*******ROUGH DRAFT ONLY******* 1 2 CASE NO: 19STCV27561 3 CREST V. PADILLA CASE NAME: LOS ANGELES, CALIFORNIA WEDNESDAY, JANUARY 12, 2022 4 5 department 38 HON. MAUREEN DUFFY-LEWIS, JUDGE 6 SANDRA GUERRA, CSR 10977 REPORTER: 7 APPEARANCES: (AS HERETOFORE NOTED.) AFTERNOON SESSION 8 TIME: 9 10 (THE FOLLOWING PROCEEDINGS WERE HELD IN 11 OPEN COURT.) 12 13 THE COURT: WE'RE BACK ON THE RECORD IN THE 14 15 MATTER THAT WE'RE CURRENTLY IN TRIAL ON. I'LL NOTE FOR 16 THE RECORD THAT ALL COUNSEL ARE PRESENT IN THE COURTROOM. 17 COUNSEL FOR THE DEFENSE. 18 19 MR. SEFERIAN: YOUR HONOR, THE PLAINTIFFS 20 REQUESTED TO TAKE A REBUTTAL WITNESS OUT OF ORDER, AND 21 WE AGREED TO THAT REQUEST, SO THE PLAINTIFF WILL BE 22 CALLING THE WITNESS. 23 THE COURT: ALL RIGHT. AS LONG AS THERE'S NO 24 PROBLEM WITH THAT. 25 MR. STICHT: PROFESSOR KLICK. THE PLAINTIFFS 26 CALL PROFESSOR KLICK. 27 THE COURT: ALL RIGHT. PLEASE COME UP TO THE 28 WITNESS STAND, PROFESSOR KLICK, TO BE SWORN.

1 THE CLERK: PLEASE RAISE YOUR RIGHT HAND. 2 3 JONATHAN KLICK, CALLED BY THE PLAINTIFF AFTER HAVING BEEN DULY SWORN TO 4 5 STATE THE TRUTH, THE WHOLE TRUTH AND NOTHING BUT THE TRUTH, TESTIFIED UNDER OATH AS FOLLOWS: 6 7 THE CLERK: PLEASE STATE AND SPELL YOUR NAME 8 9 FOR THE RECORD. 10 THE WITNESS: JONATHAN, J-O-N-A-T-H-A-N, LAST 11 NAME KLICK, K-L-I-C-K. 12 13 DIRECT EXAMINATION 14 15 BY MR. STICHT: 16 Q. GOOD AFTERNOON, PROFESSOR KLICK. THANK YOU FOR COMING TO LOS ANGELES FOR THIS TRIAL. 17 18 I WOULD ASK THE COURT TO INQUIRE IF YOU PREFER BEING WITH YOUR MASK OR WITHOUT YOUR MASK AND WHETHER 19 20 SHE WOULD PERMIT BOTH. 21 A. I WOULD PROBABLY PREFER WITHOUT. 22 THE COURT: WHATEVER YOU PREFER WOULD WORK. 23 AND FOR THE RECORD, EVERYONE ELSE HAS THEIR MASK ON. 24 THEY ARE IN COMPLIANCE, AND HE IS ENCLOSED IN A PLASTIC 25 SHEETED AREA AROUND THE WITNESS BOX. 26 COUNSEL, YOU MAY INQUIRE. 27 MR. STICHT: AUDIO STARTS YOU CAN TAKE THE 28 BASE AND THE MICROPHONE AND MOVE IT A LITTLE CLOSER TO

1	YOU AND MAKE SURE YOU'VE GOT GOOD INTAKE.
2	THE WITNESS: SURE.
3	BY MR. STICHT:
4	Q. THANK YOU.
5	PROFESSOR KLICK, WOULD YOU PLEASE JUST GIVE US
6	A SUMMARY OF YOUR EDUCATIONAL AND PROFESSIONAL
7	EXPERIENCE/BACKGROUND, AND I'LL ASK YOU A COUPLE OF
8	DETAILS?
9	A. SURE. I'M CURRENTLY THE CHARLES A. HEIMBOLD
10	JR. PROFESSOR OF LAW AT THE UNIVERSITY OF PENNSYLVANIA.
11	AND I'M ALSO THE ERASMUS CHAIR OF EMPIRICAL
12	LEGAL STUDIES AT ERASMUS UNIVERSITY IN ROTTERDAM, IN THE
13	NETHERLANDS. I'VE BEEN A LAW PROFESSOR SINCE 2004.
14	PREVIOUS TO PREVIOUS UNIVERSITY OF
15	PENNSYLVANIA, I WAS AT THE FLORIDA STATE UNIVERSITY IN
16	THE LAW SCHOOL. I WAS THE JEFFREY A. STOOPS PROFESSOR
17	OF LAW AT FLORIDA STATE.
18	IN TERMS OF MY EDUCATION, I HAVE A PH.D. IN
19	ECONOMICS AND I ALSO HAVE A LAW DEGREE.
20	THE COURT: FROM?
21	THE WITNESS: FROM GEORGE MASON UNIVERSITY.
22	THE COURT: THANK YOU.
23	BY MR. STICHT:
24	Q. AND IF YOU COULD SLOW DOWN JUST A BIT FOR THE
25	COURT REPORTER TO BE COMFORTABLE WITH ALL OF US.
26	SOMETIMES I GO TOO SLOW.
27	IF YOU WOULD NOT MIND TAKING A LOOK AT
28	EXHIBIT 39, WHICH WE'LL HELP YOU LOCATE.

1	
1	DO YOU RECOGNIZE THAT DOCUMENT, SIR?
2	A. I DO.
3	Q. AND WHAT IS IT?
4	A. THAT'S MY CV.
5	Q. OKAY. IS THAT YOUR MOST RECENT CV?
6	A. IT'S THE MOST RECENT CV THAT I PUT TOGETHER,
7	YES.
8	Q. AND WAS THERE AN ADDITION TO THIS CV SINCE THIS
9	CV WAS ACTUALLY SUBMITTED TO US?
10	A. YES, I WAS NAMED THE CHARLES A. HEIMBOLD
11	PROFESSOR OF LAW AT THE UNIVERSITY OF PENNSYLVANIA IN
12	DECEMBER.
13	Q. AND JUST FOR THE RECORD, WHAT'S THE DIFFERENCE
14	OR SIGNIFICANCE OF THAT CHANGE?
15	A. WELL, I PREVIOUSLY SINCE 2008 HAVE BEEN A
16	FULL PROFESSOR WITH TENURE AT THE UNIVERSITY OF
17	PENNSYLVANIA, AND THEN IN DECEMBER, I WAS AWARDED AN
18	ENDOWED CHAIR. IT'S JUST AN HONORIFIC AT THE UNIVERSITY
19	OF PENNSYLVANIA.
20	Q. BOTH IN THE LAW SCHOOL?
21	A. YES, IN THE LAW SCHOOL.
22	Q. NOW, YOU MENTIONED YOUR PH.D., WHICH IS THE
23	SECOND ITEM ON YOUR EDUCATION THERE, AT GEORGE MASON
24	UNIVERSITY.
25	WAS THAT AT THE CENTER FOR LAW AND ECONOMICS?
26	A. NO, IT WAS JUST IN THE ECONOMICS DEPARTMENT AT
27	GEORGE MASON.
28	Q. NOW, IN TERMS OF YOUR PROFESSIONAL EXPERIENCE

1	HERE AS A VISITING PROFESSOR OF LAW FROM FALL OF 2007,
2	YOU SAY YOU BECAME A FULL PROFESSOR IN 2008?
3	A. CORRECT.
4	Q. AND THEN WHAT'S THE ROTTERDAM ERASMUS
5	UNIVERSITY ENTRY AFTER THAT?
6	A. I WAS NAMED TO A EUROPEAN COMMISSION-FUNDED
7	CHAIR AT THE ERASMUS UNIVERSITY, WHICH IS IN ROTTERDAM
8	IN THE NETHERLANDS. I'M PART OF BOTH THE EUROPEAN
9	DOCTORATE IN LAW AND ECONOMICS PROGRAM AND THE EUROPEAN
10	MASTER'S IN LAW AND ECONOMICS PROGRAM, WHICH IS HOUSED
11	AT HALF A DOZEN EUROPEAN UNIVERSITIES, BUT MY CHAIR IS
12	HOUSED AT ERASMUS.
13	Q. NOW, YOU REALLY DIDN'T MENTION IT, BUT YOU ALSO
14	HAVE A BACHELOR'S OF SCIENCE IN ECONOMICS, RIGHT?
15	A. CORRECT.
16	Q. FROM VILLANOVA?
17	A. CORRECT.
18	Q. AND YOU ALSO HAVE ON YOUR CV A VISITING
19	PROFESSOR IN THE FALL OF 2013 YALE LAW SCHOOL?
20	A. THAT'S CORRECT. YES.
21	Q. IN YOUR PROFESSIONAL EXPERIENCE AT THE
22	UNIVERSITIES OF PENN AND ROTTERDAM AND YALE, DID YOU
23	ENCOUNTER CORPORATE GOVERNANCE?
24	A. SO I'VE TAUGHT CORPORATE LAW AT UNIVERSITY OF
25	PENNSYLVANIA. AT YALE I DID NOT TEACH CORPORATE LAW OR
26	CORPORATE GOVERNANCE, BUT I HAD TAUGHT CORPORATE FINANCE
27	IN BETWEEN THERE AT COLUMBIA UNIVERSITY AT THE LAW
28	SCHOOL.

1	I'VE ALSO AT FLORIDA STATE UNIVERSITY TAUGHT A
2	CLASS IN CORPORATE GOVERNANCE, A CLASS IN CORPORATE
3	FINANCE, AND IRREGULARLY TAUGHT THE CORPORATE LAW CLASS
4	WHILE I WAS THERE.
5	Q. AND WHEN WERE YOU IN FLORIDA?
6	A. WHERE WAS I? I'M SORRY.
7	Q. NO, I'M SORRY. WHEN.
8	A. I WAS IN FLORIDA STATE UNIVERSITY FROM 2004 TO
9	THE END OF 2007.
10	Q. AND YOUR COLUMBIA LAW SCHOOL ENTRY IS ON YOUR
11	PROFESSIONAL EXPERIENCE AND SO IS YOUR FLORIDA STATE
12	UNIVERSITY, CORRECT?
13	A. THAT'S CORRECT.
14	Q. IN TERMS OF YOUR TEACHING EXPERIENCE, HOW WOULD
15	YOU SUMMARIZE IT?
16	A. THERE ARE ACTUALLY TWO STRAINS OF MY TEACHING
17	EXPERIENCE. ONE STRAIN, I TEACH PURE LAW CLASSES, SO
18	TORTS, FOR EXAMPLE, I TEACH REGULARLY AT THE UNIVERSITY
19	OF PENNSYLVANIA, BUT THE OTHER STRAIN IS I TEACH
20	EMPIRICAL METHODS IN VARIOUS FLAVORS.
21	SO I TEACH STATISTICS FOR LAWYERS. I TEACH
22	SCIENTIFIC AND EXPERT EVIDENCE, WHICH HAS AN EMPIRICAL
23	COMPONENT TO IT. I'VE TAUGHT REGULARLY IN THE ERASMUS
24	PROGRAM CLASSES IN CAUSAL INFERENCE USING ECONOMETRIC
25	METHODS AND THINGS OF THAT NATURE.
26	Q. COULD YOU EXPAND JUST A LITTLE BIT ON WHAT YOU
27	MEAN BY EMPIRICAL TEACHING?
28	A. SURE. SO IN SOCIAL SCIENCE OR EVEN IN LAW, WE

