CHAPTER 14

Assessing Mechanisms

What You'll Learn

- Estimating an average causal effect doesn't tell us why or how that effect arises.
- Learning about the mechanisms underlying a causal relationship is harder than it seems. Generally, we can't just measure potential mechanisms and learn which ones are most important.
- Combining theory, measurement, and clear thinking can help us learn about the mechanisms underlying causal relationships.

Introduction

As we discussed in chapter 3, when we say that a treatment affects an outcome, all we mean is that a change in the treatment changes the outcome. But that effect need not be direct or proximate—the effect of some event on some outcome could be the result of a long chain of relationships. So, in many settings, even once we've credibly estimated a causal relationship, we might remain uncertain why or how the treatment affects the outcome.

For example, suppose we found that attending charter schools rather than regular public schools causes an increase in the likelihood that students go to college. That's an interesting, policy-relevant finding. It tells us that, on average, charter schools are helping students. But it doesn't tell us *how*—that is, it doesn't tell us the mechanisms by which charter schools help students. Maybe the curriculum is more innovative, maybe students benefit from having more motivated peers, maybe discipline is stricter, maybe the facilities are nicer, maybe there are more advanced placement (AP) classes, maybe the students are better prepared for standardized tests, maybe there are more opportunities for after-school enrichment, or maybe the school motivates the students to work harder. People typically think of mechanisms as answers to *how* questions ("How did this effect arise?") or *why* questions ("Why did this happen?").

Randomizing students into charter schools versus public schools, or employing some other compelling research design, allows us to assess the average effect of going to a charter school. But it won't unpack which mechanisms are doing the work. Sometimes, understanding the mechanisms is important. For instance, if we are going to try to build more charter schools, we would like to know which features of existing charter schools are the most important to replicate. Should we make sure that there are nicer facilities,
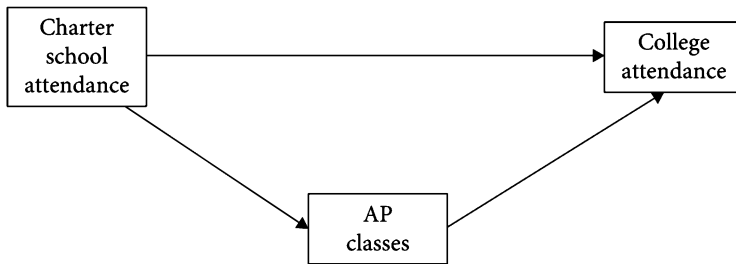
Figure 14.1. AP classes may be one of the mechanisms by which charter schools affect college attendance.

more AP classes, more standardized test prep, or stricter discipline? In this chapter, we consider some of the difficulties in trying to learn not just about effects but also about the mechanisms underlying those effects.

Causal Mediation Analysis

One approach that some researchers take to try to get at causal mechanisms is called *causal mediation analysis*. In causal mediation analysis, the goal is for the data to directly tell us how important a role some mechanism plays in driving a particular effect.

Figure 14.1, which recalls our illustration of mechanisms in figure 9.9, illustrates the idea. Suppose, for instance, that we want to know how much of the effect that attending a charter school has on college admission is due to charter school students taking more AP classes. In our earlier language, attending a charter school is the treatment and college admission is the outcome. We call taking AP classes a *mediator*—a mechanism by which charter schools have their effect on college admission. The idea is that attending charter schools affects taking AP classes and taking AP classes affects college admission. So some of the effect of attending a charter school on college admission runs through the AP class mechanism. We'd like to know how much of the effect is due to AP classes (represented by the arrows from charter schools to AP classes to college attendance) and how much is due to other factors (represented by the arrow directly from charter schools to college attendance).

If you *weren't* thinking clearly, you might be tempted to use the techniques from chapter 10 to try answer this question. You'd start by using an admissions lottery to estimate the effect of attending the charter school on college admission. You can do so by regressing college admission on winning the charter school admissions lottery for the set of students who were entered into the lottery. Then you'd measure the number of AP classes each student took and re-run the regression of college admission on winning the charter school lottery, but controlling for AP classes. The thought is that, if the estimated effect of charter schools shrinks once you control for AP classes, then that portion of the effect that disappeared is attributable to the AP class mechanism since, by controlling for AP classes, you are effectively holding them constant so that they are statistically removed from your estimate of the effect.

