to specify their feelings as either "very dissatisfied," "somewhat dissatisfied," "somewhat satisfied," or "very satisfied." The items in this scale are ordered, ranging from least to most satisfied. This is what distinguishes ordinal from nominal scales. Unlike nominal scales, ordinal scales allow comparisons of the degree to which two individuals rate the variable. For example, our satisfaction ordering makes it meaningful to assert that one person is more satisfied than another with their microwave ovens. Such an assertion reflects the first person's use of a verbal label that comes later in the list than the label chosen by the second person.

On the other hand, ordinal scales fail to capture important information that will be present in the other levels of measurement we examine. In particular, the difference between two levels of an ordinal scale cannot be assumed to be the same as the difference between two other levels (just like you cannot assume that the gap between the runners in first and second place is equal to the gap between the runners in second and third place). In our satisfaction scale, for example, the difference between the responses “very dissatisfied” and “somewhat dissatisfied” is probably not equivalent to the difference between “somewhat dissatisfied” and “somewhat satisfied.” Nothing in our measurement procedure allows us to determine whether the two differences reflect the same difference in psychological satisfaction. Statisticians express this point by saying that the differences between adjacent scale values do not necessarily represent equal intervals on the underlying scale giving rise to the measurements. (In our case, the underlying scale is the true feeling of satisfaction, which we are trying to measure.)

The **interval** level of measurement involves assigning scores using numerical scales in which intervals have the same interpretation throughout. As an example, consider either the Fahrenheit or Celsius temperature scales. The difference between 30 degrees and 40 degrees represents the same temperature difference as the difference between 80 degrees and 90 degrees. This is because each 10-degree interval has the same physical meaning (in terms of the kinetic energy of molecules).

Interval scales are not perfect, however. In particular, they do not have a true zero point even if one of the scaled values happens to carry the name “zero.” The Fahrenheit scale illustrates the issue. Zero degrees Fahrenheit does not represent the complete absence of temperature (the absence of any molecular kinetic energy). In reality, the label “zero” is applied to its temperature for quite accidental reasons connected to the history of temperature measurement. Since an interval scale has no true zero point, it does not make sense to compute ratios of temperatures. For example, there is no sense in which the ratio of 40 to 20 degrees Fahrenheit is the same as the ratio of 100 to 50 degrees; no interesting physical property is preserved across the two ratios. After all, if the “zero” label were applied at the temperature that Fahrenheit happens to label as 10 degrees, the two ratios would instead be 30 to 10 and 90 to 40, no longer the same! For this reason, it does not make sense to say that 80 degrees is “twice as hot” as 40 degrees. Such a claim would depend on an arbitrary decision about where to “start” the temperature scale, namely, what temperature to call zero (whereas the claim is intended to make a more fundamental assertion about the underlying physical reality).

In psychology, the intelligence quotient (IQ) is often considered to be measured at the interval level. While it is technically possible to receive a score of 0 on an IQ test, such a score would not indicate the complete absence of IQ. Moreover, a person with an IQ score of 140 does not have twice the IQ of a person with a score of 70. However, the difference between IQ scores of 80 and 100 is the same as the difference between IQ scores of 120 and 140.

Finally, the **ratio** level of measurement involves assigning scores in such a way that there is a true zero point that represents the complete absence of the quantity. Height measured in meters and weight measured in kilograms are good examples. So are counts of discrete objects or events such as the number of siblings one has or the number of questions a student answers correctly on an exam. You can think of a ratio scale as the three earlier scales rolled up in one. Like a nominal scale, it provides a name or category for each object (the numbers serve as labels). Like an ordinal scale, the objects are ordered (in terms of the ordering of the numbers). Like an interval scale, the same difference at two places on the scale has the same meaning. However, in addition, the same ratio at two places on the scale also carries the same meaning (see Table 4.1).

The Fahrenheit scale for temperature has an arbitrary zero point and is therefore not a ratio scale. However, zero on the Kelvin scale is absolute zero. This makes the Kelvin scale a ratio scale. For example, if one temperature is twice as high as another as measured on the Kelvin scale, then it has twice the kinetic energy of the other temperature.

Another example of a ratio scale is the amount of money you have in your pocket right now (25 cents, 50 cents, etc.). Money is measured on a ratio scale because, in addition to having the properties of an interval scale, it has a true zero point: if you have zero money, this actually implies the absence of money. Since money has a true zero point, it makes sense to say that someone with 50 cents has twice as much money as someone with 25 cents.

Stevens's levels of measurement are important for at least two reasons. First, they emphasize the generality of the concept of measurement. Although people do not normally think of categorizing or ranking individuals as measurement, in fact, they are as long as they are done so that they represent some characteristic of the individuals. Second, the levels of measurement can serve as a rough guide to the statistical procedures that can be used with the data and the conclusions that can be drawn from them. With nominal-level measurement, for example, the only available measure of central tendency is the mode. With ordinal-level measurement, the median or mode can be used as indicators of central tendency. Interval and ratio-level measurement are typically considered the most desirable because they permit for any indicators of central tendency to be computed (i.e., mean, median, or mode). Also, ratio-level measurement is the only level that allows meaningful statements about ratios of scores. Once again, one cannot say that someone with an IQ of 140 is twice as intelligent as someone with an IQ of 70

Data Analysis

Measures that vary in the type and levels will also determine what kind of statistical approach is correct to use for a specific design.

Chapter 5, 12 and 16 will review different statistical methods for cases where there are IV's and DV's that have different levels of measurement.

The most common type of experimental design is to use an interval or ratio measure for the DV and a nominal measure for the IV. Experiment 1 is designed this way.

because IQ is measured at the interval level, but one can say that someone with six siblings has twice as many as someone with three because number of siblings is measured at the ratio level.