1 MIGHT DISTINGUISH THEORETICAL SCHOLARSHIP AND 2 THEORETICAL INSTRUCTION, WHICH WOULD BE INVOLVED COMING 3 UP WITH HYPOTHESES OR COMING UP WITH LEGAL SPECULATION OR ECONOMIC SPECULATION. 4 5 THE EMPIRICAL SIDE OF THINGS WOULD BE FIGURING 6 OUT HOW TO TAKE THOSE HYPOTHESES AND TEST THEM AGAINST 7 REAL WORLD DATA. AND SO IN TERMS OF TEACHING EMPIRICAL METHODS, 8 9 I BROADLY TEACH HOW TO ENGAGE IN VARIOUS FORMS OF DATA 10 COLLECTION, DATA ANALYSIS, AND HOW TO CONNECT BOTH 11 LEGAL, ECONOMIC AND SOCIAL SCIENCE CONCLUSIONS TO THOSE 12 EMPIRICAL INVESTIGATIONS. Q. AND SO ON YOUR CV, UNDER TEACHING EXPERIENCE 13 YOU HAVE CORPORATE FINANCE AT FLORIDA STATE AND 14 15 COLUMBIA, AS YOU MENTIONED. YOU ALSO MENTIONED THE 16 SUBJECT OF ECONOMETRICS AT THE GRADUATE LEVEL IN CANTERBURY UNDER TEACHING EXPERIENCE. 17 18 CAN YOU EXPAND ON THAT JUST A BIT? 19 A. SURE. I WAS AWARDED THE ERSKINE VISITORSHIP 20 FROM THE UNIVERSITY OF CANTERBURY, WHICH IS IN CHRISTCHURCH, NEW ZEALAND, AND I TAUGHT IN THE ECONOMICS 21 22 AND FINANCE DEPARTMENT THERE, THEIR GRADUATE COURSE IN 23 BASIC ECONOMETRICS. 24 AND AT THE UNDERGRADUATE LEVEL AT GEORGE MASON, Ο. 25 YOU ALSO TAUGHT ECONOMETRICS? 26 A. YES, BASIC ECONOMETRICS. 27 Q. AND FOR UNIVERSITY OF PENN, FLORIDA STATE AND 28 VILLANOVA, YOU ALSO TAUGHT IN LAW AND ECONOMICS?

A. I HAVE. I'VE TAUGHT BOTH A THEORETICAL LAW AND
 ECONOMICS CLASS AS WELL AS EMPIRICAL LAW AND ECONOMICS
 AT ALL OF THOSE PLACES, AS WELL AS SPECIALIZED CLASSES
 ON LAW AND ECONOMICS.

5 FOR EXAMPLE, I CURRENTLY, AND HAVE FOR THE LAST 6 FIVE OR SIX YEARS, I BELIEVE, TAUGHT THE REQUIRED COURSE 7 IN OUR JD/MBA PROGRAM. SO PENN LAW HAS A PROGRAM WHERE IT HAS STUDENTS WHO ARE BOTH GETTING A LAW DEGREE AT OUR 8 9 LAW SCHOOL BUT ARE ALSO GETTING AN MBA AT THE WHARTON 10 SCHOOL, AND ALL OF THOSE STUDENTS ARE REQUIRED TO TAKE A 11 CLASS FROM ME AT THE END OF THEIR FIRST YEAR OF 12 INSTRUCTION IN THE LAW AND ECONOMICS OF THE FIRM.

AND SO WE DO A LOT OF WORK ON HOW BOTH
THEORETICAL AND EMPIRICAL LAW AND ECONOMICS INFORM THE
MANAGEMENT ORGANIZATION, PERFORMANCE OF FIRMS.

16 Q. HOW WOULD YOU DEFINE ECONOMETRICS FOR SOMEONE17 WHO ISN'T FAMILIAR WITH THAT TERM?

18 A. SURE. ECONOMETRICS IS THE USE OF STATISTICAL
19 METHODS TO STUDY BOTH ECONOMIC AND SOCIAL SCIENCE
20 PHENOMENA USING ECONOMIC AND SOCIAL SCIENCE DATA.

21 SO IT REALLY IS APPLIED STATISTICS, WHERE THE 22 APPLICATIONS ARE IN ECONOMICS NARROWLY OR SOCIAL SCIENCE 23 MORE BROADLY.

Q. WHAT'S A GOOD EXAMPLE, JUST TO GET MORE OF A HANDLE ON THAT SUBJECT?

A. SURE.

27 SO I'M REALLY A METHODOLOGY PERSON, SO I WORK 28 ACROSS SUBSTANTIVE AREAS. SO I'VE USED THESE METHODS IN THE CORPORATE SETTING, IN HEALTH SETTINGS AND CRIME
 SETTING, ANTITRUST SETTING.

BUT TO GIVE YOU AN EXAMPLE, I HAVE DONE WORK ON 3 4 LOOKING AT WHAT IS THE EFFECT OF POLICE ON CRIME, SO WE 5 HAVE GENERAL SOCIAL SCIENCE THEORIES THAT SUGGEST THAT POLICE GENERATE DETERRENTS FOR CRIME, BUT IT'S DIFFICULT 6 7 STATISTICALLY TO SORT OUT CAUSALITY IN THOSE SITUATIONS, BECAUSE YOU CAN'T JUST LOOK AT PLACES THAT HAVE MORE 8 9 POLICE AND SEE IF THEY HAVE LESS CRIME BECAUSE IN GENERAL PLACES THAT HAVE A LOT OF CRIME HIRE A LOT OF 10 11 POLICE.

12 SO YOU NEED TO USE STATISTICAL METHODS. AND I 13 CAN GO IN MORE DETAIL, IF YOU'RE INTERESTED, TO SORT OF 14 TEASE OUT THOSE CAUSAL RELATIONSHIPS.

15 TO GIVE ONE EXAMPLE OF A STUDY, WE USE 16 SOMETHING THAT'S KNOWN AS REGRESSION DISCONTINUITY DESIGN. LOOKING AT THE UNIVERSITY OF PENNSYLVANIA, 17 WHERE WE HAVE OUR OWN POLICE FORCE, AND IT'S MUCH LARGER 18 19 THAN THE PHILADELPHIA POLICE FORCE PROPORTIONATELY, BUT 20 THE UNIVERSITY OF PENNSYLVANIA POLICE FORCE NEEDS TO STOP AT THE HISTORICAL PENN BOUNDARY, SO WHAT WE DID 21 22 EFFECTIVELY -- I CAN ADD MORE NUANCE IF PEOPLE ARE 23 INTERESTED -- BUT WHAT WE DID EFFECTIVELY IS COMPARE 24 CRIME ESSENTIALLY ON ONE SIDE OF THE LINE TO THE OTHER 25 SIDE OF THE LINE AS THE POLICE FORCE SHIFTED. 26 Ο. AND THAT'S YOUR FIELD OF EXPERTISE? 27 Α. ECONOMETRIC METHODS AND CAUSAL INFERENCE I 28 WOULD SAY IS MY FIELD OF EXPERTISE, AND I'VE APPLIED IT

1 IN MANY AREAS, SO CRIME BEING ONE. 2 Q. CRIME IS ONE? 3 Α. YES. 4 YOU ALSO MENTIONED SOCIAL SCIENCES. THAT WOULD Ο. 5 INCLUDE WHAT AREAS? A. SO I'VE PUBLISHED IN SOME OF THE TOP PSYCHOLOGY 6 7 JOURNALS, TAKING STATISTICAL AND ECONOMETRIC METHODS TO DATA AND PSYCHOLOGY. SO THAT'S OBVIOUSLY BROADER THAN 8 9 ECONOMICS, ALTHOUGH IT HAS ECONOMICS IMPLICATIONS. 10 OBVIOUSLY LAW QUESTIONS. SO I'VE DONE A DECENT 11 AMOUNT OF WORK LOOKING AT, SAY, WHAT IS THE EFFECT OF 12 MEDICAL MALPRACTICE LAW AND MEDICAL MALPRACTICE REFORM ON THE BEHAVIOR OF DOCTORS. 13 SO THAT'S NOT -- BROADLY SPEAKING, IT'S 14 15 ECONOMICS, BUT IT'S REALLY MORE OF EITHER A HEALTH 16 POLICY OR A LEGAL POLICY QUESTION THAT WE HAVE APPLIED THESE STATISTICAL METHODS TO. 17 18 O. SO AS YOU LOOK AT YOUR REFEREED PUBLICATIONS IN 19 YOUR CV, ARE THERE ANY THAT YOU WOULD LIKE TO HIGHLIGHT 20 IN THAT CONTEXT? SO THE TOP ONE ON THERE IS A VERY NICE PAPER. 21 Α. 22 THAT'S ON CRIME, AND THAT'S ACTUALLY PUBLISHED IN THE 23 TOP CRIMINOLOGY JOURNAL IN THE WORLD. AND WE USED 24 METHODS TO FIGURE OUT WHAT IS THE EFFECT OF ALCOHOL ON 25 CRIME. 26 IF WE GO A BIT FURTHER DOWN, THERE'S ANOTHER 27 CRIME PAPER THERE DOING THE SAME THING LOOKING AT THE 28 EFFECT OF SECURITY GUARDS ON CRIME.

1 Ο. WHAT'S THE TITLE OF THAT ONE, SIR? 2 Α. "THE INEFFECTIVENESS OF OBSERVE AND REPORT 3 CONTROLS ON CRIME." 4 THE NEXT ONE DOWN IS ALSO IN THIS FIELD, BUT IN 5 THIS CASE IT'S LOOKING AT THE EFFECT OF TORT LAW ON CRIME. THAT'S "DETERRENTS AND LIABILITY FOR INTENTIONAL 6 7 TORTS." THE "MEDICARE SECONDARY PAYER AND SETTLEMENT 8 9 DELAY" IS A PAPER ON ESSENTIALLY LEGAL PROCEDURE AND 10 LOOKING AT WHAT IS THE EFFECT OF THE FEDERAL MEDICARE 11 SECONDARY PAYER LAW ON THE SETTLEMENT DYNAMICS IN AUTO 12 ACCIDENT TORT CASES. AND SO WE CAN GO DOWN. MOST -- THERE ARE A 13 HANDFUL OF THESE REFEREED PUBLICATIONS THAT ARE PURE 14 15 THEORY ARTICLES, BUT MOST WOULD FALL IN THE CATEGORY OF EMPIRICAL ANALYSIS. 16 Q. AND INCLUDE YOUR ECONOMETRICS ANALYSIS? 17 18 Α. THAT'S CORRECT. THAT'S CORRECT. 19 Q. LET'S GO TO THE NEXT PAGE OF YOUR LAW REVIEW 20 PUBLICATIONS. ARE THERE ANY IN PARTICULAR YOU'D LIKE TO HIGHLIGHT THAT RELATE TO YOUR EXPERTISE? 21 22 A. SURE. SO THE THIRD ONE DOWN, "THE LOGIC AND 23 LIMITS OF EVENTS STUDIES IN SECURITIES FRAUD 24 LITIGATION," IS A METHODOLOGY PIECE. AND SO WE'RE USING 25 THAT ARTICLE TO DO TWO THINGS REALLY. 26 ONE IS TO TEACH PRACTITIONERS AND JUDGES WHAT 27 STATISTICAL EVENT STUDIES, WHICH IS AN ECONOMETRICS TOOL 28 THAT'S USED IN CORPORATE FINANCE, WHAT STATISTICAL EVENT

STUDIES CAN BE USEFUL IN SECURITIES FRAUD CASES. AND
 THEN WE APPLY IT TO THE FEDERAL HALLIBURTON CASES, WHICH
 REQUIRE GREATER USE OF EVENT STUDIES IN U.S. SECURITY
 FRAUD LITIGATION.