This idea might initially sound sensible. We are trying to figure out how much of the effect of charter schools on college admission runs through AP classes. We'd like to compare the effect of charter schools on college admission to the effect of charter schools on college admission *purged of the effect of taking AP classes*. So why won't it work?

The basic problem is that controlling for AP classes is not the same as purging their effect. You can see this just by thinking conceptually. To purge the effect of AP classes from the effect of attending a charter school on college admission, surely we must have a way of estimating the effect of taking AP classes on college admission. But we have described no research design to do so. Let's try to think that through as clearly as we can.

The charter school admissions lottery lets us estimate the effect of charter school attendance on college admission. It also lets us estimate the effect of charter school attendance on taking AP classes. But what lets us estimate the effect of taking AP classes on college admission? We are sure that you can think of lots of confounders for the relationship between how many AP classes a student takes and whether that student is admitted to college. So just regressing college admissions on AP classes surely won't do it.

To see why this is a problem, let's think about an extreme version of what could go wrong. Suppose that charter schools indeed cause students to take more AP classes, but that AP classes have no effect whatsoever on college admissions. (This is, of course, just for the sake of argument.) So, if our controlling strategy works to identify the importance of the mechanism, then we should find no difference between the effect of charter school attendance on college admission, whether or not we control for AP classes (since it is not in fact one of the mechanisms). But suppose taking AP classes happens to be correlated with academic talent (which we don't have a measure of) and that academic talent affects college admissions. Now, when we run the regression of college admissions on charter school attendance and AP classes, we will find that the estimated effect of going to a charter school is indeed lower when we control for AP classes. This is because AP classes are proxying for (i.e., measuring) academic talent. From this statistical result, we will wrongly conclude that allowing students to take AP classes is, therefore, an important mechanism by which charter schools cause college admissions. But, in fact, we've stipulated that AP classes have no effect. Taking AP classes just happens to be correlated with things like talent that are also correlated with getting into college. Our controlling strategy, therefore, was misleading. If we don't think clearly about this type of analysis, we could end up making bad decisions about how to allocate resources or design schools.

A literature delves into what kinds of conditions you would need to have causal mediation analysis work. Without going into the technical details, it boils down to something along the lines of having research designs that allow you to separately estimate the effect of the treatment on the outcome, the effect of the treatment on the mechanism, and the effect of the mechanism on the outcome. If you can estimate these quantities, you can net out the effect of the treatment on the outcome that runs through the mechanism. The key takeaway, of course, is that there is no magical technical or statistical way to identify which mechanisms matter. If we want to learn about mechanisms, just like when we wanted to learn about causal effects, we are going to have to work hard and think clearly.

Intermediate Outcomes

One thing an analyst can do is test for the effect of their treatment on other, perhaps more intermediate outcomes that might provide some hints about mechanisms. Once you have a research design that allows you to estimate the effect of some treatment, it can, in principle, be applied to any downstream outcome that you can measure.

So, to partially assess mechanisms, we can see which intermediate outcomes appear to be affected by the treatment. Returning to our example, we can use the charter school lottery to assess the effect of going to a charter school not just on college admission but also on intermediate outcomes like study habits, extracurricular participation, standardized test scores, taking AP classes, and so on. Of course, this doesn't give us a way to assess the effect of those intermediate outcomes on the final outcome (here, college admissions). For that, we'd need a separate research design. So, for reasons just discussed in the last section, this approach won't tell us exactly how much of an effect is explained by one mechanism or another. But it can allow us to make some progress in thinking about which mechanisms likely do or do not help us understand the effect. For instance, if it turns out that attending a charter school has no effect on taking AP classes, it seems unlikely that AP classes are one of the mechanisms by which charter schools affect college admissions.