Reliability and Validity of Operational Definitions

Developing a novel measure of a construct that consistently and accurately numerically captures a complex construct is a complex and time-consuming task. We will discuss the general methodology for this later (Chapter 17, Surveys and Instrument Design) since this process is more often engaged with as part of non-experimental research than experimental research and is also generally outside the scope of this introductory class on psychological science. However, drawing inferences about experimental data will require considering how well the operational definition captures the underlying construct. Misalignment between the operational definition and the construct can lead to problems with inferences about the construct or can limit the applicability of findings to contexts outside the laboratory.

In the context of measurement, reliability refers to how consistently the measure obtains an accurate assessment of the underlying construct. For example, in personality research, characteristics such as ‘conscientiousness’ are expected to be stable individual traits over time. That means that subsequent attempts to measure the trait should generally produce the same number. However, data collected from human participants is virtually never perfectly stable for a wide variety of reasons. Participants might have external or internal distractions while engaged with a measure, or might have state-level effects (e.g., tiredness or hunger) that unexpectedly influence the score obtained. Everything that influences our measure that us unrelated to the construct creates measurement error, which shows up in our experimental data as a contribution to the observed variance in performance. We will discuss methodological techniques for managing measurement error as best we can in Chapters 3 and 4, but even with best practices, there will always be some component of “noise” in our data (also important for our statistical approach, Chapter 5).

Another key aspect of an effective measured operational definition is its validity in capturing the underlying construct. Robust techniques for establishing validity of a novel measure are complex (Chapter 17) but a simpler key version of the issue is seen as the face validity of a measure. Face validity is one that can often be evaluated intuitively and is simply a question of whether the measure actually relates to the underlying construct. If we were to claim that our Experiment 1 recognition memory measure is a measure of how likely you are to forget why you walked into the kitchen, we would lack face validity and this level of inference about our data should not be trusted. In contrast, if we claimed that our measure was relevant for understanding how students could build better memory for studying material in the classroom, we would have better face validity (but not perfect and examples of where there might be a disconnect is left as an exercise for the reader).

Intelligence

To use a fairly controversial example, the IQ scale is an operational definition of the concept of *intelligence*, but there is no broad consensus of what exactly *intelligence* is. The IQ scale clearly measures something that has robust correlations with measures like academic success. However, whether there is a single underlying construct that is *intellignce* continues to be hotly debated. One alternative idea is that there are multiple *types of intelligence* that might be best measured separately. Obtaining data that argues for single or multiple types of intelligence turns out to be extremely challenging

Key Takeaways and Exercises

- Measurement is the assignment of scores to individuals so that the scores represent some characteristic of the individuals. Psychological measurement can be achieved in a wide variety of ways, including self-report, behavioral, and physiological measures.
- Psychological constructs such as intelligence, self-esteem, and depression are variables that are not directly observable because they represent behavioral tendencies or complex patterns of behavior and internal processes. An important goal of scientific research is to conceptually define psychological constructs in ways that accurately describe them.
- For any conceptual definition of a construct, there will be many different operational definitions or ways of measuring it. The use of multiple operational definitions, or converging operations, is a common strategy in psychological research.
- Variables can be measured at four different levels—nominal, ordinal, interval, and ratio—that communicate increasing amounts of quantitative information. The level of measurement affects the kinds of statistics you can use and conclusions you can draw from your data.
- Psychological researchers do not simply assume that their measures work. Instead, they conduct research to show that they work. If they cannot show that they work, they stop using them.
- There are two distinct criteria by which researchers evaluate their measures: reliability and validity. Reliability is consistency across time (test-retest reliability), across items (internal consistency), and across researchers (interrater reliability). Validity is the extent to which the scores actually represent the variable they are intended to.
- Good measurement begins with a clear conceptual definition of the construct to be measured. This is accomplished both by clear and detailed thinking and by a review of the research literature.

Exercises

As an exercise in thinking through the process of creating operational definitions, considering the following 3 common sayings. For each, provide an example of how you might operationally define (a) an independent variable, (b) a dependent variable, and (c) state the direction in which the IV is hypothesized to affect the DV.

- People feel sadder in blue rooms than in pink rooms
- It takes longer to recognize a person in a photograph seen upside down
- Absence makes the heart grow fonder

Additional optional questions

- Practice: Complete the Rosenberg Self-Esteem Scale and compute your overall score.
- Practice: Think of three operational definitions for sexual jealousy, decisiveness, and social anxiety. Consider the possibility of self-report, behavioral, and physiological measures. Be as precise as you can.
- Practice: For each of the following variables, decide which level of measurement is being used.
    - A university instructor measures the time it takes her students to finish an exam by looking through the stack of exams at the end. She assigns the one on the bottom a score of 1, the one on top of that a 2, and so on.
    - A researcher accesses her participants' medical records and counts the number of times they have seen a doctor in the past year.
    - Participants in a research study are asked whether they are right-handed or left-handed.
- Discussion: Think back to the last college exam you took and think of the exam as a psychological measure. What construct do you think it was intended to measure? Comment on its face and content validity. What data could you collect to assess its reliability and criterion validity?

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3 Experimental Control

We have described the process of setting up an experimental design as starting with the high-level constructs and then implementing operational definitions that allow us to create an experimental procedure that assesses the effect of an independent variable on a dependent variable. The previous chapter discussed some aspects of creating a measured operational definition that can be used as the dependent variable. In Chapters 3 and 4, we will review issues and methods for creating effective independent variables that will allow us to draw strong conclusions from our research studies.