5 Q. WITH ALL DUE RESPECT, SIR, COULD YOU JUST MAKE6 THAT A LITTLE MORE PEOPLE FRIENDLY?

A. SURE. SO GOING BACK IN SECURITIES FRAUD A LONG
TIME, IN A FAMOUS CASE CALLED BASIC V. LEVINSON, THE
FRAUD ON THE MARKET THEORY FOR ALLOWING ESSENTIALLY
CLASS CERTIFICATION AND SECURITY FRAUD CASES WAS
ENDORSED BY THE COURT.

12 AND, YOU KNOW, ESSENTIALLY WHAT THE FRAUD ON 13 THE MARKET THEORY SAYS IS THAT UNLIKE NORMAL CLASS 14 ACTIONS, WHERE YOU WOULD HAVE TO ENSURE THAT EVERY 15 MEMBER OF THE CLASS -- THE COMMONALITY REQUIREMENT, YOU 16 WOULD ESSENTIALLY HAVE TO PROVE THAT EVERYONE IN THE 17 CLASS HAD HEARD, SAY, THE FRAUDULENT STATEMENT AND THEN 18 ACTED UPON IT IN A SIMILAR WAY.

19 WHAT FRAUD ON THE MARKET ALLOWED IS THE IDEA 20 THAT AS LONG AS WE PRESUME THAT PEOPLE RELY ON THE 21 INTEGRITY OF STOCK MARKET PRICES WHEN MAKING THEIR 22 TRADING DECISIONS, THAT IS SUFFICIENT FOR THE 23 COMMONALITY REQUIREMENT IN A SECURITIES FRAUD CLASS 24 ACTION.

AND SO THEN THE COURT DID START TO WORRY ABOUT, WELL, HOW DO WE KNOW THAT STOCK MARKET PRICES ACTUALLY REFLECT FRAUDULENT STATEMENTS AND THINGS LIKE THAT. SO IF A CEO MAKES A FRAUDULENT STATEMENT, THE STOCK PRICE GOES UP, HOW DO WE KNOW IT WAS BECAUSE OF THE FRAUDULENT
 STATEMENT VERSUS JUST NORMAL VARIATION IN THE STOCK
 MARKET.

4 AND THE STANDARD STATISTICAL TOOL THAT'S USED 5 IN ECONOMICS AND FINANCE IS KNOWN AS AN EVENTS STUDY. AND ESSENTIALLY IN MUCH OF THE LITIGATION -- FEDERAL 6 7 LITIGATION THAT OCCURRED SUBSEQUENTLY, THE COURTS ADOPTED THE VIEW THAT EVENT STUDIES WERE AT LEAST 8 9 SUFFICIENT TO DEMONSTRATE THESE KINDS OF ISSUES. AND IN SOME CASES, SAY, THE DURHAM CASE OR THE HALLIBURTON 10 11 CASE, THE COURT CAME VERY CLOSE TO SAYING THAT THEY ARE 12 REQUIRED.

13 AND SO ESSENTIALLY WHAT WE DO THERE, AS 14 INDICATED BY YOUR QUESTION -- LAWYERS AND JUDGES MIGHT 15 NOT BE SO COMFORTABLE WITH THESE METHODS, AND SO THAT 16 ARTICLE IS AN ATTEMPT TO PROVIDE ALMOST A COOKBOOK AND AN INTUITION FOR (A), HOW EVENT STUDIES ARE DONE, WHAT 17 18 THEY CAN DO, WHAT THEY CAN'T DO, AND SOME PARTICULAR 19 NUANCES THAT MIGHT BE RELEVANT IN VARIOUS SECURITIES 20 FRAUDS SETTINGS.

Q. BUT IN THE ECONOMETRICS CONTEXT, OR THE 21 EMPIRICAL RESEARCH THAT YOU DO, OR ANALYSIS, THAT SIMPLY 22 23 MEANS THAT YOU ARE TRYING TO RULE IN OR RULE OUT WHAT? 24 SURE. SO IN THE EVENT STUDY CONTEXT IN A Α. 25 SECURITIES FRAUD CASE, YOU ARE TRYING TO RULE OUT, FOR 26 EXAMPLE, THAT IT WAS JUST NORMAL BACKGROUND MARKET 27 MOVEMENTS THAT WERE OCCURRING, CHANGING THE STOCK PRICE. 28 YOU'RE TRYING TO RULE THAT OUT RELATIVE TO THE LEGALLY

1	RELEVANT HYPOTHESIS, WHICH WOULD BE THAT THE INSIDER
2	STATEMENT, FRAUDULENT STATEMENT ACTUALLY MOVED STOCK
3	PRICES.
4	Q. HAD THE EFFECT?
5	A. CORRECT. AND SO BECAUSE AN ELEMENT OF THE
6	FRAUD CASE WOULD INVOLVE CAUSALITY, CAUSAL INFERENCE IS
7	VERY IMPORTANT IN THAT SETTING. SO WE SPEND A LOT OF
8	TIME IN THAT ARTICLE TALKING ABOUT WHEN CAUSAL
9	INFERENCES CAN BE MADE.
10	Q. ON THE CAUSATION ELEMENT?
11	A. CORRECT.
12	Q. AS YOU LOOK AT THE LAW REVIEW PUBLICATIONS, IS
13	THERE ANYTHING ELSE THAT YOU THINK WOULD BE PARTICULARLY
14	RELEVANT TO YOUR EXPERTISE FOR THIS CASE?
15	A. SO ON THE NEXT ON THE FOLLOWING PAGE, THE
16	ARTICLE IN THE "COLUMBIA LAW REVIEW" CALLED "AGENCY
17	COSTS, CHARITABLE TRUSTS, AND CORPORATE CONTROL:
18	EVIDENCE FROM HERSHEY'S KISS-OFF," WE STUDY A UNIQUE
19	NATURAL EXPERIMENT IN THE CORPORATE CONTROL CONTEXT TO
20	LOOK AT WHAT THE EMPIRICAL EVIDENCE SUGGESTS IS THE
21	VALUE OF HAVING A FIRM SUBJECTED TO THE MARKET FOR
22	CORPORATE CONTROL.
23	Q. I'LL STOP YOU FOR A MOMENT AND ASK YOU WHAT YOU
24	MEAN BY EMPIRICAL EVIDENCE.
25	A. SURE. SO AGAIN, AS SUGGESTED EARLIER, IF YOU
26	HAVE A LEGAL OR SOCIAL SCIENCE HYPOTHESIS SO IN THIS
27	CASE, FIRMS SUBJECTED TO THE MARKET FOR CORPORATE
28	CONTROL WILL EXHIBIT LOWER AGENCY COSTS, FOR EXAMPLE.

THAT'S A LEGAL AND CORPORATE FINANCE HYPOTHESIS.

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AND SO TO DO THE EMPIRICAL TESTING, WE HAVE A
NATURAL EXPERIMENT AND WE GO AND WE LOOK IF THE DATA
SUGGESTS OUTCOMES OF THE NATURAL EXPERIMENT THAT ARE
CONSISTENT WITH THAT CORPORATE CONTROL HYPOTHESIS.

Q. AND BY NATURAL EXPERIMENT, WHAT GENERALLY DO7 YOU MEAN?

A. SO NATURAL EXPERIMENT IS SORT OF A PLAY OFF OF THE MORE FORMAL TYPE OF EXPERIMENT. SO IF WE -- IF WE THINK OF A DRUG TEST OR A TEST OF A COVID VACCINE, WE USE -- GENERICALLY, SOCIAL SCIENTISTS, SCIENTISTS, USE EXPERIMENTAL METHODS TO ATTEMPT TO ISOLATE THE CAUSAL EFFECT OF THE DRUG BEING TESTED OR THE VACCINE BEING TESTED.

15 AND IN THAT SETTING, WHAT DO WE DO? WE 16 RANDOMIZE. WE HAVE SOME PEOPLE GET THE DRUG AND SOME PEOPLE NOT GET THE DRUG. AND SO WE HAVE A -- WE BREAK 17 OUR SAMPLE DOWN INTO A TREATMENT GROUP AND A CONTROL 18 19 GROUP, AND WE GO AND WE SEE WHAT CHANGES DO WE OBSERVE 20 IN THE FOLKS WHO HAVE GOT THE TREATMENT AS COMPARED TO WHAT CHANGES WE OBSERVE IN THE SAME TIME PERIOD AMONG 21 22 THE FOLKS WHO DIDN'T GET THE TREATMENT, THE FOLKS WHO 23 GET THE PLACEBO THE CONTROL GROUP.

IN HARD SCIENCE, AND TO SOME EXTENT IN SOCIAL
SCIENCE, THESE TYPE OF RANDOMIZED EXPERIMENTS ARE PRIZED
FOR THEIR EFFICACY IN FIGURING OUT CAUSAL RELATIONSHIPS.
IN MANY SOCIAL SCIENCES CONTEXTS, HOWEVER, AND CERTAINLY
IN LAW AND POLICY CONTEXTS, IT'S JUST NOT FEASIBLE FOR A

RESEARCHER TO ENGAGE IN THAT EXPERIMENTAL MANIPULATION,
 RIGHT.

SO WE COULDN'T RANDOMIZE CORPORATE GOVERNANCE
MECHANISMS ACROSS FIRMS AND SEE HOW DOES BEHAVIOR
CHANGE, HOW DOES PERFORMANCE CHANGE THAT. IT JUST
WOULDN'T BE FEASIBLE.

7 SO WHAT WE DO, AND THIS HAS BEEN SORT OF THE 8 STANDARD IN SOCIAL SCIENCE SINCE AT LEAST THE LATE 9 1990'S, IS TO ATTEMPT TO FIND SERENDIPITOUS NATURAL 10 EXPERIMENTS, ALMOST A QUASI-RANDOMIZATION, SOME SHOCK 11 THAT OCCURS IN THE OUTSIDE WORLD THAT AFFECTS SOME 12 PEOPLE OR SOME FIRMS, OR MORE GENERALLY SOME ENTITIES, BUT DOESN'T AFFECT OTHERS, AND THEN USING SORT OF THE 13 14 EXPERIMENTAL INTUITION COMPARE OUTCOMES, WHATEVER 15 OUTCOME IS THE OUTCOME OF INTEREST, AMONG THE ENTITIES 16 THAT GET SORT OF HIT WITH THE TREATMENT THROUGH THE NATURAL EXPERIMENT, THROUGH THE SHOCK -- COMPARE THOSE 17 18 OUTCOMES AND THOSE CHANGES IN THOSE OUTCOMES WITH THE 19 CHANGES IN THE OUTCOMES FOR THE OTHER ENTITIES THAT ARE 20 OTHERWISE SIMILAR BUT FOR RANDOM REASONS WERE NOT SUBJECTED TO THE SHOCK. 21

Q. I'D LIKE TO COME BACK TO THAT IN A MOMENT. BUT
THE TITLE OF THAT PARTICULAR LAW REVIEW WAS "AGENCY
COSTS, CHARITABLE TRUSTS AND CORPORATE CONTROL:
EVIDENCE FROM HERSHEY'S KISS-OFFS."