One real-world example of using intermediate outcomes comes from research in Liberia by Chris Blattman, Julian Jamison, and Margaret Sheridan.

Cognitive Behavioral Therapy and At-Risk Youths in Liberia

Blattman, Jamison, and Sheridan randomly assigned some Liberian young men who were thought to be at risk for engaging in crime or violence to cognitive behavioral therapy, with the hope of improving economic outcomes and reducing crime and violence. The therapy appears to have worked well, significantly improving both outcomes. That is good news for the therapy program that Blattman, Jamison, and Sheridan were studying. But in addition to knowing that the therapy worked, it would be nice to know *how* or *why* it worked. The treatment lasted as long as eight weeks and included work on a variety of skills, from self-control to personal appearance. Some of the interventions even included monetary compensation. So there are a lot of different features of the treatment that could explain the result. And if you were going to try to transport this evidence elsewhere, you'd want to know which features of this program are important and which ones are incidental.

As we've discussed, there's no way to know with certainty which features of the intervention led to the overall effect. But the authors did measure a number of intermediate outcomes that might help to elucidate the mechanisms and aid practitioners who would like to apply the lessons elsewhere. Interestingly, the authors found little evidence that the therapy affected subjects' self-control skills, such as impulsiveness, perseverance, and conscientiousness. Therefore, they conclude that these self-control skills are unlikely to be an important mechanism through which the therapy reduced violence or improved economic outcomes. By contrast, they did find that their intervention had a large effect on other intermediate outcomes, such as social networks and attitudes toward violence. This suggests that these mechanisms are more likely to explain the success of the treatment and might be more important to replicate in other settings.

To be clear, this study does not show that self-control skills have no effect on economic well-being or violence. They might have very large effects. The research instead shows that the particular intervention under study had little effect on self-control skills and therefore self-control skills are unlikely to explain the large effects of the intervention on economic well-being or violence. Furthermore, if future practitioners were hoping to replicate the success of this behavioral therapy in Liberia, they might learn from this that, for whatever reason, they won't have much success changing self-control

skills, so they might be better off focusing more on other factors like social networks and attitudes toward violence, which seem to be the places where this particular approach has the most bite.

Independent Theoretical Predictions

Another way to get at specific mechanisms is to think theoretically and generate independent tests that might help adjudicate between different potential mechanisms. In chapter 7, we discussed how we could use independent theoretical predictions to probe whether some estimated effect was the result of noise (i.e., a false positive). There, we gave the example of re-examining the putative effect of college football games on elections. The idea here is similar. But now, instead of testing whether an observed relationship is genuine or spurious, we want to look for the mechanisms underlying an estimated effect that we believe is genuine.

A study by Sarah Anzia and Chris Berry illustrates how this can work.

Do Voters Discriminate Against Women?

There is much concern about the possibility of discrimination in the electoral process. For example, are voters biased against female candidates? Answering this question compellingly is difficult for obvious reasons. Discriminatory voters may not reveal their prejudices in surveys. And in real elections, there could be factors other than discrimination that could explain why women do better or worse than men.

Some scholars have noticed that in the United States, when female candidates run for office, they perform similarly to male candidates on average. This, they argue, suggests there may be little discrimination by voters. However, Anzia and Berry point out that if there is discrimination, we might expect that only the most qualified women will decide to run for office, which could explain why women do about as well as men even in a world with discrimination. So the fact that women perform as well as men, *when they run*, doesn't necessarily imply that voters aren't discriminating.

Continuing with this line of thinking, Anzia and Berry try to generate theoretical predictions that should hold if there indeed is discrimination against women in elections. One prediction is that if there is discrimination, all else equal, the women who are elected to office should be better at their jobs than the men who get elected. Because of discrimination, they will have to be better in order to get elected. Of course, we don't have perfect measures of job performance. But Anzia and Berry look at several measures of how members of Congress perform, including the number of bills they sponsor and also the amount of federal spending they bring home to their districts. The results are exactly in line with the theoretical prediction. Using a difference-in-differences design, they show that, on average (accounting for differences across districts and time periods to make the comparison as apples-to-apples as possible), women perform better in Congress than men, consistent with the possibility that women have to clear a higher hurdle in order to get elected because of voter discrimination.