As an example, consider starting with the hypothesis that "listening to music while studying is helpful." To evaluate this idea as a research question, we would need to choose operational definitions for music and also define what we mean by "helpful." We could potentially define what we mean by helpful as a measured operational definition of some academic performance, e.g., grade on an upcoming exam. That would give us our dependent variable. For the independent variable, a surprisingly common design error is to come up with a way of implementing a way of listening to music during studying – and then stop there. As experimental psychologists, we should get in the habit of asking a follow-up question after stating the hypothesis: "compared to what?"

Our hypothesis implicitly implies that music should help with studying more than some condition that does not involve music. In this example, the choice

In this chapter, we will start with consideration of designs based on treatment/control and then extend this idea to using an IV with two levels. Most of the designs we consider in this class are based on two levels. While the extension to three or more levels is fairly simple conceptually, this change can dramatically increase the complexity of the data analysis tools required to draw inferences about statistical reliability. Since our focus is experimental design and conclusions obtained from basic psychological science processes, we will generally rely on simpler statistical approaches to illustrate methods of testing hypotheses about psychology.

Learning Objectives

1. Understanding the independent variable in experimental design
2. Define what a control condition is, explain its purpose in research on treatment effectiveness, and describe some alternative types of control conditions.
3. How to construct two treatment conditions as the IV for a study and how this might be extended to more complex designs
4. Recognize examples of confounding variables and explain how they affect the internal validity of a study.
5. Bias and demand characteristics as potential confounding variables and the importance of random assignment
6. Situations requiring the use of single-blind and double-blind methodologies

Treatment and Control Conditions

In psychological research, a treatment is any intervention meant to change people's behavior for the better. Interventions includes psychotherapies and medical treatments for psychological disorders but also interventions designed to improve learning, promote conservation, reduce prejudice, and so on. We will discuss methodologies specific to intervention-based research in detail in Chapter 14. Here, we will start with the simplest kind of intervention research as an example of a very basic design. To determine whether a treatment works, participants would be randomly assigned to either a treatment condition, in which they receive the treatment, or a control condition, in which they do not receive the treatment. After the intervention, the dependent variable is assessed and if participants in the treatment condition score better, we can infer that the treatment led to the change in condition. In the earlier example, we could compare exam scores after studying with music to studying in silence with music acting as an intervention and silence as the control.

Choosing an effective control condition is not always a straightforward process. One challenge is that participants might be aware that they are in the treatment condition, which creates an expectation on their part about the research study. Participants who are aware they are receiving a treatment that is hypothesized to improve their performance might exhibit an influence of demand characteristics, where their scores on the dependent variable incorporate this expectation. Because of this, while minimally engaging control conditions, sometimes termed a no-treatment control condition where participants receive no treatment whatsoever, can create an interpretation problem due to the existence of placebo effects. A placebo is a simulated treatment that lacks any active ingredient or element that should make it effective, and a placebo effect is a positive effect of such a treatment. Many folk remedies that seem to work—such as eating chicken soup for a cold or placing soap under the bed sheets to stop nighttime leg cramps—are probably nothing more than placebos. Although placebo effects are not well understood, they are probably driven primarily by people's expectations that they will improve. Having the expectation to improve can result in reduced stress, anxiety, and depression, which can alter perceptions and even improve immune system functioning (Price, Finniss, & Benedetti, 2008). Placebo effects are interesting in their own right, but they also pose a serious problem for researchers who want to determine whether a treatment works.

The Powerful Placebo

Many people are not surprised that placebos can have a positive effect on disorders that seem fundamentally psychological, including depression, anxiety, and insomnia. However, placebos can also have a positive effect on disorders that most people think of as fundamentally physiological. These include asthma, ulcers, and warts (Shapiro & Shapiro, 1999). There is even evidence that placebo surgery—also called "sham surgery"—can be as effective as actual surgery.

Medical researcher J. Bruce Moseley and his colleagues conducted a study on the effectiveness of two arthroscopic surgery procedures for osteoarthritis of the knee (Moseley et al., 2002). The control participants in this study were prepped for surgery, received a tranquilizer, and even received three small incisions in their knees. But they did not receive the actual arthroscopic surgical procedure. Note that the IRB would have carefully considered the use of deception in this case and judged that the benefits of using it outweighed the risks and that there was no other way to answer the research question (about the effectiveness of a placebo procedure) without it. The surprising result was that all participants improved in terms of both knee pain and function, and the sham surgery group improved just as much as the treatment groups. According to the researchers, "This study provides strong evidence that arthroscopic lavage with or without débridement [the surgical procedures used] is not better than and appears to be equivalent to a placebo procedure in improving knee pain and self-reported function" (p. 85).

Independent Variable with Two Levels

The challenges of interpreting a placebo condition can be avoided by using an independent variable with two levels that vary across the underlying construct. In our Experiment 1, the two conditions were operational definitions of varying levels of depth of processing. Viewing the independent variable this way highlights how the operational definition of this part of the design is similar to the measured operational definition for the dependent variable. However, the independent variable needs to be conceptualized in a way that allows for implementation of two conditions that vary in the amount that the underlying construct is brought to bear.

Independent variables that can be manipulated to create two conditions and experiments involving a single independent variable with two conditions are often referred to as a single factor two-level design. This approach is also referred to as a two groups independent samples design, which will connect to a common statistical approach for this design (two independent samples t-test; see Chapter 5).

This approach is straightforward conceptually, but not always simple to put into practice. In our music and studying example, it is not necessarily immediately clear how we might construct two levels of "music" to compare. This uncertainty is often very helpful in experimental design to focus the experimenter's attention on the underlying question of "why" one might hypothesize that music helps with studying. The question of the mechanism by which the independent variable is hypothesized to affect the dependent variable, the "why?" question is often the most difficult and interesting part of psychological science. No hypothesized mechanism was given in the above example, but we might conjecture that music helps with studying by inducing a state of calm while blocking out ambient noise. In that case we could compare music to an audio recording of ambient noise that participants found not be calming. In Chapter 4, we will discuss additional considerations needed to implement novel comparison condition to maintain the validity of the research study.

