Field Experiments: Design, Analysis and Interpretation
Solutions for Chapter 10 Exercises

Alan S. Gerber and Donald P. Green[*]

Question 1

Important concepts:

a) Suppose that equations (10.1), (10.2), and (10.3) depict the true causal process that generates outcomes. Referring to these equations, define the direct effect of Z_i on Y_i and the indirect effect that Z_i transmits through M_i to Y_i.
Answer:
The direct effect is the causal influence that is transmitted from Z_i to Y_i without passing through M_i, and the indirect effect is the causal influence that passes from Z_i to Y_i through M_i. The direct effect of Z_i on Y_i is the parameter d in equation (10.3). The indirect or "mediated" effect is the product ab.

b) Explain why the equation Total effect = Direct effect + Indirect effect breaks down when the parameters of equations (10.1), (10.2), and (10.3) vary across subjects.
Answer:
The indirect or "mediated" effect is the product ab, but when these two parameters vary, their expected product is not in general equal to the product of their expectations. Thus, one cannot estimate the average a_i using equation (10.1) and multiply it by the estimate of the average b_i from equation (10.3) in order to obtain an estimated whose expected value is $E[a_i b_i]$.

c) Suppose that the effect of M_i on Y_i varies from one subject to the next. Show that the indirect effect of Z_i on Y_i is zero when the treatment effect of Z_i on M_i is zero for all subjects.
Answer:
When a_i is zero for all subjects, the expected product of a_i and b_i is zero: $E[a_i b_i] = aE[b_i] = 0E[b_i] = 0$.

d) Explain why the complex potential outcome $Y_i(M_i(0), 1)$ defies empirical investigation.
Answer:
The expression $Y_i(M_i(0), 1)$ denotes the potential outcome that would occur given two inputs: $Z_i = 1$ (i.e., the subject is assigned to the treatment group) and M_i were the value it would take on if $Z_i = 0$. These are two incompatible conditions, since Z_i is either 1 or 0. When $Z_i = 1$, for instance, the outcome we observe is $Y_i(M_i(1), 1)$; when $Z_i = 0$, the outcome we observe is $Y_i(M_i(0), 0)$.

e) Explain the distinction between the indirect effect that Z_i transmits to Y_i through M_i given in equations (10.15) and (10.16) and the causal effect of M_i, defined using $Y_i(m, z)$ notation as

[*]Solutions prepared by Peter M. Aronow and revised by Alexander Coppock

$Y_i(1,0) - Y_i(0,0)$ or $Y_i(1,1) - Y_i(0,1)$. (Hint: Look closely at how the mediator takes on its value).
Answer:
Equations 10.15 and 10.16 involve complex potential outcomes, which are inherently unobservable. The causal effect of M holding Z constant involves two potentially observable potential outcomes. The difference is that in the latter comparison, we are not trying to set the value of the mediator to its potential outcome in the wake of a manipulation of Z. Instead, we are just setting M to a value and holding Z constant.

Question 2

When researchers use an encouragement design to study mediation, what assumptions must they make in order to satisfy the CACE Theorem from Chapter 6?
Answer:
The CACE theorem assumes non-interference, excludability, and monotonicity. The latter two assumptions may be especially problematic in the context of mediation analysis. Excludability implies that potential outcomes for Y_i respond solely to M_i, whereas the regression framework of equation (10.3) allows Y_i to respond to both M_i and Z_i. When the mediating variable M_i is binary, the monotonicity assumption implies that there are no Defiers (subjects for whom $M_i = 1$ if and only if they are assigned to the control group).

Question 3

Consider the following schedule of potential outcomes for 12 observations. This table illustrates a special situation in which the disturbance e_{1i} is unrelated to the disturbance e_{3i}.