Having uncovered an interesting and compelling phenomenon, Anzia and Berry push further still. The evidence is pretty clear that, on average, female members of Congress are more productive than male members of Congress, and discrimination is a potential explanation. But are there other mechanisms that could potentially explain this effect? What if, for example, women are just better at some parts of the job than men, regardless of any selection or discrimination? Or what if women are treated differently

once they get to Congress, not because of their ability but because they are viewed as a token minority?

To address these kinds of questions, Anzia and Berry keep thinking. If it's really discrimination that explains this result, are there further theoretical predictions that they can test? One prediction is that the gap in performance between elected women and men should be greater the more discrimination women face. Of course, we don't know for sure which congressional districts discriminate more. But one reasonable hypothesis is that more conservative districts will discriminate against female candidates more than more liberal districts. Therefore, Anzia and Berry test whether the difference in performance in Congress between men and women is greater in more conservative districts, as measured by how the district votes in presidential elections. The answer is yes.

To provide further evidence of their purported mechanism, Anzia and Berry note that one large group of female congressional representatives—those who gained office because they were the widow of a recently deceased member of Congress—likely did not have to overcome the same type of discrimination as other female candidates. As such, we would not expect them to be more qualified on average. And sure enough, Anzia and Berry find that widows do not perform better than male members of Congress and their performance is notably worse than female candidates who were elected independent of their spouses.

The compellingness of the Anzia and Berry study comes not from a single, airtight research design or statistical test demonstrating the presence of discrimination. Rather, they elucidate an interesting and plausible mechanism by generating a theory of discrimination in elections and testing multiple, independent predictions that follow from that theory.

Of course, this still doesn't settle the matter. Other mechanisms might also account for the observed patterns. For instance, a variety of scholars point to evidence that women may under-estimate their abilities or be averse to putting themselves forward as candidates. These mechanisms might also account for female representatives performing better in their jobs, conditional on winning election. So there is still lots of work to do in figuring out which mechanisms are at work. But, in our view, this study provides a model for using a combination of clear thinking and data analysis to try to provide a better understanding of causal mechanisms.

Testing Mechanisms by Design

In some special circumstances, we can design studies in ways that isolate particular mechanisms. Take, for example, a clever study on how social pressure affects voter turnout by Alan Gerber, Don Green, and Christopher Larimer.

Social Pressure and Voting

Gerber, Green, and Larimer mailed postcards to a randomly selected group of registered voters. The postcards informed the recipients which of their neighbors had and had not voted in recent elections. (You may not have known this, but in the United States, whether or not you voted is a matter of public record. Only how you voted is secret.) They also indicated that another, similar postcard would be sent to neighborhood residents after an upcoming election. The implication was that, if the recipient didn't vote in the upcoming election, all their neighbors would find out about it. This

unusual (and perhaps invasive) postcard increased voter turnout dramatically; people who received the postcard were 8 percentage points more likely to vote than a control group.

Having seen these experimental results, we might wonder why the postcards had such large effects—that is, we want to know through what mechanisms the post-cards cause increased voter turnout. Was the social aspect of the treatment important? Do people really not want their neighbors to know that they don't vote? Are people mobilized just because the postcard reminded them about the election? Or are people perhaps just trying to impress researchers and so turn out once they know they are being studied?

To learn about the importance of the social mechanism, the researchers designed their experiment to include another randomly assigned postcard. This postcard mimicked every feature of the previously discussed one, with one exception. Instead of containing voter turnout information about all of the recipient's neighbors, it only contained information about members of the recipient's household. Recipients of this type of postcard would no longer worry that all of their neighbors were going to find out if they didn't vote. Now it would just be the people they lived with who would learn about their voting behavior. And those people presumably already had a pretty good guess. The thought is that this small change takes a lot of the social pressure mechanism out of the intervention. And, indeed, this postcard also increased voter turnout, but only by about 5 percentage points relative to a control group.