26 A. YES.

27 Q. HOW DO YOU PUT THE TITLE TO WHAT YOU JUST TOLD28 US?

SO THE HERSHEY COMPANY, THE CHOCOLATE MAKER 1 Α. 2 FROM PENNSYLVANIA, HAS A VERY INTERESTING CORPORATE 3 STRUCTURE. MILTON HERSHEY STARTED THE COMPANY A LONG 4 TIME AGO. AS MANY ENTREPRENEURS DO, HE WANTED TO RAISE 5 CAPITAL BUT WANTED TO MAINTAIN CONTROL, SO HE CREATED DUAL CLASS SHARES. 6 7 AND SO IT WORKED OUT THAT ESSENTIALLY MILTON 8 HERSHEY CONTROLLED SOMETHING LIKE 90 PERCENT OF THE 9 VOTES EVEN THOUGH HE ONLY HAD ABOUT A THIRD OF THE 10 EQUITY INTEREST. 11 THAT HAS SINCE GONE ON. HE'S LONG DEAD, BUT HE BEQUEATHED HIS CONTROL OF THE COMPANY TO THE MILTON 12 HERSHEY TRUST. 13 AND SO THE INTERESTING THING IN TERMS OF THE 14 15 MARKET FOR CORPORATE CONTROL, IN CORPORATE FINANCE WE 16 GENERALLY THINK THAT THIS MARKET FOR CORPORATE CONTROL, ESSENTIALLY THE TAKEOVER MARKET, HAS A DISCIPLINING 17 18 EFFECT ON AGENCY COSTS IN FIRMS, RIGHT. 19 Q. AGENCY COSTS MEANING WHAT? 20 WE TEND TO DEFINE AGENCY COSTS AS DIFFERENT Α. INTERESTS BETWEEN OWNERS AND MANAGERS. 21 22 SO, YOU KNOW, IF THERE WERE AN OWNER MAKING 23 DECISIONS, THE OWNER WOULD MAKE ESSENTIALLY EFFICIENT 24 DECISIONS, AT LEAST THE WAY ECONOMISTS SEE IT, BECAUSE 25 THE OWNER WOULD HAVE EVERY INCENTIVE TO PUT OUT ENOUGH EFFORT SUCH THAT IT JUSTIFIES THE RETURN, BUT MANAGERS 26 27 WHOSE OWN INCOME IS NOT COMPLETELY IN SYNC WITH THE 28 RETURNS THAT THEY GENERATE HAVE AN INCENTIVE TO

1 ESSENTIALLY SHIRK. THAT'S THE STANDARD ECONOMIC THEORY. 2 I PAY ALL THE COSTS OF MY OWN EFFORTS, BUT I DON'T GET ALL MY OWN BENEFITS. SO THAT'S WHAT 3 ECONOMISTS REFER TO AS AGENCY COSTS. 4 5 SO BACK TO THE MARKET FOR CORPORATE CONTROL, IN 6 CORPORATE FINANCE, GOING BACK TO AN OLD ARTICLE BY A GUY 7 NAMED HENRY MANNE, M-A-N-N-E, HAS THIS IDEA THAT 8 ESSENTIALLY SAID IF MANAGERS KNOW THAT THERE IS A VIGOROUS TAKEOVER MARKET OUT THERE, THEY WILL BE 9 DISCIPLINED OR CONSTRAINED, RIGHT. 10 11 IF THEY ESSENTIALLY SHIRK, OR SHIRK TOO MUCH, 12 ESSENTIALLY IT WILL LOWER THE VALUE OF THE FIRM, AND THAT WILL CREATE AN ATTRACTIVE TARGET FOR A TAKEOVER, 13 WHETHER IT BE THROUGH, YOU KNOW, A PRIVATE EQUITY 14 15 CONCERN, OR WHETHER IT BE THROUGH A COMPETITOR OR WHETHER IT BE THROUGH SOME OTHER TAKEOVER OR ACQUIRER. 16 17 AND BECAUSE THE MANAGERS REALIZE THIS, THE 18 THEORY IS, THE HYPOTHESIS IS THAT THOSE MANAGERS WHEN 19 THEY'RE SUBJECTED TO THIS DISCIPLINE WILL WORK HARDER, 20 WILL NOT SHIRK AS MUCH. SO TO COME BACK TO HERSHEY, THE INTERESTING 21 22 THING IS IT'S EFFECTIVELY IMPOSSIBLE TO TAKE OVER THE 23 HERSHEY COMPANY. YOU COULD BUY UP EVERY SHARE IN THE 24 NEW YORK STOCK EXCHANGE AND STILL NOT CONTROL THE 25 HERSHEY COMPANY BECAUSE ALL THE VOTES ARE HELD BY THIS 26 TRUST, OKAY. 27 SO THE THEORY WOULD IMPLY THAT HERSHEY HAS A 28 LOT OF AGENCY COSTS, THAT ITS MANAGEMENT IS SORT OF

ACTING SUBOPTIMALLY. WHERE IS THE NATURAL EXPERIMENT,
 IT'S REASONABLE TO ASK.

WELL, A NUMBER OF YEARS AGO, IT TURNS OUT THAT 3 THE STATE ATTORNEY GENERAL'S OFFICE IN PENNSYLVANIA --4 5 ATTORNEY GENERAL'S OFFICES ARE THE ONES THAT ARE TASKED WITH POLICING CHARITABLE TRUSTS IN THIS COUNTRY -- THE 6 7 ATTORNEY GENERAL'S OFFICE SORT OF REALIZED -- WHO KNOWS 8 WHY IT TOOK SO LONG, BUT REALIZED THAT IN THE HERSHEY 9 TRUST, WHICH WAS ABOUT A 9 BILLION-DOLLAR TRUST AT THE TIME, ESSENTIALLY 80 PERCENT OF THEIR HOLDINGS WERE ALL 10 11 IN HERSHEY, AND SO THIS VIOLATES THE PRUDENT INVESTOR 12 RULE, WHICH REQUIRES DIVERSIFICATION.

13 SO THE ATTORNEY GENERAL'S OFFICE WHEN IT
14 DISCOVERED THIS WENT AND TOLD THE TRUST YOU ESSENTIALLY
15 HAVE TO SELL YOU'RE CONTROLLING INTEREST. YOU NEED TO
16 DIVERSIFY.

17 THEY DIDN'T DO THIS BECAUSE HERSHEYS
18 PERFORMANCE WAS BAD, WAS GOOD, OR ANYTHING REALLY
19 RELATED TO HERSHEY. IT WAS ESSENTIALLY AS IF RANDOM,
20 THAT THE TIMING OCCURRED THAT THEY JUST, I DON'T KNOW,
21 THEY GOT TO THE H'S IN THE FILE CABINET, SO TO SPEAK.

AND THEY REALIZED THIS AND SO THEY GO AND THEY TELL THE TRUST YOU'VE GOT TO SELL. AND SO RELATIVE TO THE HYPOTHESIS, ONCE THE ANNOUNCEMENT IS MADE THAT HERSHEY HAS TO SELL ITS CONTROLLING INTEREST, YOU GO FROM A WORLD WHERE THE MARKET FOR CORPORATE CONTROL HYPOTHETICALLY DOESN'T WORK, OR DOESN'T APPLY TO HERSHEY, TO A WORLD WHERE ALL OF A SUDDEN IT WILL APPLY.

1 AND SO WE USE THAT AS OUR SHOCK, AS OUR NATURAL 2 EXPERIMENT. AND WE LOOKED AT WHAT HAPPENED TO THE 3 MARKET VALUATION OF HERSHEY WHEN THIS ANNOUNCEMENT WAS 4 MADE. AND A NUMBER OF MONTHS LATER, THE GOVERNOR OF 5 PENNSYLVANIA -- I'M SORRY, THE ATTORNEY GENERAL OF PENNSYLVANIA, WHO WANTED TO RUN FOR GOVERNOR, DECIDED 6 7 MAYBE THIS IS NOT SUCH A GOOD IDEA, AND HE FOUGHT THE 8 SALE. AND EVENTUALLY THE TRUST DROPPED ITS SALE. 9 AND SO WE WERE ABLE TO ESSENTIALLY USE THAT AS 10 ANOTHER EXPERIMENT. SO THEN YOU GO -- AGAIN FROM, IN 11 THIS CASE, THE MARKET FOR CORPORATE CONTROL APPLYING TO 12 IT NOT APPLYING, AND SO THAT'S THE NATURAL EXPERIMENT 13 THERE.

Q. AND SO WE NOT ONLY HAVE HERSHEY CHOCOLATE TODAY
AS A RESULT, BUT YOUR POINT IS MORE GENERALLY THAT
NATURAL EXPERIMENT GIVES YOU THAT CONTROL GROUP OF THE
PLACEBO, SO TO SPEAK?

A. WE CAN GET INTO THAT MORE SYSTEMATICALLY, BUT
YES. JUST TO GIVE A LITTLE BIT OF THE RESULTS THAT WE
FOUND FOR HERSHEY, THE MOMENT "THE WALL STREET JOURNAL"
REPORTED THAT THE TRUST WAS GOING TO HAVE TO SELL, YOU
SAW A 25 PERCENT INCREASE IN HERSHEY STOCK.

AND HERE'S THE INTERESTING THING: THIS WAS AN INCREASE IN THE HERSHEY STOCK THAT'S AVAILABLE TO THE PUBLIC. SO IT'S STOCK THAT WOULD HAVE BEEN WHOLLY UNAFFECTED BY ANY SALE DIRECTLY. ANYBODY WHO WAS GOING TO TAKE OVER HERSHEY WOULDN'T HAVE BEEN BUYING THE PUBLIC SHARES, THEY WOULD HAVE BEEN DEALING DIRECTLY WITH THE TRUST AND BUYING THEIR CLASS B SHARES.

1

2 SO OUR INFERENCE FROM THIS WAS THE PRICE 3 INCREASE WAS THE MARKET NOT SAYING, OH, I'M GOING TO GET 4 RICH BECAUSE SOMEBODY IS GOING TO BUY THIS COMPANY, IT 5 WAS THE MARKET SAYING, WELL, NOW WE'RE GOING TO FROM A 6 SITUATION WERE THERE WAS NO DISCIPLINE ON MANAGEMENT TO 7 A SITUATION WHERE MANAGEMENT WILL BE MORE DISCIPLINED BY 8 THIS MARKET FOR CORPORATE CONTROL.

9 SO THOSE WERE OUR RESULTS. BUT THEN YOU HAVE 10 TO WORRY, JUST LIKE I SAID EARLIER IN THE SECURITIES 11 FRAUD CASE, WHAT IF IT WERE THE CASE THAT, YOU KNOW, IT 12 JUST SO HAPPENS ON THAT DAY THE FDA OR THE CDC SAYS, EAT 13 LOTS OF CHOCOLATE, IT WILL CURE CANCER, RIGHT? THAT 14 WOULD ALSO HAVE INCREASED HERSHEY'S STOCK PRICE.

15 SO WE NEED TO RULE OUT THOSE OTHER 16 POSSIBILITIES. AND WHAT WE DO THERE IS WE RUN THE SAME ANALYSIS ON OTHER CHOCOLATE COMPANIES OR OTHER FOOD 17 CONGLOMERATES OR OTHER COMPANIES THAT ARE OTHERWISE 18 19 SIMILAR TO HERSHEY BUT ARE NOT BEING AFFECTED BY THIS 20 SALE. AND SO THAT WOULD BE THE PLACEBO OR CONTROL GROUP IN THAT ANALYSIS, OTHER COMPANIES THAT ARE LIKE HERSHEY 21 22 BUT ARE NOT SUBJECTED TO THIS PARTICULAR SHOCK.