Table 1: Question 3 Table

Observation	Yi(m = 0, z = 0)	Yi(m = 0, z = 1)	Yi(m = 1, z = 0)	Yi(m = 1, z = 1)	Mi(z = 0)	Mi(z = 1)
1	0#	0*	0	0	0	0
2	0	0*	0#	0	0	1
3	0	0	0#	0*	1	1
4	0#	1*	0	1	0	0
5	0	1*	0#	1	0	1
6	0	1	0#	1*	1	1
7	1#	0*	1	1	0	0
8	1	0*	1#	1	0	1
9	1	0	1#	1*	1	1
10	0#	1*	1	1	0	0
11	0	1*	1#	1	0	1
12	0	1	1#	1*	1	1

a) What is the average effect of Z_i on M_i?
Answer:
The average effect of Z on M is the average difference between the last two columns on p.339: $\frac{1}{3}$

b) Use yellow to highlight the cells in the table of potential outcomes to indicate which potential outcomes for Y_i correspond to $Y_i(M_i(0), 0)$. Use green to highlight the cells in the table of potential outcomes to indicate which potential outcomes for Y_i correspond to $Y_i(M_i(1), 1)$. Put an asterisk by the potential outcomes for Y_i in each row that correspond to the complex potential outcome $Y_i(M_i(0), 1)$. Put a pound sign by the potential outcomes for Y_i in each row that correspond to the complex potential outcome $Y_i(M_i(1), 0)$.

c) What is the average total effect of Z_i on Y_i?
Answer:
This difference is green minus yellow = 8/12 - 4/12 = 1/3

d) What is the average direct effect of Z_i on Y_i holding M_i constant at $M_i(0)$? Hint: see equation (10.13).
Answer:
This difference is asterisk minus yellow = 7/12 - 4/12 = 1/4

e) What is the average direct effect of Z_i on Y_i holding M_i constant at $M_i(1)$? Hint: see equation (10.14).
Answer:
This difference is green minus pound sign = 8/12 - 5/12 = 1/4

f) What is the average indirect effect that Z_i transmits through M_i to Y_i when $Z_i = 1$? Hint: see equation (10.15).
Answer:
This difference is green minus asterisk = 8/12 - 7/12 = 1/12

g) What is the average indirect effect that Z_i transmits through M_i to Y_i when $Z_i = 0$? Hint: see equation (10.16).
Answer:
This difference is pound sign minus yellow = 5/12 - 4/12 = 1/12

h) In this example, does the total effect of Z_i equal the sum of its average direct and indirect effect?
Answer:
Yes because the average of the direct effects is 1/4 and the average of the indirect effects is 1/12, which sums to the total effect, 1/3

i) What is the average effect of M_i on Y_i when $Z_i = 0$?
Answer:
This is the 3rd column minus the 1st column: 6/12 - 3/12 = 3/12

j) Suppose you were to randomly assign half of these observations to treatment ($Z_i = 1$) and the other half to control ($Z_i = 0$). If you were to regress Y_i on M_i and Z_i, you would obtain unbiased estimates of the average direct effect of Z_i on Y_i and the average effect of M_i on Y_i. (This fact may be verified using the R simulation at http://isps.research.yale.edu/FEDAI.) What special features of this schedule of potential outcomes allows for unbiased estimation?
Answer:
See simulation below and following question for answer.

```
In [1]: set more off
        input z Y0M0 Y1M0 Y0M1 Y1M1 M0 M1
```

```
        0 0 0 0 0 0 0
        0 0 0 0 0 0 1
        0 0 0 0 0 1 1
        0 0 1 0 1 0 0
        0 0 1 0 1 0 1
        0 0 1 0 1 1 1
        1 1 0 1 1 0 0
        1 1 0 1 1 0 1
        1 1 0 1 1 1 1
        1 0 1 1 1 0 0
        1 0 1 1 1 0 1
        1 0 1 1 1 1 1
        end
```

```
In [2]: tabstat Y0M0 Y1M0 Y0M1 Y1M1, stat(mean)

        gen M = .
        gen Y = .
```

```
   stats |      Y0M0      Y1M0      Y0M1      Y1M1
---------+----------------------------------------
    mean |       .25        .5        .5       .75
--------------------------------------------------
```

```
In [3]: //coefmat
        capture program drop coef
        program define coef, rclass
                replace M = M0*(1-z) + M1*z
                replace Y = Y0M0*(1-z)*(1-M) +
                        Y1M0*(z)*(1-M) + Y0M1*(1-z)*(M) + Y1M1*(z)*(M)
                qui reg Y M z
                return scalar coy = _b[_cons]
                return scalar com = _b[M]
                return scalar coz = _b[z]
                return scalar nocoli = _se[z]
        end

        qui tsrtest z r(coy) using co_y.dta, overwrite: coef
        qui tsrtest z r(com) using co_m.dta, overwrite: coef
        qui tsrtest z r(coz) using co_z.dta, overwrite: coef
        qui tsrtest z r(nocoli) using nocoli.dta, overwrite: coef
```

```
In [4]: // tcoefmat
        capture program drop tcoef
        program define tcoef, rclass
```

```
        replace M = M0*(1-z) + M1*z
        replace Y = Y0M0*(1-z)*(1-M) + Y1M0*(z)*(1-M) + Y0M1*(1-z)*(M) + Y1M1*(z)*(M
        qui reg Y z
        return scalar tcoy = _b[_cons]
        return scalar tcoz = _b[z]
end

qui tsrtest z r(tcoy) using tco_y.dta, overwrite: tcoef
qui tsrtest z r(tcoz) using tco_z.dta, overwrite: tcoef
```