Extending this design approach

We will discuss a series of examples with two-group designs as these are the simplest designs that still illustrate the critical issues of extraneous variables and potential experimental confounds. However, sometimes greater insights can be gained by adding more conditions to an experiment. When an experiment has one independent variable that is manipulated to produce more than two conditions it is referred to as a single factor multi level design. So rather than comparing a condition in which there was one witness to a condition in which there were five witnesses (which would represent a single-factor two-level design), Darley and Latané's experiment (Chapter 1) used a single factor multi-level design, by manipulating the independent variable to produce three conditions (a one witness, a two witnesses, and a five witnesses condition).

In addition, we will discuss creating designs with two conditions that all participants get to experience, which are termed within-participant designs (Chapter 7). This approach can be very effective as long as the unique challenges of condition order can be managed. In addition, we will extend our consideration of experimental design two cases with two independent variables, typically called factors and factorial design (Chapter 9). Even the simplest factorial designs add a lot of complexity to the experimental design, procedure and data analysis. They allow for a much broader and more interesting range of hypotheses to be tested. Most modern published psychological research uses multi-level factorial design. Although we will keep to simpler designs for examples here, the extension from a two-factor design to arbitrarily complex designs is conceptually straightforward.

Internal Validity

The term internal validity is used to characterize an experimental design that will be able to test the underlying hypothesis. Any major problem that impairs the ability to draw a conclusion from the experimental data is a problem with the internal validity of the study. One way this can happen is if there is a mistake in the operational definitions. If they do not accurately reflect the underlying construct, the main inference about the constructs cannot be drawn from the data.

This core idea is distinct from external validity, which reflects the degree to which the conclusions can be applied to participants outside the research lab, e.g., in the real world. External validity generally depends on the methods of sampling participants, that is, how they are found and recruited into the study. This issue will be discussed in depth in Chapter 13, but as a preview, you can consider the concern being raised about the general dependence of psychological research on behavior measured from undergraduate students at major American universities. The question is whether the results obtained from university participants correctly predict the behavior of the broader population and whether we need to consider broader sampling or limiting the expected breadth of our conclusions.

The question of internal validity is not whether the results apply outside the population engaged in the study, but whether the experimental design itself is robust enough to draw any conclusions at all.

Confounding Variables

The most important consideration to consider first when evaluating whether an experiment has a high degree of internal validity is whether there is a confounding variable embedded in the design. In a two-group design, an experimental confound is an extraneous variable that varies with the planned independent variable that leads to different conclusions about the results of the study.

Extraneous variables

Extraneous variables are any variables that affect scores on the dependent variable that are not part of the experimenter’s design. There are always a large number of these implicit in any experiment. For example, in the Experiment 1 memory study, the words themselves affect how well they will be remembered later. Uncommon words and longer words are more memorable than short, frequently encountered words. Details of the experimental context such as what time of day, where, when and with what external distractions will affect performance. Individual differences in memory, or familiarity with the words will affect scores on the recognition test. None of these variables are confounds for this experiment. Instead, these reflect factors that affect the recognition score dependent variable that will mostly show up as variance in the observed data.

Most of the time, extraneous variables do not affect the internal validity of a study. That is, they create noise in measures that can lead to a failure to reject the null hypothesis statistically. They create Type 2 errors, but this is the less consequential problem of the two main errors in experimentation.

For an extraneous variable to be a confound, it has to vary with the independent variable. As an example, while some words are more easily remembered than others, for this to be a confound problem for our Experiment 1, participants would have to see different words in the two conditions (rating liking or counting vowels). If the participants in the rating liking condition also saw words that were more memorable, e.g., were longer, we would have an inference problem with our data and be at risk for a Type 1 error, the type we strongly try to avoid. The inference problem should be clear: if the participants in the liking condition also had longer words, we would be able to say (a) rating liking was associated with better recognition score and (b) longer words were associated with better scores. We would have no way to tell which effect actually produced our data and this is the general problem when a confound has been detected. The real problem is that when there is an experimental confound, we learn nothing from the study. We cannot say later that the IV affected the DV, nor do we have any evidence that the IV did not affect the DV.

A properly designed study is planned around anticipated extraneous variables and carefully designed to avoid confounds. Basic approaches for this are the subject of Chapter 4. Confounds are more likely to occur in non-experimental designs where the independent variable is not under the experimenter's control.

In experimental design, the risk of a confound often comes from practical questions associated with implementing the procedure in ways that are unexpected in planning the experimental design. As an example, consider the hypothetical scenario where after planning Experiment 1, we discovered that there was a mistake in the online protocol so that all the participants in the class received the shallow encoding instructions (count vowels). But we found another group of students unable to attend the first class who could participate in the deep encoding condition, but these were non-native English speakers who had been unable to travel to attend the first class. In this scenario, we would have accidentally created a confounded study where all the non-native English speakers were in the same condition and if their memory for English words was different, we would not be able to draw any conclusions from our data.

When planning a research study, or readying about a completed study, the standard method to try to identify potential confounds is to try to think of as many extraneous variables as possible that might affect the DV. There will generally be quite a few, but most or all of these will not vary with the IV so we do not have to worry about them reducing the internal validity of the study by creating a confound.

One very common aspect to consider as a potential extraneous variable is individual differences in performance. On a memory test, maybe some of the participants are just better at memorizing lists of words than others. For almost any study, individual differences will almost always be an extraneous variable, but very rarely will this be a confound for the results. The reason this is rarely a confound is the common use of a simple, but important procedure called random assignment to conditions.