Q. ANYTHING ELSE IN THE LAW REVIEW PUBLICATIONS
THAT YOU THINK WE SHOULD KNOW ABOUT FOR THIS PARTICULAR
CASE?

A. NO, I DON'T BELIEVE SO.
Q. WHAT ABOUT IN THE BOOK CONTRIBUTIONS SECTION?
A. YES. PROBABLY THE MOST IMPORTANT IS WE HAVE A

1	CHAPTER ON EMPIRICAL LAW AND ECONOMICS IN THE "OXFORD
2	HANDBOOK OF LAW AND ECONOMICS" IN 2017.
3	THE "OXFORD HANDBOOKS" SERIES EVEN MORE
4	BROADLY THAN "LAW AND ECONOMICS," THE "OXFORD HANDBOOKS"
5	SERIES IS WELL KNOWN AND A WELL-KNOWN REFERENCE GUIDE
6	FOR MANY AREAS OF EVEN NOT JUST SOCIAL SCIENCE THEY
7	EVEN HAVE HANDBOOKS IN HUMANITIES AND LAW AND THINGS
8	LIKE THAT.
9	BUT THEY'RE WELL-KNOWN, WELL-RESPECTED
10	REFERENCE GUIDES. AND WE WERE CHOSEN BY THE EDITOR OF
11	THE SERIES TO WRITE THE CHAPTER ON THE USE OF
12	ECONOMETRIC METHODS IN LAW AND ECONOMICS.
13	Q. AND BY WE, YOU MEAN YOU AND JONAH
14	A. JONAH GELBACH.
15	Q. GELBACH, G-E-L-B-A-C-H?
16	A. THAT'S CORRECT.
17	Q. AND THE TITLE OF THAT BOOK WAS EMPIRICAL LAW
18	AND ECONOMICS?
19	A. THAT WAS THE TITLE OF THE CHAPTER. THE BOOK
20	WAS "OXFORD HANDBOOK OF LAW AND ECONOMICS."
21	Q. SORRY, THANK YOU.
22	COMING BACK TO THIS CONCEPT OF ECONOMETRICS,
23	RIGHT, IT COMES BACK TO THIS.
24	AND WHAT ABOUT YOUR PRESENTATIONS. YOU'VE GOT
25	A COUPLE OF PAGES OF PRESENTATIONS IN A VARIETY OF
26	CONTEXTS. DO YOU WANT TO HIGHLIGHT ANY ONE OR TWO IN
27	PARTICULAR?
28	A. JUST FOR RECENCY PURPOSES, A COUPLE OF MONTHS

1	AGO, IN SEPTEMBER 2021, I WAS ASKED TO BE THE KEYNOTE
2	SPEAKER AT THE POLISH ASSOCIATION OF LAW AND ECONOMICS.
3	SO THAT WOULD BE THE NATIONAL LAW AND ECONOMICS
4	ASSOCIATION IN POLAND.
5	Q. I'M SORRY, WHERE WAS THAT ON YOUR
6	A. IT'S THE FIRST PRESENTATION, KEYNOTE ADDRESS,
7	POLISH ASSOCIATION OF LAW AND ECONOMICS.
8	Q. AND IF YOU COULD JUST REFER TO THE BOTTOM RIGHT
9	PAGE AND TELL ME WHAT THE PAGE NUMBER IS.
10	A. SURE. 039005.
11	Q. THANK YOU. PLEASE CONTINUE.
12	A. SO THAT WAS A KEYNOTE WHICH IS YOU KNOW,
13	QUITE FRANKLY, THE KEYNOTES TEND TO BE LESS SUBSTANTIVE
14	THAN OTHER TALKS, BUT THEY'RE MORE HONORIFIC, SO YOUR
15	CHOSEN AS SORT OF A RECOGNITION OF YOUR POSITION IN THE
16	FIELD.
17	BUT, YOU KNOW, THE TALKS THEMSELVES ARE A
18	LITTLE BIT LESS INTERESTING. THE MORE INTERESTING TALKS
19	ARE SOME OF THE MORE UNIVERSITY TALKS, JUST WORKSHOP
20	TALKS WHERE YOU GIVE ESSENTIALLY YOUR RESEARCH IN A
21	DRAFT FORM AND YOU DISCUSS YOUR RESEARCH WITH THE
22	AUDIENCE AND TAKE QUESTIONS AND CRITICISMS AND ATTEMPT
23	TO ADDRESS THEM.
24	IN TERMS OF PRESENTATIONS I'VE PRESENTED MY
25	WORK AT ALL THE TOP LAW SCHOOLS, ESSENTIALLY. MANY
26	ECONOMICS DEPARTMENTS AND THEN IN SUNDRY OTHER PLACES.
27	PRESENTED SOME OF MY WORK AT HARVARD MEDICAL SCHOOL.
28	I'VE PRESENTED MY WORK IN FRONT OF FEDERAL AGENCIES; THE

1	FEDERAL TRADE COMMISSION, FOR EXAMPLE.
2	Q. AND BY YOUR WORK, YOU MEAN MORE SPECIFICALLY
3	A. MANY OF THE PUBLICATIONS THAT YOU SEE
4	PREVIOUSLY ON THE CV.
5	Q. OKAY.
6	A. I GUESS ONE MORE TO HIGHLIGHT THE EXTENT OF THE
7	EMPIRICAL METHODS AND THE INTEREST IN EMPIRICAL METHODS.
8	SO IN ADDITION TO THE GENERAL "OXFORD HANDBOOK," WE WERE
9	ALSO I WAS ALSO ASKED TO WRITE THE EMPIRICAL CHAPTER
10	IN THE "OXFORD HANDBOOK OF FIDUCIARY LAW."
11	Q. AND WHAT PAGE IS THAT ON, SIR?
12	A. 039004. SO FIVE BULLET POINTS DOWN.
13	Q. YES.
14	A. EMPIRICAL ANALYSIS OF FIDUCIARY LAW. SO THAT
15	CHAPTER ALTHOUGH OF COURSE FIDUCIARY LAW IS BROADER
16	THAN CORPORATE LAW, THAT CHAPTER DID SPEND A LOT OF TIME
17	ON EMPIRICAL ANALYSIS OF FIDUCIARY DUTIES AND FIDUCIARY
18	LAW IN THE CORPORATE CONTEXT.
19	Q. I'VE ALWAYS THOUGHT OF FIDUCIARY LAW AS A
20	SUBSET OF CORPORATE LAW. AM I MISTAKEN?
21	A. I THINK THE AGENCY FOLKS WOULD SAY IT'S THE
22	OTHER WAY AROUND.
23	Q. OKAY. WELL, ASSUMING THAT YOUR
24	PRESENTATIONS THOSE ARE THE HIGHLIGHTS THAT ARE
25	SUFFICIENT TO SET THE STAGE FOR YOUR EXPERTISE ON WHAT
26	YOU WANT TO TELL THE COURT HERE OF ALL OF THE THINGS
27	WE'VE COVERED, YOUR PRESENTATIONS, YOUR BOOK
28	CONTRIBUTIONS, YOUR REFEREED PUBLICATIONS, HAVE YOU

1	LISTED ALL OF THEM IN HERE OR IS THIS A SUBSET OF
2	EVERYTHING?
3	A. I BELIEVE IT'S COMPLETE ON THE PUBLICATIONS. I
4	BELIEVE THE PRESENTATIONS ARE MOSTLY COMPLETE. I
5	BELIEVE THAT'S CORRECT.
6	MR. STICHT: YOUR HONOR, OUR INTENTION IS TO
7	PROCEED AT THIS POINT TO GET INTO PROFESSOR KLICK'S
8	EXPERTISE AS IT RELATES DIRECTLY TO THE CASE.
9	AND SO WE WOULD ASK THE COURT TO AUTHORIZE THAT
10	HE BE ACCEPTED AS AN EXPERT IN THIS CASE IN THE FIELD OF
11	ECONOMETRICS AND METHODOLOGIES THAT ARE PERTINENT TO
12	THIS CASE.
13	THE COURT: ANY OBJECTION?
14	MR. SEFERIAN: NO, YOUR HONOR.
15	THE COURT: YES. HE'S DENOTED AS AN EXPERT IN
16	THE FIELD. YOU MAY INQUIRE.
17	MR. STICHT: THANK YOU, YOUR HONOR.
18	BY MR. STICHT:
19	Q. NOW, PROFESSOR KLICK, I WOULD LIKE TO MOVE
20	AND YOU CAN REFER BACK TO YOUR CV IF YOU NEED TO AND ANY
21	PARTICULAR THAT YOU MIGHT REMEMBER NOW THAT YOU FORGOT.
22	WOULD YOU KINDLY START WITH ME, AND LET'S JUST
23	HAVE A LITTLE CLASS, IF YOU WILL, OR A LESSON ON THIS
24	SO-CALLED STATISTICAL STUDIES, THESE ECONOMETRICS, THESE
25	METHODOLOGIES, SET THE FRAMEWORK FOR US BEFORE WE GET
26	INTO SOME SPECIFICS OF WHAT IS THE FIELD OF
27	ECONOMETRICS, WHY IS IT RELEVANT TO THIS PARTICULAR
28	CASE, ALONG THAT WAY AND I'LL HELP YOU GUIDE YOU AS

L

1 YOU GO.

A. SURE. SO GENERICALLY, ECONOMETRICS, AND
PERHAPS APPLIED STATISTICS MORE GENERALLY, IS A SET OF
TOOLS THAT ATTEMPT TO TEST HYPOTHESES, HYPOTHESES ABOUT
SOCIAL SCIENCE, ECONOMICS, THE LAW. IN THIS PARTICULAR
CONTEXT, HYPOTHESES ABOUT CORPORATE GOVERNANCE.

AND SO THE FIELD INVOLVES ATTEMPTING TO TAKE
REAL WORLD DATA AND SEEING TO WHAT EXTENT IT IS
CONSISTENT WITH THE HYPOTHESIS OR IS IT MORE CONSISTENT
WITH SOME ALTERNATIVE HYPOTHESIS, FOR EXAMPLE.

AND SO THERE ARE VARYING LEVELS OF COMPLEXITY.
SO WITHIN THE FIELD OF APPLIED STATISTICS OR APPLIED
ECONOMETRICS, WE MIGHT START OUT WITH SOMETHING AS
DESCRIPTIVE STATISTICS, GET SOME DATA, FIGURE OUT SOME
AVERAGES AND COMPARE SOME AVERAGES.

16 Q. BEFORE YOU GO THERE, WHAT'S A REAL QUICK EASY17 EXAMPLE OF JUST THE HYPOTHESIS ITSELF?

A. WELL, SO A HYPOTHESIS THAT'S RELEVANT IN THIS
CASE IS IS IT THE CASE THAT GREATER FEMALE PARTICIPATION
ON CORPORATE BOARDS LEADS TO BETTER PERFORMANCE OR
BETTER OUTCOMES IN VARIOUS WAYS.