In [5]:
```
// mcoefmat
capture program drop mcoef
program define mcoef, rclass
        replace M = M0*(1-z) + M1*z
        qui reg M z
        return scalar mcom = _b[_cons]
        return scalar mcoz = _b[z]
end

qui tsrtest z r(mcom) using mco_m.dta, overwrite: mcoef
qui tsrtest z r(mcoz) using mco_z.dta, overwrite: mcoef
```

In [6]:
```
// colMeans(na.omit(coefmat))
preserve
qui use "co_y.dta", clear
qui rename theta co_y

qui merge 1:1 _n using "co_m.dta"
qui rename theta co_m

qui drop _merge
qui merge 1:1 _n using "co_z.dta"
qui rename theta co_z
qui drop _merge

//check colinearity
qui merge 1:1 _n using "nocoli.dta"
qui rename theta nocoli
qui drop _merge


qui drop if _n == 1
// omit instances of perfect colinearity between M and Z
qui drop if nocoli==0

tabstat co_y co_m co_z,stat(mean)

restore
```

```
   stats |      co_y      co_m      co_z
---------+------------------------------
    mean |       .25       .25       .25
----------------------------------------
```

```
In [7]: // colMeans(na.omit(tcoefmat))
        preserve
        qui use "tco_y.dta", clear
        qui rename theta tco_y

        qui merge 1:1 _n using "tco_z.dta"
        qui rename theta tco_z
        qui drop _merge

        qui /*check coli*/
        qui merge 1:1 _n using "nocoli.dta"
        qui rename theta nocoli
        qui drop _merge

        // drop the observation statistics
        qui drop if _n == 1

        tabstat tco_y tco_z,stat(mean)
        restore
```

```
   stats |     tco_y     tco_z
---------+--------------------
    mean |  .3333333  .3333333
------------------------------
```

```
In [8]: //colMeans(na.omit(mcoefmat))
        preserve
        qui use "mco_m.dta", clear
        qui rename theta mco_m

        qui merge 1:1 _n using "mco_z.dta"
        qui rename theta mco_z
        qui drop _merge

        // drop the observation statistics
        qui drop if _n == 1
        tabstat mco_m mco_z,stat(mean)
        restore
```

```
   stats |     mco_m     mco_z
---------+--------------------
    mean |  .3333333  .3333333
------------------------------
```

k) In order to estimate average indirect effect that Z_i transmits through M_i to Y_i, estimate the regressions in equations (10.1) and (10.3) and multiply the estimates of a and b together.[1] Use the simulation to show that this estimator is unbiased when applied to this schedule of potential outcomes. Why does this estimator, which usually produces biased results, produce unbiased results in this example?
Answer:

```
In [9]: preserve

         qui use "mco_z.dta", clear
         qui rename theta mco_z

         qui merge 1:1 _n using "co_z.dta"
         qui rename theta co_z
         qui drop _merge

         // check colinearity
         qui merge 1:1 _n using "nocoli.dta"
         qui rename theta nocoli
         qui drop _merge

         qui gen asbs = mco_z*co_z
         qui drop if _n == 1
         // omit instances of perfect colinearity between M and Z
         qui drop if nocoli==0


         tabstat asbs,stat(mean)
         restore



    variable |      mean
-------------+----------
        asbs |   .082244
------------------------
```