The clever part of this design is that, by including multiple treatments in the experiment, the authors were able to estimate how much the social aspect of the first postcard matters. In particular, having voter turnout information made public across a whole neighborhood, instead of just within a household, appears to account for 3 percentage points of the overall 8 percentage point effect.

Disentangling Mechanisms

Sometimes, we can do a similar pulling apart of mechanisms even when we don't get to design the study ourselves. To do so, of course, we must have multiple research designs that make it possible to separately estimate the effects of different mechanisms. Let's see an example.

Commodity Price Shocks and Violent Conflict

For decades, scholars have studied economic conditions, violent conflict, and the causal relationships between the two. It's hard to think of a topic where the stakes are higher. We would love to improve economic conditions and reduce violent conflict around the world, but we don't seem to know how to do so.

We already discussed the difficulty of empirically assessing the effect of the economy on conflict in chapter 9. When we observe a strong correlation between conflict and poor economic conditions, it is unclear whether the former causes the latter, the latter causes the former, some confounding factors cause both, or some combination of all these possibilities is at work.

To gain more traction on one part of the problem, many scholars have tried to find research designs that allow us to more credibly estimate the effect of economic conditions on violent conflict. One common strategy involves using commodity price shocks as part of a difference-in-differences design. The basic idea is as follows.

Poppy farming is a major industry in parts of Afghanistan. One might think that Afghan farmers and farm workers making money in the poppy industry would be less willing to go fight because they'd be giving up relatively good economic opportunities. But, clearly, we can't just regress the amount of violence in different parts of Afghanistan against the amount of poppy farming to learn about the relationship between conflict and economic opportunity. Confounders abound. Poppies are the key raw ingredient to heroin, so poppy farming goes along with the drug trade, which might affect violence independently. Moreover, poppies grow in mountainous areas. And the terrain might also affect the amount of violence.

Another thought is that the willingness of poppy farmers and farm workers to fight might go down when the poppy business is particularly good and go up when it is bad. But, again, such an over-time comparison has potential confounders. Perhaps a surge in demand for poppies happens to also coincide with features of the season, U.S. troop deployments, or other factors that also affect violence.

But a difference-in-differences strategy might address both concerns. That is, we'd like to look at differences in violence in poppy-producing places when the poppy business is good versus bad, and we'd like to compare those differences to the same differences in non-poppy-producing places. The idea is that by accounting for any changes in violence over time and accounting for the possibility that the baseline levels of violence differ between poppy- and non-poppy-producing places, we can obtain a more credible estimate of the effect of economic prosperity on violence.

To pull off a strategy like this, of course, we need some measure of when the poppy business is good versus bad. For that, researchers use changes to the world price of poppies (or the world price of heroin). The idea is that, for most commodities, most countries are small players. So the world price of that commodity is unlikely to be strongly influenced by things that are happening in that country. And therefore, perhaps we can use changes in the world price to estimate the effect of local economic prosperity (remember instrumental variables from our discussion of noncompliance in chapter 11).

The idea is a pretty nice one. Using difference-in-differences designs, we can estimate the effects of economic shocks on violence more credibly than we could just by comparing violence levels in rich and poor places, or even over time.

Rather than doing this for just one commodity and one country, scholars have done the painstaking work of measuring how much of each of hundreds of commodities each country produces. From this, they create an index, for each country, of its commodity bundle. They have also collected the world price of each commodity for each year, so they can measure how much the value of each country's commodity bundle changes each year. From this, they can do a giant difference-in-differences analysis to see how violence changes in response to economic shocks the world over.

Interestingly, when scholars did this, they got a bunch of conflicting results. Sometimes it looked like positive economic shocks reduce violence, sometimes they appeared to increase violence, and sometimes they had no detectable effect. These contradictory and inconsistent findings were somewhat disconcerting. Better data and research designs are supposed to provide more, not less, definitive answers.