22 Q. YOU POSE THAT AS A QUESTION RATHER THAN A TRUE23 HYPOTHESIS WITH A PERIOD.

A. SURE. SO WE USUALLY START WITH A QUESTION
FIRST AND THEN WE ATTEMPT TO REFINE IT IN A TESTABLE
HYPOTHESIS. SO IF WE JUST SAID GENERICALLY WHETHER
GREATER FEMALE REPRESENTATION LEADS TO BETTER OUTCOMES,
WE'D HAVE TO COME UP WITH ALL SORTS OF DEFINITIONS ON

1 WHAT COUNTS AS A BETTER OUTCOME AND THINGS OF THAT 2 NATURE. SO STARTING VERY BROADLY, WE HAVE WHAT IS THE 3 GENERIC QUESTION, AND THEN THAT WOULD MOTIVATE A NUMBER 4 5 OF MORE SPECIFIC HYPOTHESES. SO A MORE SPECIFIC HYPOTHESIS MIGHT BE, IT IS 6 7 THE CASE THAT MORE WOMEN ON BOARDS -- A FIRM WITH HIGHER REPRESENTATION OF WOMEN ON ITS BOARD GENERATES HIGHER 8 9 STOCK RETURNS. THAT COULD BE A HYPOTHESIS. 10 AND THEN WE COULD GO TO THE DATA TO SEE WHETHER 11 THE HYPOTHESIS IS FALSIFIED OR NOT. 12 Ο. BUT IT'S IMPORTANT -- I DON'T WANT TO PUT WORDS IN YOUR MOUTH. ARE YOU SUGGESTING IT'S IMPORTANT TO 13 14 POSE THAT INITIAL HYPOTHESIS AS A QUESTION? 15 Α. WELL, USUALLY WE ARE DOING THIS WHOLE INOUIRY BECAUSE WE WANT TO ANSWER SOME QUESTIONS. 16 17 Ο. OKAY. 18 I THINK IT'S A MORE NATURAL, INTUITIVE WAY FOR Α. 19 PEOPLE TO REALLY START WRAPPING THEIR HEADS AROUND THE 20 QUESTION, THE ISSUE. OKAY, BECAUSE WOULDN'T YOU AGREE THAT IF I WAS 21 Q. 22 TO POSTULATE AS A HYPOTHESIS -- MORE WOMEN ON BOARDS 23 EOUALS GREATER PERFORMANCE IS ONE WAY TO PUT THE 24 HYPOTHESIS, BUT ANOTHER WAY TO PUT IT IS, DOES 25 INCREASING THE WOMEN ON BOARDS ACTUALLY RESULT IN HIGHER 26 PERFORMANCE? 27 AND YOU COULD ALSO HYPOTHESIZE IT AS IF YOU ADD 28 WOMEN ON BOARDS, WILL IT OR WILL IT NOT INCREASE

1 CORPORATE PERFORMANCE.

2 A. SO TO GET TO A TESTABLE HYPOTHESIS, WE CAN TAKE 3 THOSE QUESTIONS AND THEN REFINE THEM FOR SURE. 4 SO WE COULD -- THE GENERAL QUESTION THAT WE'RE 5 INTERESTED IN, YOU KNOW, ESSENTIALLY WHAT IS THE EFFECT OF WOMEN ON BOARDS. BUT THE MORE TRACTABLE STATISTICAL 6 7 OUESTION WILL BE SOMETHING ALONG THE LINES OF IS IT THE 8 CASE -- OR I'M SORRY. 9 MORE WOMEN ON BOARDS -- HYPOTHESIS NO. 1, MORE 10 WOMEN ON BOARDS LEADS TO BETTER PERFORMANCE. 11 HYPOTHESIS NO. 2, MORE WOMEN ON BOARDS LEADS TO 12 THE SAME PERFORMANCE. HYPOTHESIS NO. 3, WOMEN ON BOARDS LEADS TO 13 WORSE PERFORMANCE. 14 15 THAT WOULD SPAN THE SET OF POSSIBILITIES. AND 16 DEPENDING ON WHAT KIND OF DATA WE HAD OR WHAT KIND OF METHOD WE WERE USING, WHAT KIND OF NATURAL EXPERIMENT 17 18 MIGHT BE AVAILABLE, WE MIGHT FRAME THE HYPOTHESIS ANY OF 19 THOSE THREE WAYS. 20 I THINK YOU MENTIONED THE WORD FALSIFIED. WHAT Q. DID YOU MEAN BY THAT? 21 22 SO THE IDEA OF FALSIFICATION, I GUESS, GOES Α. 23 BACK TO KARL POPPER AT LEAST. 24 POPPER'S PHILOSOPHY OF SCIENCE IS THAT FOR 25 SOMETHING TO BE SCIENCE, IT NEEDS TO BE FALSIFIABLE, 26 RIGHT. SOMETHING THAT CAN'T BE FALSIFIED, THAT THERE'S 27 NO SCENARIO UNDER WHICH IT COULD POSSIBLY BE FALSIFIED, 28 POPPER DIDN'T CLASSIFY IT AS SCIENCE.

1 SO POPPER HAS HAD A STRONG INFLUENCE ON SORT OF 2 THE DEVELOPMENT OF USING STATISTICAL METHODS TO GENERATE 3 EVIDENCE. IN FACT IN THE FEDERAL COURTS, THIS IDEA OF 4 FALSIFICATION IS THE CORE -- IS ONE OF THE CORE OF, SAY, 5 THE DAUBERT STANDARD, FOR EXAMPLE. 6 Q. OKAY. I'M SORRY I INTERRUPTED, BUT YOU SAID 7 ONE OF THE METHODS YOU USED IS DESCRIPTIVE. 8 Α. SURE. 9 SO IF WE SORT OF START AT THE MOST SIMPLISTIC. 10 IT WILL PROBABLY BE INSUFFICIENT IN MOST SITUATIONS, BUT 11 THE MOST SIMPLISTIC SORT OF ATTEMPT TO DO SOME EMPIRICAL 12 ANALYSIS MIGHT BE TO SIMPLY LOOK AT DESCRIPTIVE 13 STATISTICS. 14 SO, FOR EXAMPLE, IN A CASE LIKE THIS, YOU HAVE, 15 LET'S SAY, AN AVERAGE OUTCOME, WHETHER IT BE STOCK 16 RETURNS OR SOME KIND OF ACCOUNTING MEASURE OF PERFORMANCE OR ANYTHING THAT YOU'RE INTERESTED IN 17 18 STUDYING. TAKE SORT OF THE AVERAGE OF THAT OUTCOME 19 AMONG FIRMS THAT HAVE EITHER THE REQUISITE NUMBER OF 20 WOMEN, DEPENDING ON WHAT YOUR HYPOTHESIS IS, AND COMPARE 21 THAT TO THE AVERAGE AMONG THE FIRMS THAT DON'T HAVE THE 22 REQUISITE NUMBER OF WOMEN, AND YOU COULD DO THAT IN SORT 23 OF COMPARISON. 24 NOW, DESCRIPTIVE STATISTICS, MEAN, MEDIAN, 25 THINGS LIKE THAT, ARE GENERALLY, AT LEAST IN THE ACADEMIC WORLD AND MORE BROADLY I THINK AS WELL --26 27 DESCRIPTIVE STATISTICS ARE A STARTING POINT. 28 THERE'S NEVER AN ENDING POINT BECAUSE

IMMEDIATELY YOU MIGHT SAY, WELL, WHAT IF THERE ARE OTHER
 DIFFERENCES RATHER THAN JUST THE BOARD COMPOSITION AMONG
 THESE SETS OF COMPANIES.

Q. OKAY. FOR EXAMPLE, IF I WAS TO TELL YOU THAT
THERE WERE 638 CORPORATIONS SUBJECT TO THE LAW IN THIS
CASE, AND WHAT YOU JUST TOLD US WOULD BE A DESCRIPTIVE
STATISTIC WOULD BE LOOKING AT ALL 638 AND GETTING THE
AVERAGE PERFORMANCE. IS THAT WHAT --

9 A. SURE. AND IT WOULDN'T HAVE TO BE PERFORMANCE. 10 IT COULD BE ANY OUTCOME THAT ONE WERE INTERESTED IN.

Q. BUT TO KEEP IT SIMPLE IN DOLLARS AND CENTS, JUST LOOKING AT THEIR BOTTOM LINE, AT THE END OF ONE YEAR VERSUS THE END OF ANOTHER YEAR WHERE THERE WAS A CHANGE IN THE BOARD COMPOSITION CAUSED BY SB-826, RIGHT, SO THAT'S LIKE A MEDIAN OF ALL THE 638 CORPORATIONS?

16 A. WELL, SO YOU CAN EITHER LOOK AT THE MEDIAN OR17 THE MEAN. THE TWO ARE DIFFERENT.

18 Q. THE AVERAGE?

19 A. RIGHT.

20 THOSE TWO ARE DIFFERENT. AND THERE ARE 21 DESCRIPTIVE STATISTICS REASONS TO PERHAPS PREFER ONE 22 VERSUS ANOTHER IN A GIVEN CIRCUMSTANCE, BUT SURE, ONE 23 COULD, SAY, COMPARE FIRM PERFORMANCE BEFORE AND AFTER 24 THE PASSAGE OF A LAW AND JUST DO IT ON THE MEAN -- THE 25 MEAN PERFORMANCE. THAT WOULD BE -- THAT WOULD MOSTLY BE 26 A DESCRIPTIVE STATISTICS EXERCISE.

Q. BUT THAT WOULD NOT, YOU SAID, ACCOUNT FOR SOMEOTHER VARIABLES. AND I WOULD SAY SUCH AS DIFFERENT

INDUSTRIES, MAYBE? 1 2 Α. WELL, SURE. SO THERE WOULD BE A QUESTION OF, 3 YOU KNOW, WHAT YOU'RE AVERAGING OVER, RIGHT. SO IF YOU REALLY ARE AVERAGING OVER THE WHOLE OF COMPANIES IN 4 5 CALIFORNIA, YOU MIGHT WORRY THAT THE COMPOSITION OF COMPANIES MIGHT HAVE CHANGED FROM PERIOD T TO PERIOD T 6 7 PLUS 1. 8 SO IMAGINE -- THIS OBVIOUSLY ISN'T TRUE, BUT 9 IMAGINE THAT IN PERIOD T, WHEN THE LAW WASN'T IN EFFECT, 10 THE MAJORITY OF THE COMPANIES IN CALIFORNIA WERE PUBLIC 11 UTILITIES. AND THEN IN YEAR T PLUS 1, THOSE PUBLIC 12 UTILITIES HAD FOLDED OR LEFT THE STATE OR SOMETHING LIKE THAT AND WERE ESSENTIALLY REPLACED BY HIGH-TECH FIRMS. 13 14 AND YOU LOOKED AT IT AND YOU SAID, OH, MY 15 GOODNESS, THE AVERAGE PERFORMANCE AFTER WAS MUCH HIGHER. 16 AND IF YOU WERE TEMPTED TO SAY, WELL, MAYBE THAT'S BECAUSE OF THE BOARD COMPOSITION CHANGE, WELL, YOU'D 17 18 HAVE TO RULE OUT MAYBE IT WAS BECAUSE OF THE INDUSTRIAL 19 COMPOSITION CHANGE, RIGHT. 20 AND SO DESCRIPTIVE STATISTICS HAVE A DIFFICULTY HANDLING THOSE KINDS OF DIFFERENCES OR THOSE KIND OF 21 22 CHANGES. 23 THERE'S A SEPARATE ISSUE -- EVEN IF WE COULD 24 RULE THAT OUT, THERE'S A SEPARATE ISSUE OF WHAT IF 25 THERE'S A GENERIC BACKGROUND TREND, RIGHT. SO IMAGINE WE GET BACK TO THE SITUATION WHERE WE LOOK IN PERIOD T 26 27 TO PERIOD T PLUS 1, PERFORMANCE GOES WAY UP, BUT THEN WE 28 LOOK IN THE REST OF THE COUNTRY THE PERFORMANCE WENT WAY

1 UP THERE AS WELL, RIGHT.

IT WOULD BE HARD TO SAY, WELL, THIS IS ANYTHING
SPECIFIC TO CALIFORNIA IF WE'RE OBSERVING THE SAME
TRAJECTORY OR THE SAME TREND EVERYWHERE ELSE.

5 AND SO DESCRIPTIVE STATISTICS WOULD HAVE A HARD 6 TIME KIND OF HANDLING THAT SORT OF THING TOO. THERE ARE 7 MORE COMPLICATED, AND I PRESUME WE'LL GET INTO THEM --8 THERE ARE MORE COMPLICATED METHODS THAT DO BETTER, DO A 9 BETTER JOB AND CAN HANDLE SOME OF THESE SUBTLETIES, BUT 10 DESCRIPTIVE STATISTICS WOULDN'T DO A GREAT JOB.