The simulation confirms that the results are unbiased (excluding random assignments that result in perfect collinearity between Z and M) for the direct and total effects. The reason is that the special conditions (1) constant direct and indirect effects on Y and (2) no relationship between

[1] Text mistakenly has "multiply estimates of a and c together.

unobserved causes of Y and unobserved causes of M. In effect, M is as good as randomly assigned in this special case.

Question 4

Earlier we indicated that in Bhavnani's experiment, the pathway between random reservations for women and voter turnout appears to be zero, suggesting that we may be able to rule out this mediator as a possible pathway.

a) With the replication dataset at http://isps.research.yale.edu/FEDAI, use randomization inference to test the sharp null hypothesis of no treatment effect on turnout in 2002 for any subject.

```
In [1]: import delim ./data/chapter10/Bhavnani_APSR_2009,clear
        rename controltreat z
        rename turnout y

        ritest z _b[z], reps(1000) nodots: regress y z

  res. var(s):  z
   Resampling:  Permuting z
Clust. var(s):  __000001
     Clusters:  227
Strata var(s):  none
       Strata:  1

------------------------------------------------------------------------------
T            |     T(obs)       c       n   p=c/n   SE(p) [95% Conf. Interval]
-------------+----------------------------------------------------------------
       _pm_1 |  -.6234801     572    1000  0.5720  0.0156  .5406588   .6029161
------------------------------------------------------------------------------
Note: Confidence interval is with respect to p=c/n.
Note: c = #{|T| >= |T(obs)|}


In [2]: //ate
         di %8.7f el(r(b), 1, 1)

-0.6234801


In [3]: // p-value two-sided
         di %8.4f el(r(p), 1, 1)

  0.5720
```

b) Following the steps described in Chapter 9, use randomization inference to test the null hypothesis that $Var(\tau_i) = 0$.

```
In [4]: // p-value for one-tailed comparison
        ritest z  testvar=((r(sd_2)^2)-(r(sd_1)^2)), ///
        reps(10000) sav(10_4_var.dta, replace) nodots: ///
        sdtest y, by(z)

  res. var(s):  z
   Resampling:  Permuting z
Clust. var(s):  __000004
     Clusters:  227
Strata var(s):  none
       Strata:  1

------------------------------------------------------------------------------
T            |     T(obs)       c       n   p=c/n   SE(p) [95% Conf. Interval]
-------------+----------------------------------------------------------------
     testvar |   6.723442    6294   10000  0.6294  0.0048  .6198475   .6388771
------------------------------------------------------------------------------
Note: Confidence interval is with respect to p=c/n.
Note: c = #{|T| >= |T(obs)|}


In [5]: global testvar = el(r(b), 1, 1)

In [6]: set more off
        preserve
        use "10_4_var.dta", clear

        qui count if testvar>=$testvar
        // one-tailed p-value
        di %8.4f r(N)/_N

        qui count if abs(testvar)>=abs($testvar)
        // tow-tailed p-value
        di %8.4f r(N)/_N
        restore


// one-tailed p-value
  0.2851

// two-tailed p-value
  0.6294
```

c) It is tempting to include voter turnout in 1997 as a covariate when assessing the relationship between reservations and turnout in 2002, but is turnout in 1997 a pre-treatment covariate? Explain why or why not.
Answer:

No. Turnout in 1997 occurs after random assignment and may be affected by randomly assigned reservations for women candidates in the 1997 election.