Random Assignment to Conditions

As long as participants are randomly assigned to conditions, individual differences should never confound the final result. It is tempting to worry that it is possible to get unlucky in our randomization and assign all the better/worse participants to the same condition. However, this is exactly what our statistical tools are designed to test. For all our statistical tools for deriving inferential statistics, the final "p value" is formally the probability that we accidentally observed the difference we did due to this random chance (under the null hypothesis that there was no effect of the IV). When we reject the null, we explicitly consider and reject the possibility that individual differences, or any other non-confounding extraneous variable accounted for our results.

It is important to note that for random assignment to work, it has to be carried out correctly and there needs to be an adequately large sample of participants recruited for the study. We will discuss sample size in the context of statistics (Chapter 5), sampling (Chapter 13) and designing research (Chapter 15). A good, simple rule-of-thumb is to try to have at least 30 participants in each of your experimental conditions, if possible. It isn't always possible to obtain that many volunteers, however, and 15-20 per condition also often works.

Smaller sample sizes weaken the effectiveness of random assignment. In some specialized cases with restricted populations (e.g., neuropsychological studies) it is not possible to recruit large samples. In these cases, it may be necessary to use designed based on matched participants, where participant-based extraneous variables are assessed and explicitly balanced across the IV. This and related techniques were used in some older psychological science studies that pre-date the modern recommendations to use larger sample sizes. The challenge of matching procedures is the need to identify all possible participant-based extraneous variables and then have reliable, independent measures of all of these prior to assigning conditions. It is generally much simpler just to randomly assign a large group of participants to conditions and trust that the statistical model will account for assignment luck.

Demand Characteristics and Bias

Random assignment, properly carried out, will prevent individual differences from confounding an experiment. However, incorrectly following the randomization procedure can lead to embedding bias in a study. Bias in a study creates a problem similar to a confound but which is generally smaller in effect, but often much harder to detect. An example of where sampling bias can creep into a research study is when a novel experimental procedure is being developed with a complex IV and to test the procedure, the experimenter runs the first group of volunteers all in the treatment condition. This might be done ostensibly to test the procedure to make sure it is working as intended. However, this can create an accidental bias in that the first participants to sign up for your experiment are often the most engaged and motivated participants who really want to do well on your DV measure. Their data is now disproportionately in the treatment condition. Later when you compare treatment to control, there has accidentally been a bias included where treatment is correlated with motivation, weakening the internal validity of the study in the same manner as a confounded variable.

Because of the risk of this somewhat subtle kind of bias, we strongly prefer procedures that cannot be influenced by experimenters' expectations or desires. Virtually all researchers want their experiments to succeed, so avoid the possibility of implicitly embedding bias by removing the experimenter's opinion when assigning participants to conditions. Standard experimental methods will have meticulously detailed protocol instructions to be followed to the letter to avoid weakening the conclusions. Cases where this was not done will be discussed in the context of Research Ethics (Chapter 8) and specifically the Responsible Conduct of Research (Chapter 19).

The expectations of the experimenter are not the only source of concern for implicit bias in carrying out our research procedures. Participants in research are often very sensitive to the demand characteristics of the protocol. These effects are similar in spirit to placebo effects in that these expectations affect performance on the DV. In general, if the participants in a research study are aware of the underlying hypothesis, this may influence their performance on the task that measures the dependent variable. This will confound the study and make the conclusions inaccurate.

As an example, suppose participants in our Experiment 1 knew about both conditions being studied and that we expected that rating liking would produce better memory than counting vowels. They might then try harder to remember the words if they were asked to rate liking and score better on the recognition test based on their motivation. This would have the effect of creating a confound between motivation and depth, potentially leading to a Type 1 error in conclusions.

The simplest way to avoid this problem in a two-group independent samples design is to not inform the participants about the hypothesis or the other condition of the study that they are not participating in. This is termed a single-blind procedure and was the way we implemented our Experiment 1 here. This is an extremely common method for designing psychological research that strengthens the internal validity of the experiment by eliminating concerns about demand characteristics. Later in Chapter 8, we will touch on the subtle ethical implications of this common approach (we prefer participants to know what they are engaging in when participating in research, yet we usually cannot explain everything in advance).

More complex procedures need to be used in experimental design when there is concern about the possibility of experimenter bias affecting the measurement of the dependent variable. This effect needs to be considered whenever there is a subjective element to the quantification of the DV. While many measures and scales are scored objectively (e.g., our recognition memory measure or the Self Esteem scale from Chapter 2), there are many areas of psychological study that are not as externally objective. For example, to evaluate a treatment aimed at reducing stage fright and improving stage performance, it would be necessary to quantitatively evaluate performance. Or we might need to measure an aspect of emotional expression such as laughter or quality of partner interactions in a study of relationships. For any subjective judgment, we assume that experimenters who are aware of the design and are invested in the outcome of the study are at risk for experimenter bias and should not be the source of the DV measure.

One common method for when DV requires a subjective evaluation is to use independent raters who provide the scores of the judgment without knowing the condition the participant was in. These raters are blind to the experimental condition, so that their rating cannot be influenced by the experimental hypothesis. The raters must generally be trained with detailed instructions on how the scoring of the DV is to be carried out. It is also common to have multiple raters and compare scores for overall consistency to establish the reliability of the procedure.

In cases where independent raters cannot be used, a double-blind methodology may be employed to remove experimenter bias. This approach is most commonly seen in medical research, such as pharmaceutical intervention designs aimed to test whether a new drug is effective at treating a disease. In medical research such as this, it is difficult to implement an external scoring system for a complex DV like improved health outcomes because the participants are patients under care of a physician who is often also the experimenter.