Q. BUT IT WOULD BE YOUR POSITION THAT DESCRIPTIVE STATISTICS ALONE WOULD NOT ALLOW YOU TO JUST REACH A CONCLUSION THAT ADDING MORE WOMEN ON THE BOARDS CAUSES AN INCREASE IN PERFORMANCE IF ALL YOU LOOKED AT WAS THE AVERAGE RATE OF RETURN OF THE PERFORMANCE OF THE CORPORATIONS IN THAT 638 SAMPLE?

A. NO, OF COURSE NOT. IN SORT OF RIGOROUS
ACADEMIC WORK, FOR SURE, YOU MIGHT START -- IN AN
INTRODUCTORY WAY, YOU MIGHT START WITH THE DESCRIPTIVE
STATISTICS, BUT DRAWING A CONCLUSION ON THAT BASIS WOULD
BE WHOLLY UNACCEPTABLE.

Q. OKAY. SO IS THERE ANYTHING ELSE WE NEED TO BE
AWARE OF ABOUT DESCRIPTIVE STATISTICS BEFORE WE MOVE
INTO WHAT'S NEXT?

A. SO IN SOME OF -- IN SOME ANALYSES, ADMITTEDLY
RELATIVELY SIMPLISTIC ANALYSES, THERE'S AN ATTEMPT TO DO
SOME OF THIS ACCOUNTING FOR DIFFERENCES. SO RATHER
THAN, FOR EXAMPLE, COMPARING THE AVERAGE RETURN OF THE

WHOLE SET OF COMPANIES, ONE MIGHT LOOK AT THE AVERAGE
 RETURN WITHIN A GIVEN INDUSTRY.

3 Q. OF THE PUBLIC UTILITY?

A. RIGHT. SO, SAY, INSTEAD OF DOING WHAT'S THE
AVERAGE RETURN ACROSS CALIFORNIA IN GENERAL, IT MIGHT BE
WHAT'S THE AVERAGE RETURN IN PUBLIC UTILITIES, WHAT'S
THE RETURN IN TELECOM, WHAT'S THE AVERAGE RETURN IN
HEALTH CARE ET CETERA, ET CETERA. AND THAT'S AN
ATTEMPT -- THAT'S AN ATTEMPT TO TRY TO SORT OUT SOME OF
THESE DIFFERENCES ACROSS FIRMS.

11 THE PROBLEM IS DOING THAT KIND OF ATTEMPT YOU 12 CAN REALLY ONLY HANDLE SORT OF ONE DIFFERENCE AT A TIME, 13 RIGHT. SO IF YOU -- IF YOU THOUGHT, WELL, YES, THE 14 INDUSTRY MIGHT BE RELEVANT, BUT SO MIGHT THE LIFE CYCLE 15 STAGE OF THE FIRM, RIGHT, HOW COULD YOU DO THAT. YOU 16 SAY, WELL, I'M ONLY GOING TO LOOK AT PUBLIC UTILITIES 17 WHO ARE IN THEIR FIRST FOUR YEARS, YOU KNOW, OF LIFE.

WELL, OKAY. BUT HOW DO YOU KNOW THAT FOUR
YEARS WAS THE RIGHT CATEGORY, RIGHT. AND THEN HOW DO WE
KNOW THAT THOSE ARE THE ONLY TWO VARIABLES THAT MATTER.

SO THERE ARE MORE SOPHISTICATED ECONOMETRICS
AND STATISTICAL TOOLS THAT ALLOW US TO ACCOUNT FOR
MULTIPLE DIFFERENCES AT ONCE AND THEN ALSO ALLOW US TO
HANDLE THESE SORT OF MORE CONTINUOUS DIFFERENCES.

THE DESCRIPTIVE STATISTICS AT BEST ONLY ALLOW
US TO HANDLE A COUPLE OF DIFFERENCES AT BEST AT ONCE,
AND REALLY DO A POOR JOB OF HANDLING ANY DIFFERENCE
THAT'S CONTINUOUS RATHER THAN, SAY, DISCRETE OR

1 CATEGORICAL.

2	Q. SO WHAT IS ONE OF THESE OTHER MORE RIGOROUS
3	METHODS BESIDES DESCRIPTIVE STATISTICS?
4	A. SO I THINK SORT OF THE BREAD AND BUTTER TOOL
5	FOR ALL APPLIED STATISTICIANS, ECONOMETRICIANS AND
6	OTHERS IS THE TOOL OF REGRESSION ANALYSIS.
7	THE COURT: THE TOOL OF WHAT?
8	THE WITNESS: REGRESSION ANALYSIS,
9	R-E-G-R-E-S-S-I-O-N. AND SO ESSENTIALLY, REGRESSION
10	ANALYSIS IS AN ATTEMPT TO USE THE DATA TO FIT A FUNCTION
11	OF VARIOUS VARIABLES IN THE RELATIONSHIP OF SOME OUTCOME
12	AND SOME PREDICTOR VARIABLES, RIGHT.
13	SO I MIGHT SAY I WANT TO EXPLAIN RETURNS AS A
14	FUNCTION OF BOARD COMPOSITION, LIFE CYCLE STAGE, MARKET
15	CAP, INDUSTRY, ET CETERA, ET CETERA, ET CETERA, RIGHT.
16	AND SO REGRESSION ANALYSIS IS AN ATTEMPT TO USE
17	THE DATA TO ESTIMATE THAT FUNCTION. AND SO THE WAY THAT
18	IT'S DONE IS ESSENTIALLY YOU HAVE DATA ON YOUR OUTCOME
19	VARIABLES, YOU HAVE DATA ON ALL OF YOUR PREDICTOR
20	VARIABLES, AND ESSENTIALLY WHAT YOU WANT TO DO IS YOU
21	WANT TO COME UP WITH ESSENTIALLY COEFFICIENTS IN THE
22	FUNCTION, PARAMETERS IN THE FUNCTION.
23	SO, YOU KNOW, SAY EXPLAINING MY RETURNS IN THE
24	FIRMS, YOU KNOW, LET'S SAY THERE'S AN INCREASE IN
25	RETURNS WHEN BOARDS HAVE MORE WOMEN ON THEM, THERE'S A
26	DECREASE IN RETURNS AS FIRMS MOVE ON IN THEIR LIFE
27	CYCLE. AND THEN FOR EACH OF 10 DIFFERENT INDUSTRIES,
28	THEY EACH HAVE DIFFERENT LEVELS OF RETURNS, RIGHT.

1 THE REGRESSION TECHNIQUE WOULD MATHEMATICALLY 2 ATTEMPT TO COME UP WITH THOSE PARAMETERS, ESSENTIALLY 3 THOSE COEFFICIENTS IN THAT FUNCTION IN SUCH A WAY THAT 4 IT'S OPTIMAL, WHERE OPTIMAL HERE IS DEFINED AS FITTING 5 THE DATA AS BEST AS POSSIBLE.

6 SO WHAT DOES THAT MEAN? WELL, IF YOU IMAGINE 7 THAT WE'VE GOT THE REAL DATA, RIGHT, WE'VE GOT DATA ON 8 THE RETURNS AND WE'VE GOT DATA ON EACH OF THOSE 9 PREDICTORS. AND THEN WE HAVE A POTENTIAL FUNCTIONAL 10 RELATIONSHIP, OUR MODEL, SO TO SPEAK -- OUR REGRESSION 11 MODEL, SO TO SPEAK.

12 THE MODEL IS GOING TO MAKE A PREDICTION BUT 13 THAT PREDICTION WON'T BE PERFECT. SO THERE WILL BE A 14 GAP BETWEEN WHAT THE DATA ACTUALLY SAY AND WHAT THE 15 MODEL SAYS.

16 THE GAP BETWEEN THOSE TWO THINGS WE REFER TO AS 17 AN ERROR, RIGHT. SO THE MODEL PREDICTS 10, THE REAL 18 DATA SAYS 12, AND SO THAT WOULD BE AN ERROR OF TWO. AND 19 YOU LOOK AT THIS ERROR FOR EVERY SINGLE DATA POINT.

20 WHAT THE REGRESSION TECHNIQUE DOES IS IT 21 CHOOSES PARAMETER VALUES SUCH THAT YOU'RE MINIMIZING THE 22 SQUARE OF THOSE ERRORS. YOU TAKE ALL THOSE ERRORS, YOU 23 SOUARE THEM AND YOU SUM THEM UP.

24AND YOU CHOOSE REGRESSION PARAMETERS SUCH THAT25YOU MINIMIZE ESSENTIALLY THAT ERROR BETWEEN THE REAL26DATA AND YOUR MODEL.

Q. HOW CAN YOU TRY TO PLACE THAT INTO PLAIN28 ENGLISH FOR US NEOPHYTES?

1	A. SURE.
2	Q. IN TERMS OF THE EXAMPLE AND MODIFY HOWEVER
3	YOU FEEL APPROPRIATE OF 638 CALIFORNIA FIRMS, AND
4	PUBLIC UTILITIES VERSUS NASDAQ COMPANIES. OR EVEN MORE
5	SPECIFICALLY, THE RUSSELL 3000 LARGER COMPANIES AND THE
6	MICROCAP.
7	A. SURE. LET'S START MORE SIMPLE THAN THAT.
8	SO REGRESSION IS NOT A NEW TOOL. IT'S NOT A
9	NEW TECHNIQUE. IT WAS INVESTED BY GAUSS, THE FAMOUS
10	MATHEMATICIAN, BACK IN THE EARLY 1800S.
11	GAUSS WAS INTERESTED IN PREDICTING THE LOCATION
12	OF A COMET.
13	Q. NEXT WEEK THERE'S A METER, I THINK, COMING
14	WITHIN A MILLION MILES OF EARTH.
15	A. YES, REGRESSION TOOLS ARE USED IN THAT CONTEXT
16	ТОО.
17	SO GAUSS HAS DATA, HISTORIC DATA THAT SAYS, YOU
18	KNOW, AT THIS TIME PERIOD, HERE'S WHERE THE COMET WAS,
19	AT THIS PERIOD HERE'S WHERE THE COMET WAS. AND HE
20	WANTED TO BE ABLE TO PREDICT WHERE THE COMET WOULD BE
21	SORT OF IN THE SUBSEQUENT PERIODS.
22	SO HE HAD A VERY SIMPLE REGRESSION MODEL HE
23	WANTED TO ESTIMATE. I HAVE SOME FUNCTION IN THIS
24	CASE, HIS PREDICTOR OF IS OF TIME. IF TIME IS MY
25	PREDICTOR VARIABLE, I WANT TO BE ABLE TO PREDICT
26	LOCATION BASED ON TIME.
27	I HAVE THIS HISTORICAL DATA THAT TELLS ME AT
28	TIME T HERE'S WHERE THE COMET WAS, TIME T PLUS 1 HERE'S
1 WHERE THE COMET WAS, ET CETERA, ET CETERA.

IF I COME UP WITH COEFFICIENTS OR PARAMETERS ON
THIS TIME VARIABLE SUCH THAT I HAVE A MODEL THAT BEST
FITS THIS HISTORICAL DATA, RIGHT, I'M GOING TO KEEP
CHANGING MY PARAMETERS AS I KEEP GETTING A CLOSER AND
CLOSER FIT TO THE HISTORICAL DATA.

ONCE I GET TO THE CLOSEST POSSIBLE FIT, THAT'S
GOING TO BE MY FAVORITE MODEL. THAT'S GOING TO BE MY
REGRESSION MODEL. THAT'S HOW HE INVENTED REGRESSION.