Question 5

In most places in the United States, you can only vote if you are a registered voter. You become a registered voter by filling out a form and, in some cases, presenting identification and proof of residence. Consider a jurisdiction that requires and enforces voter registration. Imagine a voter registration experiment that takes the following form: unregistered citizens are approached at their homes with one of two randomly chosen messages. The treatment group is presented with voter registration forms along with an explanation of how to fill them out and return them to the local registrar of voters. The control group is presented with an encouragement to donate books to a local library and receives instructions about how to do so. Voter registration and voter turnout rates are compiled for each person who is contacted using either script. In the table below, Treatment = 1 if encouraged to register, 0 otherwise; Registered = 1 if registered, 0 otherwise; Voted = 1 if voted, 0 otherwise; and N is the number of observations).

Table 2: Question 5 Table

Treatment	Registered	Voted	N
0	0	0	400
0	0	1	0
0	1	0	10
0	1	1	90
1	0	0	300
1	0	1	0
1	1	0	100
1	1	1	100

a) Estimate the average effect of Treatment (Z_i) on Registered (M_i). Interpret the results.
Answer:
The registration rate is 40% in the treatment group and 20% in the control group, for an ATE of 0.20, or 20 percentage points.

b) Estimate the average total effect of treatment on voter turnout (Y_i).
Answer:
The turnout rate is 20% in the treatment group and 18% in the control group, for an ATE of 0.02, or 2 percentage points.

c) Regress Y_i on X_i and M_i. What does this regression seem to indicate? List the assumptions necessary to ascribe a causal interpretation to the regression coefficient associated with M_i. Are these assumptions plausible in this case?
Answer:

```
In [1]: clear
        set obs 1000
```

```
egen y = fill(0,0)
replace y = 1 in 411/500
replace y =1 in 901/1000
egen z = fill(0,0)
replace z = 1 in 501/1000
egen m = fill(0,0)
replace m = 1 in 401/500
replace m = 1 in 801/1000
regress y z m


      Source |       SS           df       MS      Number of obs   =     1,000
-------------+----------------------------------   F(2, 997)       =    652.06
       Model |       87.22         2       43.61   Prob > F        =    0.0000
    Residual |       66.68       997  .066880642   R-squared       =    0.5667
-------------+----------------------------------   Adj R-squared   =    0.5659
       Total |       153.9       999  .154054054   Root MSE        =    .25861

------------------------------------------------------------------------------
           y |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
           z |      -.112     .01676    -6.68   0.000     -.144889    -.079111
           m |        .66   .0182867    36.09   0.000     .6241152    .6958848
       _cons |       .048     .01213     3.96   0.000     .0241967    .0718033
------------------------------------------------------------------------------
```


The results seem to suggest that registration has a strong effect on voter turnout, which makes intuitive sense; however, registration per se is not randomly assigned, and so this regression estimator may be biased. The regression also seems to indicate that the treatment exerts a negative effect on turnout holding registration constant. This finding makes no sense substantively; intuitively, one would think that the treatment should, if anything, have a positive effect net of its indirect via registration because the act of encouraging someone to register may also make them more interested in voting. Because Z and M are correlated, the inclusion of M (a post-treatment covariate) may lead to biased estimation of BOTH causal effects.

d) Suppose you were to assume that the treatment has no direct effect on turnout; its total effect is entirely mediated through registration. Under this assumption and monotonicity, what is the Complier average causal effect of registration on turnout?
Answer:
As noted above, the estimated ITT is 0.02, and the estimated ITT_d is 0.20, so the ratio of the two quantities is $0.02/0.20 = 0.10$. Among Compliers (those who register if and only if encouraged), the ATE of registration is a 10 percentage point increase in turnout.

Question 6

Fellner, Sausgruber, and Traxler (2009) collaborated with an Austrian tax collection agency to examine the conditions under which people who own televisions pay the mandatory annual fee when requested to do so via an official letter from the agency.[2] The researchers randomly varied the content of the mailings so that it emphasized either (1) a threat of prosecution for tax evasion, (2) a fairness appeal to pay one's fair share rather than forcing others to bear one's tax burden, or (3) information stating the descriptive norm that 94% of households comply with this tax. These interventions seem to accentuate three mediators: fear of punishment, concern for fairness, and conformity with perceived norms. There are two outcome measures. One is whether the recipient responded to the request for an explanation for non-payment by mailing in a prepaid envelope. The other outcome, which is a subset of the first, is payment of the registration fee. The table above presents an excerpt of the results.