The double-blind procedure involves administering the IV in a way such that the researcher does not know which participants are in each condition. In a drug study, this is done by a pharmacy providing numerically labeled doses that are half treatment drug and half placebo. The research staff administers the drug without knowing whether the participant is receiving treatment or control so that all subsequent health measure assessments are done blind to experimental condition. At a specific later planned date, the conditions are revealed so that data analysis can be done to identify the efficacy of the drug.

A double-blind procedure is an extremely rigorous and robust procedure for assessing efficacy of interventions. It is, however, difficult to implement properly, which makes research depending on this approach slower and more expensive to carry out. Because of this, the approach is not in common use in psychological science. In most psychological research, we can identify bias-free measures for our dependent variables or implement independent-rater procedures which are much easier and simpler to deploy in practice.

Key Takeaways

- An experiment is a type of empirical study that features the manipulation of an independent variable, the measurement of a dependent variable, and control of extraneous variables. Control condition.
- An **extraneous variable** is any variable other than the independent and dependent variables. A **confound** is an extraneous variable that varies systematically with the **independent variable**.
- Constancy
- Counter-balancing
- **Random assignment** to conditions in between-participants experiments is a fundamental element of experimental research. The purpose of this technique is to control extraneous variables so that they do not become confounding variables.
- Studies are high in **internal validity** to the extent that the way they are conducted supports the conclusion that the independent variable caused any observed differences in the dependent variable. Experiments are generally high in internal validity because of the manipulation of the independent variable and control of extraneous variables.

Exercises

Question 1: Laughter is the best medicine

Imagine you have just read an article in the newspaper describing a scientific study in which researchers found that people who laugh a lot tend to have lower blood pressure, stronger immune systems, feel less stressed out.

Considering the problem of extraneous variables and potential confounds, give an alternate hypothesis for how this relationship might be observed without supporting the authors' conclusion. Note that this requires a statement consistent with the data, not consistent with the conclusion.

Outline an experimental approach to this question that would more directly test the hypothesis. Provide an example of an operational definition of the IV, the DV and what you would expect to find if laughter positively affects health.

Question 2: Briefly answer the following questions about experimental control from our Experiment 1:

- Why have both groups read the same words?
- Why have 1-5 scales for responding for both conditions?
- Why require the word to be on screen for minimum 3 s?
- Does it matter if the trivia questions use words from the study list?

4 Experimental Control

The fairly well-known phenomenon of “stereotype threat” reflects a situation where exposure to some commonly-held cultural expectations about poor performance actually causes poorer performance. Methodologies for studying stereotype threat are often excellent design examples, but I have sometimes hesitated to rely on them in classroom situations because of the effects they cause. If simply repeating the expectation that some subgroup is, for example, bad at math causes poorer math performance, then even discussing the research imposes a cost on students in the class. As a general rule, stereotypes are examples of a general misunderstanding of correlation and causation as they describe current relationships that might exist in the world that lead to misleading conclusions about why (e.g., the existing relationship is thought to be intrinsic instead of reflecting cultural bias).

In spite of being a sensitive topic, research on stereotype threat provides an excellent example of unexpected extraneous variables influencing conclusions drawn from research. Stereotype threat research can be carried out with a simple design with two levels of an independent variable. In one condition, participants are exposed to the stereotype threat content and in the other condition, participants are exposed to control content that does not mention the stereotype. The dependent variable is measured performance on a related test. If performance is reliably lower after being exposed to the stereotype, then we see that the IV affected the DV and can draw the inference that stereotype threat affects performance.

However, not every study of stereotype threat produces a reliable effect of the IV on the DV, leading to questions about the robustness of the phenomena. In Aronson et al. (1999) some insight into this variability was provided in a study that examined stereotype threat on math performance but further asked participants how important math was to them. For students who self-reported that math was extremely important to their identity, stereotype threat was found to impair performance. Their study was also notable in that they used an unusual threat stimulus where white males were exposed to the stereotype that "Asians perform better on math tests," showing that this effect also applies to non-minority participants. But for students who reported that math was not important to them, no effect of stereotype threat was found. In fact, the stereotype threat led to better performance in the low math identification group.

This study both advances the understanding of the phenomenon but also illustrates some of the challenges of extraneous variables. How important math is to the participants is a variable that might have been overlooked in previous studies. If there were many participants in a research study who did not care about math, that study would likely not have observed a reliable effect of the threat on their performance. This uncontrolled extraneous variable would lead to increased variance on the math performance measure (some participants showing the effect and some not), leading to statistical results that are not reliable. Note that the absence

Text Box

Text box text

of a statistical effect here does not allow us to conclude that stereotype threat does not exist. This is an example of an important idea to be discussed in Chapter 5 that we cannot draw inferences from non-reliable results.

This example does illustrate the most difficult aspect of coping with extraneous variables in experimental design in that they are often not known in advance of the research. It can take a lot of experience and expertise in the specific research domain to learn where design problems might emerge from. And in cases like this, the new variable ends up significantly extending the known theory about the main constructs for the study.

Learning Objectives

1. Managing the effect of extraneous variables on experimental measures
2. Constancy: keep as many factors as possible consistent across levels of the independent variable
3. Counterbalancing: if factors cannot be kept constant, distribute them evenly across the independent variable to avoid confounds
4. Practical concerns: defining the procedure, instructions to participant, pilot testing and manipulation checks

Non-confounding Extraneous Variables

In Chapter 3, the problem of experimental confounds was used to illustrate the importance of planned experimental control. Once all potential confounds are eliminated from the design of a study, the next challenge is to manage the extraneous variables to reduce variance on our dependent measure. Any measure derived from human participants is going to have variance in performance associated with it. We will see the term variance used to evaluate the DV statistically. Conceptually, this variance results in part from measurement error, which reflects the important idea that no quantitative operational definition is ever perfect. We can also think of this variance as “noise” in that it reflects aspects of our data that we are not directly concerned with as part of our experimental hypothesis.