What is going on? Ernesto Dal Bo and Pedro Dal Bo suggested one possible theoretical explanation. They pointed out that there are at least two mechanisms through which economic conditions might influence conflict, and they pull in opposite directions. On the one hand, good economic conditions create more and better jobs, and workers with those good jobs might be less willing to leave them to fight. In other words,

good economic conditions increase the opportunity costs of fighting. If you're unemployed and hungry, then you might join a revolution, but having a good-paying job might be enough to deter that inclination. On the other hand, armed groups often fight over control of economic resources. Good economic conditions mean there is more to fight over. In other words, good economic conditions increase the benefits of predation. If you live in a desolate place with no economic value, what's the point in fighting over it? But if there's money to be made from controlling a booming economy, maybe it's worthwhile to fight. Perhaps the fact that a shock to economic conditions can activate these opposing forces explains the murky results of previous studies.

So where do we go from here? Knowing that there are competing forces is theoretically illuminating, but it's not enough to inform policy. Fortunately, Dal Bo and Dal Bo thought more about conditions under which one force might dominate the other. Their idea was that in labor-intensive industries and economies, the *opportunity cost* mechanism should dominate because economic shocks create more need for labor, higher wages, and better jobs. But in capital-intensive industries and economies, the *predation* mechanism should dominate because better economic conditions create more to fight over without meaningfully increasing wages or employment.

In a 2013 study, Oeindrila Dube and Juan Vargas found a way to empirically test these ideas using the sort of difference-in-differences strategy we've already described. They studied armed conflict in Colombia, and they focused most of their attention on two major industries: coffee and oil. Coffee is relatively labor-intensive, requiring lots of workers to farm and process. Oil is relatively capital-intensive: once an oil well is drilled, oil producers do not need lots of workers on hand. Importantly, some locales in Colombia have coffee-intensive economies, while others have oil-intensive economies. Consistent with theoretical predictions, positive shocks to the world price of coffee appear to reduce conflict in coffee-intensive locales relative to non-coffee-intensive locales. This is evidence in favor of the opportunity costs mechanism. By contrast, positive shocks to the world price of oil appear to increase conflict in oil-intensive locales relative to non-oil-intensive locales. This is evidence in favor of the predation mechanism.

This evidence suggests that economic conditions do indeed influence violent conflict through multiple mechanisms. As such, economic improvements can either mitigate or exacerbate conflict, depending on which mechanism dominates in a given context. This means that if we find no average relationship between economic shocks and conflict world-wide, that doesn't mean economic conditions don't matter for conflict. Rather, the average relationship might be masking multiple off-setting effects that we can only understand if we disentangle the mechanisms. So, perhaps economic aid can mitigate conflict, but only when it corresponds with job opportunities. Economic aid that simply increases the size of the economic pie that warring factions can fight over, but does not provide employment opportunities, will likely make matters worse. This is an important insight that could only have been discovered through the interplay of data, research design, theory, and clear thinking. The data alone wasn't enough. Nor was a good research design. It took all of these tools together.

Wrapping Up

We've accomplished a lot in part 3. We fulfilled one of the central goals of the book—really understanding why correlation need not imply causation. We explored why causal inference is so difficult and delved into the intellectually exciting world of

creative research designs that can help us credibly estimate causal effects and uncover mechanisms. This is really important material, and we hope you feel good about what you've learned.

But, on its own, even knowledge of causal effects is not sufficient for making the best use of quantitative information to inform decisions. In part 4 we turn to some final topics that will help us think clearly about the right questions, the right evidence to answer those questions, and the limits of quantification.

Key Terms

- **Mediator:** A feature of the world that is affected by the treatment and affects the outcome.
- **Causal mediation analysis:** Techniques for trying to estimate how much of the effect of a treatment on an outcome is the result of the treatment's effect on a mediator and the mediator's effect on the outcome.

Exercises

14.1   In the 1990s, the U.S. Department of Housing and Urban Development ran a large-scale field experiment called Moving to Opportunity. They randomly selected some households living in high-poverty public housing projects and offered them housing vouchers (i.e., money that could be used to pay rent) if they moved to a low-poverty neighborhood. Other households were given nothing. The goal was to learn whether moving to a low-poverty neighborhood would be beneficial for economic, mental, and physical well-being.