Table 3: Question 6 Table

	No mail	Standard letter	Letter with threat	Letter with norms	Letter with threat & norms	Letter with appeal to fairness	Letter with threat & fairness
Payment of registration fee	1.58%[3]	0.0862	0.0967	0.0823	0.097	0.0819	0.0932
Any response from recipient	N/A	0.4309	0.4501	0.407	0.4277	0.3882	0.4281
N	2586	6858	6694	6825	6960	6920	6750

a) This experiment included two control groups, one that received no letter and another that received a standard letter. Explain how the use of two control groups aids the interpretation of the results.
Answer:

The use of the standard letter helps the researchers assess the effect of the specific content of the various letters, holding constant the receipt of an official letter. For example, by comparing the STANDARD LETTER to the LETTER WITH THREAT, the researcher is able to assess the effects of threat among those who receive a letter of some sort. The NO MAIL group enables the researcher to assess the effect of receiving some sort of letter. If the aim is to assess the policy implications of sending out a given type of letter as opposed to nothing at all, the appropriate control group is the NO MAIL condition.

b) Analyze the data using the statistical model of your choice, and assess the effectiveness of threats, assertion of norms, and appeals to fairness.

```
In [1]: clear
        set obs 43593
        egen condition = repeat(), values("No Mail")
        replace condition = "Standard" in 2587/9444
        replace condition = "Threat" in 9445/16138
        replace condition = "Norms" in 16139/22963
        replace condition = "Threat+Norms" in 22964/29923
        replace condition = "Fairness" in 29924/36843
```

[2]Fellner, Sausgruber, and Traxler 2009.

```
        replace condition = "Threat+Fairness" in 36844/43593

        gen no_mail = 1 if strpos(condition, "Mail") > 0
        replace no_mail = 0 if no_mail ==.
        gen standard = 1 if strpos(condition, "Standard") > 0
        replace standard = 0 if standard ==.
        gen threat = 1 if strpos(condition, "Threat") > 0
        replace threat = 0 if threat ==.
        gen norms = 1 if strpos(condition, "Norms") > 0
        replace norms = 0 if norms ==.
        gen fairness = 1 if strpos(condition, "Fairness") > 0
        replace fairness = 0 if fairness ==.

        egen y = fill(1,1)
        replace y = 0 in 41/2586
        replace y = 0 in 3178/9444
        replace y = 0 in 10092/16138
        replace y = 0 in 16701/22963
        replace y = 0 in 23639/29923
        replace y = 0 in 30491/36843
        replace y = 0 in 37473/43593
```

```
In [2]: gen threatnorms = threat*norms

        gen threatfairness  = threat*fairness

        regress y no_mail standard threat norms fairness threatnorms threatfairness , nocons
```

```
      Source |       SS           df       MS      Number of obs   =    43,593
-------------+----------------------------------   F(7, 43586)     =    609.55
       Model |   330.89642         7  47.2709171   Prob > F        =    0.0000
    Residual |  3380.10358    43,586  .077550213   R-squared       =    0.0892
-------------+----------------------------------   Adj R-squared   =    0.0890
       Total |        3711    43,593  .085128346   Root MSE        =    .27848


--------------------------------------------------------------------------------
             y |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
---------------+----------------------------------------------------------------
       no_mail |   .0154679   .0054762     2.82   0.005     .0047345    .0262013
      standard |   .0861767   .0033627    25.63   0.000     .0795857    .0927678
        threat |   .0966537   .0034037    28.40   0.000     .0899824     .103325
         norms |   .0823443   .0033709    24.43   0.000     .0757374    .0889513
      fairness |   .0819364   .0033476    24.48   0.000      .075375    .0884978
   threatnorms |  -.0820153   .0058387   -14.05   0.000    -.0934592   -.0705714
threatfairness |   -.085405    .005855   -14.59   0.000    -.0968808   -.0739291
--------------------------------------------------------------------------------
```