As we have seen previously in Chapter 3, an extraneous variable is anything that varies in the context of a study other than the independent and dependent variables. In an experiment on the effect of expressive writing on health, for example, extraneous variables would include participant variables (individual differences) such as their writing ability, their diet, and their gender. They would also include situational or task variables such as the time of day when participants write, whether they write by hand or on a computer, and the weather. Extraneous variables pose a problem because many of them are likely to have some effect on the dependent variable. For example, participants’ health will be affected by many things other than whether or not they engage in expressive writing. This influencing factor can make it difficult to separate the effect of the independent variable from the effects of the extraneous variables, which is why it is important to control extraneous variables by holding them constant.

Extraneous variables make it difficult to detect the effect of the independent variable by adding variability or “noise” to the data. Imagine a simple experiment on the effect of mood (happy vs. sad) on the number of happy childhood events people are able to recall. Participants are put into a negative or positive mood (by showing them a happy or sad video clip) and then asked to recall as many happy childhood events as they can. Even in the happy mood condition, some participants would recall fewer happy memories because they have fewer to draw on, use less effective recall strategies, or are less motivated. And even in the sad mood condition, some participants would recall more happy childhood memories because they have more happy memories to draw on, they use more effective recall strategies, or they are more motivated. If the effect of these extraneous variables was large, then the added variance in performance can make even a real effect of the manipulation difficult to detect (a Type 2 error).

Control of Extraneous Variables

The principles for implementing best practices for reducing the effect of extraneous variables are simple in theory. Once the variables have been identified, keep as many as possible constant across conditions. Anything that cannot be kept constant but can be controlled, counterbalance across conditions so that it occurs equally often across levels of the independent variable. These two basic techniques remove the possibility of extraneous variables being confounds and maintain the internal validity of the study.

Practically speaking, implementing these aspects of experimental control can be difficult. As seen in the example with stereotype threat, it can be difficult to identify all possible extraneous variables in advance of running a study. In addition, differences in performance arising from individual differences in the participants cannot be externally controlled and can only be handled by random assignment. However, there are generally a set of variables related to the stimuli used in the experiment and testing conditions that can be managed in order to both avoid confounds and minimize noise in the DV.

Constancy

As much as possible in any experimental design, keep things constant across the levels of the independent variable. Use a meticulously written procedure for carrying out the procedure and follow the guidelines to the letter. Keep the stimuli the same across conditions as much as possible. Avoid changing anything about the recruiting process, task instructions or context in which data collection is carried out.

It can be surprising to students who get to participant in psychological science research how explicitly detailed data collection procedures typically are. Many studies have carefully- written scripts for interacting with participants in research. This is done to keep interactions as constant as possible across conditions and also across experimenters. The importance of this level of experimental control is also seen when research is reported through the Methods section of an APA-formatted research report (Chapter 6). Many of those specific script details are included with the presentation of the experimental results so that the reader can identify key aspects of experimental control in carrying out the study.

Counterbalancing

For any factors that cannot be kept constant, distribute how these are implemented equally across conditions. For example, if participants are being run throughout the day, collect data from both of the experimental conditions equally early and late in the day to avoid confounds due to circadian (time of day) effects. If it is necessary to have multiple experimenters, make sure they each contribute to data collection in each condition. If the stimuli are presented in different orders to participants, make sure the orders are distributed properly across the conditions of the study.

Note that counterbalancing is focused on making sure the extraneous variables do not confound the study but does not address the issue that these variables may contribute to measurement noise. That means that these factors that affect the DV importantly may increase the variance of that measure and creates the risk of a Type 2 error (where we fail to obtain reliable results even though the hypothesis was correct). We prefer to take risk of a Type 2 error over the risk of a Type 1 error where we incorrectly claim the IV affected the DV but our inference is incorrect due to a confound embedded in the design.

In some cases, we may not be able to fully control variables like the time of day the participants complete the study (e.g., online) and therefore cannot formally counterbalance to guarantee the same number of participants complete the study in the morning or evening. In those cases, we might rely on an approach more similar in spirit to random assignment to keep these variables from being confounds for the study.

Constancy by Restricting Recruiting?

Keeping extraneous variables constant can also be applied to holding participant variables constant. For example, many studies of language limit participants to right-handed people, who generally have their language areas isolated in their left cerebral hemispheres. Left-handed people are more likely to have their language areas isolated in their right cerebral hemispheres or distributed across both hemispheres, which can change the way they process language and thereby add noise to the data.

In principle, researchers can control extraneous variables by limiting participants to one very specific category of person, such as 20-year-old, heterosexual, female, right-handed psychology majors. The obvious downside to this approach is that it would lower the external validity of the study—in particular, the extent to which the results can be generalized beyond the people actually studied. For example, it might be unclear whether results obtained with a sample of younger lesbian women would apply to older gay men. In many situations, the advantages of a diverse sample (increased external validity) outweigh the reduction in noise achieved by a homogeneous one.

Historically, a great deal of early health-based research was done with insufficient attention to maintaining appropriate diversity in participant recruiting (e.g., all participants were white males). The attempt to justify this at the time was that this reduced variability in participants, increasing the power to detect whether a health improving intervention was clinically effective. Technically this approach increased internal validity of the design while reducing the external validity of the conclusions (to be further discussed in Chapter 13). However, it should be clear that this also raises significant ethical concerns that these research studies were not being designed to provide benefit widely across the population. The tension between improving the scientific process with homogeneous recruiting samples and the ethical goal of benefiting all people will be one of our examples of “Where Ethics Gets Interesting” (Chapter 8/19).