Researchers have examined the data and found that households in the treated condition (receiving a voucher to move to a low-poverty neighborhood) experienced better physical health, mental health, and subjective well-being than those in the untreated condition (receiving nothing). There were no significant differences in economic outcomes.

(a) This result is strong evidence that a treatment—receiving a housing voucher that you can use only by moving to a low-poverty neighborhood—causes improved well-being. Does this imply that the treatment works through the mechanism of moving people to a low-poverty neighborhood? Offer at least one other mechanism that might explain the result.

(b) How could you modify or add to the experiment in order to better elicit the effect of moving to a low-poverty neighborhood?

(c) There was actually one additional treatment considered in the experiment. Another group of individuals were randomly assigned to receive a housing voucher to move anywhere they liked (not just to a low-poverty neighborhood). In light of this information, what comparison(s) would you make in order to separate the effect of moving to a low-poverty neighborhood from the potential effects of moving in general or receiving the financial benefit of a voucher?

(d) *Bonus challenge:* Not surprisingly, there was some noncompliance in this experiment—some people who were assigned to the voucher

treatment chose not to move. And as you can imagine, the rate of compliance was different between the two treatments: 63 percent of households used the voucher when there was no restriction on where they could move, but only 48 percent used it when it required that they move to a low-poverty neighborhood. Further complicating matters, some of the households in the unrestricted condition moved to low-poverty neighborhoods even though they didn't have to. What would you do if you wanted to estimate the effect of moving to a low-poverty neighborhood in light of these noncompliance issues? (There's no easy answer, but we hope it's illuminating to think through all the complications and appreciate how hard it is to learn about causal mechanisms.)

14.2   There is some compelling evidence that education increases political participation. Let's think about why this might be.

Some people hypothesize this is due, at least in part, to an income or wealth mechanism. Perhaps education increases economic prosperity, wealthy people care more about taxes or economic policy, and therefore they are more likely to vote. Consider the following kinds of evidence that might be brought to bear on this hypothesis about mechanisms. How convincing do you find each, and why?

(a) Suppose, if you run a regression of voter turnout on years of schooling, you get a large coefficient, and if you run another regression of voter turnout on years of schooling and income, the coefficient associated with schooling is notably smaller.

(b) Because of compulsory schooling requirements, people born toward the end of the calendar year tend to get more education (because they are young for their grade and so have to stay in school a year longer before they can drop out). Exploiting this natural experiment and using instrumental variables, labor economists have estimated that schooling significantly increases earnings. Using another natural experiment, researchers have found that winning the lottery increases voter turnout.

(c) Suppose you find that among people who obtain college degrees in engineering, those in higher-paid specialities (e.g., aerospace, chemical, and petroleum) participate in politics more than those in lower-paid specialities (e.g., civil, environmental, and mechanical).

Readings and References

If you are interested in learning more about causal mediation analysis, you could start with these readings:

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Kosuke Imai, Luke Keele, Dustin Tingley, and Teppei Yamamoto. 2011. "Unpacking the Black Box of Causality: Learning about Causal Mechanisms from Experimental and Observational Studies." *American Political Science Review* 105(4):765–89.

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The studies on economic prosperity and conflict are

Ernesto Dal Bo and Pedro Dal Bo. 2011. "Workers, Warriors, and Criminals: Social Conflict in General Equilibrium." *Journal of the European Economic Association* 9(4):646–77.

Oeindrila Dube and Juan F. Vargas. 2013. "Commodity Price Shocks and Civil Conflict: Evidence from Colombia." *Review of Economic Studies* 80:1384–1421.

There is a ton of work on the effects of Moving to Opportunity. For a classic paper, see

Lawrence F. Katz, Jeffrey R. Kling, and Jeffrey B. Liebman. 2001. "Moving to Opportunity in Boston: Early Results of a Randomized Mobility Experiment." *Quarterly Journal of Economics* 116(2):607–54.