This regression models the proportion of people paying fees as an additive function of each letter's content. No intercept is included, so we include dummy variables for each of the core treatments (NOMAIL, STANDARD, THREAT, NORMS, FAIRNESS) and interactions between THREAT and NORMS and between THREAT and FAIRNESS. These interaction terms are coded 1 if the letter contains both of these ingredients and 0 otherwise. Regression suggests that both of these interactions are strongly negative, which implies that the addition of a second ingredient undercuts the effects of the first ingredient. For example, the effect of THREAT is an increase of 9.7 percentage points; the effect of FAIRNESS is an increase of 8.2 percentage points. However, when threat and fairness appear in same letter, the effect is 9.7 + 8.2 - 8.5 = 9.4 percentage points, which is a slightly smaller effect than THREAT alone.

c) What light do these results shed on the question of why people respond (or fail to respond) to requests to pay taxes?
Answer:
If one isolates the core treatments (THREAT, NORMS, and FAIRNESS), it appears that THREAT is most effective, and THREAT is the only core treatment that is more effective than the STANDARD letter. Neither FAIRNESS nor NORMS seems particularly effective by themselves, nor do they appear to enhance the effectiveness of THREAT appeals.

Question 7

Several experimental studies conducted in North America and Europe have demonstrated that employers are less likely to reply to job applications from ethnic minorities than from non-minorities.

a) Propose at least two hypotheses about why this type of discrimination occurs.
Answer:
Hypothesis 1: Employers believe that ethnic minorities are less productive; according to this hypothesis, discrimination occurs because of rational economic calculations, not hostility toward ethnic minorities. Hypothesis 2: Employers tend to be hostile to ethnic minorities and discriminate against them in order to maintain "social distance" from them. Hypothesis 3: Employers themselves believe ethnic minorities to be as productive as non-minorities and do not discriminate out of animus toward them, but employers believe that their current employees look down on ethnic minorities and defer to their employees' tastes.

b) Propose an experimental research design to test each of your hypotheses, and explain how your experiment helps identify the causal parameters of interest.
Answer:
There is no ideal way to test these hypotheses, because each of them involves individual beliefs or tastes, which are unobserved. Some suggestive evidence, however, may be generated by experimentally inducing changes to beliefs or accommodating tastes. In order to test hypothesis 1, the application letter could provide evidence of qualifications and work experience attesting to the applicant's productivity; the point of this test is to see whether stereotypes about productivity can be overcome by applicant-specific information. The hostility hypothesis is more difficult to test, since it involves an interaction between the employer's attitudes and the minority treatment. In principle, one could conduct an unrelated survey of employers in order to gauge their attitudes toward various groups and assess whether their pattern of discrimination toward the fictitious applicants coincides with their general attitudes as expressed in response to the survey. Regarding the last hypothesis, one might devise a treatment that signals that the applicant is an especially likable and friendly person who fits in well in any situation.

c) Create a hypothetical schedule of potential outcomes, and simulate the results of the experiment you proposed in part (b). Analyze and interpret the results.
Answer:

Table 4: Hypothetical schedule of potential outcomes for Question 7

Employer Type	Outcome	Y(Non-minority)	Y(Minority)	Y(Productive Minority)	Y(Likeable Minority)
Hostile	Grants Interview	50	25	25	30
Hostile	No Interview	950	975	975	970
Accepting	Grants Interview	100	75	100	80
Accepting	No Interview	900	925	900	920

The above table simulates potential outcomes for 1000 people who, in response to a survey, express hostility toward minorities and 1000 people who are accepting of them. Each of these blocks could be randomly divided into four experimental groups, each of which receives one of the treatments. Suppose the results of the experiment were close to the expected proportions given above. The numbers above imply that employers in each block discriminate against minorities. Both groups are 2.5 percentage points more likely to interview a non-minority applicant than a minority applicant; since hostile employers are (for unknown reasons) less likely to interview any applicant, the ethnicity cue has a much larger effect on the odds they will grant an interview than it does on the odds that an accepting employer will grant an interview. Cues that the candidate is productive have no effect on hostile employers but eliminate the difference between minority and non-minority candidates among accepting employers. This treatment-by-covariate interaction (not necessarily causal, but suggestive) suggests that animus causes hostile employers to disregard applicants' qualifications; among the accepting, a showing of qualifications overcomes the presupposition that ethnic candidates are less productive. The likability treatment has little effect, suggesting that the consideration of who will "fit in" to the employment environment plays a small role in the decision to interview.