Modern approaches to psychological science have reinforced the idea that we should avoid restricted sampling as much as possible in research. Concerns have been raised about the reliance on “WEIRD” participant samples: Western, educated, industrialized, rich and democratic. These are kinds of participant groups that are included in research that depends on university undergraduates in the USA, Canada, and Western European countries. Commonalities in social or cultural expectations in these participants may be implicitly embedded in many psychological research reports. The main implication of this idea is that there may be unknown extraneous variables that vary across social and cultural groups that affect behavior in ways we have yet to explore in research. That does not invalidate research that depends on WEIRD populations, but may affect applications of the findings to broader, more diverse populations.

One technique for increasing potential diversity of research is to use methodologies for collecting data online. Research on how online methodologies affect recruiting diversity is ongoing. Collecting data online likely improves diversity compared to WEIRD samples, but may still restricts sampling to relatively higher socio-economic status due to the need to have technological access depending on a device and internet connectivity.

Design of Experiment 1

Our Experiment 1 reflects a handful of design decisions aimed to keep extraneous variables constant across the two conditions in the study: deep and shallow encoding. All participants rated the exact same set of 30 words, although the instructions for the rating varied as the independent variable. The words themselves were selected to be between 5 and 8 letters in length and to have a “written frequency” occurrence of 30-80 times per million. The characteristics of the words were kept similar to reduce variance in memory for the words chosen for the experiment.

Unless you have some experience in memory research using word lists, you might not have anticipated that the length or frequency of the stimulus would be important for the design. Knowing what potential extraneous variables are relevant to a specific study often requires some prior knowledge of research in that domain. Once the variables are identified, the technique for controlling them is straightforward: select words in a restricted range from a database of word frequency information.

In addition to the stimuli, note that the two scales used for rating the stimuli were also constructed to have 5 levels. Although it is unlikely that the specific number of levels on the scale will affect memory, it is good practice to keep as many design elements the same as possible across conditions.

In cases where the data collection for Experiment 1 are done in the classroom, we also gain the benefit of all the participants complete the study in the same conditions in terms of surrounding and time of day. When this experiment is completed by participants outside the classroom, there may be influences of outside distractions and attention that are outside of experimental control. Note that these would be examples of extraneous variables that increase variance, but do not confound the study because we have no reason to believe that either of the conditions of the independent variable would be more affected by distraction.

The design of Experiment 1 also includes 3 minutes of irrelevant trivia questions to be completed after performing the word rating and before the surprise recognition test. The time of the trivia task is kept constant across participants, but the number of questions answered and the content of the questions is not. The number and content of the questions experienced is allowed to vary randomly across all the participants in the study, potentially contributing to variance in the memory measure but not in a way that is confounded with the study conditions.

Practical considerations

Best practices for controlling extraneous variable in carrying out psychological research can lead to fairly elaborate and precise procedures for research personnel. As a consequence of this, it is very common for research procedures to be evaluated with a short period of pilot testing before staring formal data collection. Sometimes this can mean simply practicing carrying out the research procedures under observation of other researchers to ensure it is working as intended by the planned operational definitions. It can also mean running a small preliminary sample of participants to evaluate the procedure and scripts. It should be very clear in the overall research plan when pilot testing is underway and when that process is complete and formal data collection for the planned study starts. Pilot testing data is not intended for inclusion in published research and may often depend on knowledgeable members of the research team (or collaborating teams). This can affect demand characteristics of those participants making their behavior or performance importantly different from the main intended recruited sample.

A common feature of pilot testing of procedures is to include a measure referred to as a manipulation check. This is a measure that will often look like a dependent variable but is not part of the research hypothesis. For example, in a mood manipulation study using music to create positive/negative moods, participants might be asked after listening to the music to rate their mood. If mood ratings were not consistent with the independent variable (music type), we would have concern about the operational definition being used. In some research publications, manipulation check data may be included and even analyzed statistically but note that no real hypothesis is being tested. A statistically reliable effect that the music manipulation affected self-rated mood only validates the operational definition of the IV and does not lead to any general conclusion.

Pilot and preliminary testing can also be used to examine the distributional characteristics of the dependent variable. As we will see in the next chapter, our ability to draw inferences from our data will depend on observing statistically reliable effects of the IV on the DV. Poorly controlled extraneous variables may lead to high levels of variability in performance, which will show up as high variance and may indicate a need to improve experimental control in design. Accurate estimates of variance often require large participant samples, though, so this cannot always be anticipated.

Pilot testing is often very useful to identify potential statistical problems with floor effects or ceiling effects in the DV. Ceiling and floor effects occur when the dependent variable measurement range is not properly anticipated in the experimental design. For example, a floor effect will occur when a task is too difficult for participants. If participants are given a problem-solving task with the intention of the measure being the number of problems solved but nobody is able to solve any of the problems, everybody will score zero regardless of the IV manipulation (no reliable difference can be detected). Similarly, if all participants get all the answers correct, performance is at ceiling for all groups and again there is no possibility of observing a statistically reliable effect. Pilot testing is often used to verify that scores on the dependent variable will be within a range that allows for detectable influence from the independent variable so that we have some chance that our statistics will be effective.

Key Takeaways

- Extraneous variables that do not confound the study increase variance in performance (measurement error, noise).
- Constancy: as much as possible, keep things the same across levels of the independent variable
- Counterbalancing: for anything that cannot be kept constant, keep this factor from being confounded with the independent variable
- Restricted participant sampling may reduce variance (increasing constancy of participant variables) but should be used carefully due to effects on generalizability of findings
- Identifying all the possible extraneous variables is harder than controlling for ones that are known

- Rigorous systematic procedures for data collection are important and contribute to research success
- Pilot testing is the process of working out details for research procedures and often precedes formal data collection

Exercises