Question 8

Sometimes is it difficult and costly to conduct a long-term evaluation of policies or programs. For example, many states have instituted civics education requirements in high schools on the grounds that this type of curriculum makes for a more knowledgeable and involved citizenry. However, it is often impossible to track students after they leave high school. Suppose you were asked to evaluate the impact of a recommended civics curriculum that is being considered by a state that currently does not have a civics requirement. You may randomly assign a large number of schools and students to different curricula, but you can only measure outcomes up to the point at which students leave school.

a) Propose one or more mediating variables that you think explain why civics classes affect the attitudes and behaviors of students after they leave school.
Answer:
One hypothesis is that civics teaches students about the importance of public affairs. Another

hypothesis is that civics provides information about how to get involved in community activities and politics.

b) Propose a research design that would shed light on whether your hypothesized mediating variables are affected by civics classes.
Answer:
Randomly assign 10^{th} grade students to three groups: a no-civics group, a group that is exposed to a yearlong curriculum that emphasizes the importance of public affairs, and a group that is exposed to a yearlong curriculum that exposes students to a variety of local community service and political opportunities. Interview students at the end of their 10^{th}, 11^{th}, and 12^{th} grade years about their interest in public affairs and willingness to volunteer for local community service or political activities.

c) One problem with measuring short term outcomes is that effects may dissipate over time. Although your study cannot address this issue directly because long-term outcomes cannot be measured, suggest ways in which your design could at least shed some light on the rate at which effects decay over time.
Answer:
Decay could be studied by assessing whether the treatment effect observed immediately after the yearlong class diminishes when students are reinterviewed after 11th grade (one year later) and after 12th grade (two years later).

Question 9

Researchers who attempt to study mediation by adding or subtracting elements of the treatment confront the practical and conceptual challenge of altering treatments in ways that isolate the operation of a single causal ingredient. Carefully compare the four mailings from the Gerber et al. (2008) study, which are reproduced in the appendix to this chapter.

a) Discuss the ways in which the treatments differ from one another.
Answer:
The four treatments are: Civic Duty, Hawthorne, Self, and Neighbors. Civic Duty emphasizes citizens' responsibilities to participate in the Democratic process. Hawthorne simply informs subjects that they are under study. Self and Neighbors reveal voter history: the self treatment informs subjects of their past voter history and the neighbors treatment informs subjects of their own past voter history and that of their neighbors. Also, Self and Neighbors promise to send an updated vote history.

b) How might these differences affect the interpretation of Table 10.2?
Answer:
The largest difference is between the control group and the neighbors treatment. The reasons why the neighbors treatment are so effective may be many. It could be that the treatment reminds subjects of their civic duty. It could be that the treatment reminds subjects that they are being studied. It could be that the treatment reminds subjects of their own voter behavior. The other treatments in the experiment explicitly vary these factors. This allows us to conclude that social pressure is indeed the causative ingredient in the neighbors treatment.

c) Suppose you were in charge of conducting one or more "manipulation checks" as part of this study. What sorts of manipulation checks would you propose, and why?

Answer:
The following manipulation checks would be helpful. For all treatment groups, a question such as "Have you received any mail encouraging you to vote in the past three months?" would verify that treatment subjects did receive more encouragements than control subjects. For the "Self" and "Neighbors" treatments, a question such as "Did you vote in the November 2004 election" might reveal if the treatments increased subjects' recall. Another idea: ask a random subset (so as not to disrupt voting habits among a large segment of the subject pool) whether voting is a matter of public record.