NOW, YOU MIGHT SAY, GEE, THAT SOUNDS REALLY
COMPLICATED. YOU KNOW, DO YOU JUST KEEP TRYING TRIAL
AND ERROR? IT TURNS OUT IT'S A CALCULUS PROBLEM. IT
TURNS OUT THAT THIS IS JUST AN OPTIMIZATION PROBLEM IN
CALCULUS.

YOU CAN TAKE YOUR ERRORS AS DEFINED AS THE
DIFFERENCE BETWEEN THE REAL DATA MINUS YOUR PREDICTED
DATA, RIGHT; THAT'S AN ERROR. YOU SQUARE THOSE THINGS,
AND THEN YOU JUST CHOOSE YOUR PARAMETERS IN YOUR MODEL
VIA CALCULUS IN THE WAY THAT MINIMIZES THAT DIFFERENCE.

20 SO THEN BACK TO YOUR MORE COMPLICATED EXAMPLE, 21 GAUSS WAS ONLY LOOKING AT OUTCOMES OF WHERE THE COMET IS 22 AND PREDICTORS IN TERMS OF TIME, BUT NOW YOU WANT TO 23 HAVE A MUCH MORE COMPLICATED MODEL.

YOU WANT TO HAVE A MODEL THAT SAYS, OKAY, WE'RE
PREDICTING AN OUTCOME, BUT NOW I WANT TO HAVE 20
DIFFERENT VARIABLES, RIGHT.

THAT WOULD HAVE BEEN A HARD PROBLEM FOR GAUSS,
NOT CONCEPTUALLY. GAUSS'S KNEW CALCULUS. IT'S STILL A

1 CALCULUS PROBLEM. BUT GAUSS'S COMPUTER WASN'T VERY GOOD 2 SO HE HAD TO DO IT BY HAND. BUT IN MODERN COMPUTERS -- THE MODERN COMPUTERS 3 4 CAN DO THIS CALCULUS PROBLEM QUITE SIMPLY. AND THE 5 COMPUTER WILL SPIT OUT HERE ARE THE PARAMETERS THAT MAKE THAT BEST FIT TO THE DATA. 6 SO WE WOULD DO THE SAME THING IN YOUR EXAMPLE. 7 8 WE FEED THE COMPUTER WITH DATA ON THE OUTCOME WE WANT TO 9 STUDY, AND THEN WE WOULD FEED THE DATA IN TERMS OF EACH OF THESE PREDICTORS, AND WE WOULD SAY, HEY, COMPUTER, 10 11 USE THIS REGRESSION TOOL TO MINIMIZE THOSE ERRORS. AND 12 THE COMPUTER WILL SPIT IT OUT AND IT WILL SAY HERE'S THE BEST ESTIMATE OR THE BEST MODEL THE BEST FIT IS PROVIDED 13 14 BY A MODEL THAT SAYS THE OUTCOME EQUALS, YOU KNOW A PLUS 15 B TIMES SOME BOARD COMPOSITION VARIABLE, PLUS C TIMES SOME LIFE CYCLE VARIABLE, PLUS D TIMES SOME MARKET CAP 16 VARIABLE, PLUS E TIMES SOME INDUSTRY INDICATOR, SO ON 17 AND SO FORTH. 18 19 Q. SO SELECTING THE PARAMETERS, AS YOU CALLED 20 THEM? A. ESTIMATING. I WOULD SAY ESTIMATING. 21 22 ESTIMATING THE PARAMETERS. IS THAT A CONSCIOUS Ο. 23 CHOICE THAT'S MADE AT THE TIME OF THE ANALYSIS? 24 SO ONCE THE DECISIONS ARE MADE AS TO WHAT Α. 25 SHOULD BE IN THIS FUNCTIONAL RELATIONSHIP, THE 26 REGRESSION ITSELF IS LARGELY MECHANICAL. 27 WHERE THE DECISIONS ARE MADE ARE IN CONSIDERING 28 WHAT -- WHAT VARIABLE TO PUT INTO THE MODEL.

1 Ο. OKAY, YOU'VE TOLD US THAT THE DESCRIPTIVE 2 STATISTICS WOULD NOT NECESSARILY BE RELIABLE BECAUSE IT 3 HAS INHERENT ERRORS JUST BY LOOKING AT AVERAGES OR 4 MEDIANS, MEANS OR MEDIANS OF -- AND I USED THE EXAMPLE 5 OF JUST DOLLARS AND CENTS -- FINANCIAL PERFORMANCE IN TERMS OF THE BOTTOM LINE --6 7 UH-HUH. Α. Q. -- AMONG 638 CORPORATIONS. SO NOW THAT YOU'VE 8 9 INTRODUCED REGRESSION, HOW DOES THAT HELP IMPROVE OR 10 REDUCE THE ERROR RATE AND IMPROVE THE OUTCOME OF KNOWING 11 WHAT THE STATISTICS WILL TELL YOU ABOUT THOSE 638 12 CORPORATIONS BY ADDING WOMEN ON BOARDS? RIGHT. SO IN DETERMINING WHAT IS THE EFFECT OF 13 Α. 14 A PARTICULAR VARIABLE ON THE OUTCOME, YOU KNOW, THE 15 REGRESSION WILL GIVE YOU AN ANSWER, BUT THAT ANSWER IS 16 ONLY RELIABLE IN THE SENSE THAT YOU ARE MAKING COMPARISONS THAT ARE VALID. 17 18 SO, FOR EXAMPLE, IF WE GO BACK TO AN EARLIER 19 ILLUSTRATION, IMAGINE THAT WE HAD THESE 600 FIRMS AND IT 20 JUST SO TURNS OUT THAT ALL OF THE HIGH-TECH FIRMS HAVE WOMEN ON THEIR BOARDS, BUT ALL THE OTHER FIRMS ARE THESE 21 22 PUBLIC UTILITIES. 23 WE HAD THAT PROBLEM EARLIER IN THE DESCRIPTIVE 24 STATISTICS. 25 WELL, IF WE RUN A REGRESSION WHERE THE ONLY 26 EXPLANATORY VARIABLE, THE ONLY PREDICTOR VARIABLE WE 27 HAVE IN THIS IS THIS BOARD COMPOSITION VARIABLE, WE'RE 28 GOING TO HAVE THE SAME PROBLEM.

THE REGRESSION WILL ESTIMATE HERE'S THE 1 2 DIFFERENCE FOR THE FIRMS THAT HAVE WOMEN ON THEIR BOARDS 3 AND THE FIRMS THAT DON'T. BUT IF YOU HAVEN'T TOLD THE 4 REGRESSION TO ACCOUNT FOR THE DIFFERENCE OF INDUSTRIES, 5 WELL, YOU'RE GOING TO HAVE THE SAME PROBLEM YOU HAD EARLIER WITH JUST THE DESCRIPTIVE STATISTICS. 6 7 THE REGRESSION DOESN'T SOLVE THAT PROBLEM 8 UNLESS YOU ESSENTIALLY CHOOSE TO ACCOUNT FOR THESE OTHER 9 DIFFERENCES. 10 AT THE END OF THE DAY, WHAT YOU WANT TO DO IS 11 YOU WANT -- YOU KNOW, THE METAPHOR IS YOU WANT AN APPLES 12 TO APPLES COMPARISON. I'LL MAKE IT EVEN MORE CLEAR IF LESS -- IF MORE 13 CONTRIVED, BUT ALSO MORE CLEAR. WHAT WE WOULD REALLY 14 WANT IS TO OBSERVE THE IDENTICAL COMPANY UNDER TWO 15 16 CIRCUMSTANCES. ONE WITH WOMEN, OR WHATEVER DETERMINANT WE HAVE 17 18 FOR HOW MANY WOMEN WE WANT ON THE BOARD. WE OBSERVE THE 19 FIRM UNDER THAT SCENARIO, AND WE WANT TO OBSERVE THE 20 FIRM IN THE SCENARIO WHERE THAT DOESN'T OCCUR. IF THE FIRM IS IDENTICAL IN EVERY SINGLE WAY 21 22 EXCEPT FOR THIS BOARD COMPOSITION ISSUE, IF WE SEE ANY 23 DIFFERENCE IN OUTCOMES, THERE'S ONLY TWO POSSIBILITIES 24 AS TO WHAT'S GENERATING THAT DIFFERENCE. 25 ONE IS RANDOM CHANCE. THE REAL WORLD IS RANDOM, THERE'S NOISE, YOU KNOW, THAT COULD BE PART OF 26 27 IT. 28 BUT THE OTHER PART OF IT WOULD BE, WELL, WE

KNOW THAT THERE IS A DIFFERENCE. THEY HAVE DIFFERENT 1 2 BOARD COMPOSITIONS. AND SO THAT'S WHAT WE WOULD LIKE TO BE ABLE TO 3 DO, GET DOWN TO THAT COMPARISON. TWO IDENTICAL FIRMS, 4 5 ONE WITH A CERTAIN BOARD STRUCTURE, ONE WITHOUT. DO THE 6 COMPARISON. 7 NOW, TWO PROBLEMS THERE. ONE IS, IF WE'RE LITERALLY ONLY COMPARING TWO FIRMS, THE POSSIBILITY THAT 8 9 THIS RANDOM NOISE OR THIS RANDOM CHANCE IS DRIVING ANY DIFFERENCE IS RELATIVELY LARGE. 10 11 SO WE REALLY DON'T JUST NEED TWO FIRMS. WE 12 NEED LOTS OF FIRMS. BUT THAT'S OKAY. IMAGINE NOW WE HAVE 500 FIRMS ALL IDENTICAL IN EVERY SINGLE WAY, BUT 13 14 250 OF THEM HAVE, YOU KNOW, THE REQUISITE NUMBER OF 15 WOMEN ON THE BOARD, THE OTHER 250 DON'T. IF WE GO AND LOOK AT THAT AND WE SEE A 16 DIFFERENCE IN THE OUTCOME, WELL, NOW, NOW THAT WE'VE 17 18 AVERAGED OVER A LARGE NUMBER OF FIRMS, THE RANDOM 19 COINCIDENCE TENDS TO WASH OUT. 20 WE GET MORE OBSERVATIONS IN YOUR ANALYSIS. WE'VE GOT STATISTICAL RULES LIKE THE LAW OF LARGE 21 22 NUMBERS THAT TELL US THE WHITE NOISE, THE NOISE OR THE 23 RANDOM PART OF THINGS IS GOING TO AVERAGE AWAY. 24 SO IN THIS 500-FIRM ANALYSIS, IF THE ONLY 25 DIFFERENCE BETWEEN THESE TWO FIRMS OR THESE -- I'M 26 SORRY, THESE 500 FIRMS IS BOARD COMPOSITION, AND WE ALSO 27 SEE SOME DIFFERENCE IN TERMS OF AN OUTCOME, WELL, WE 28 FEEL PRETTY CONFIDENT IT MUST BE BECAUSE OF THE BOARD

1 COMPOSITION, RIGHT.

28

2 WHAT'S THE PROBLEM? WELL, THE FIRMS ARE NEVER 3 IDENTICAL, RIGHT. THE FIRMS ARE NEVER IDENTICAL. AND 4 SO WE USE REGRESSION ANALYSIS TO AT LEAST START TO MAKE 5 ADJUSTMENTS FOR THE WAYS IN WHICH THESE FIRMS ARE 6 DIFFERENT OTHER THAN JUST THROUGH THE BOARD COMPOSITION.

Q. THAT WAS GOING TO BE MY QUESTION. ARE YOU
8 SUGGESTING THAT THE REGRESSION TECHNIQUE HAS BEEN USED
9 TO TRY TO EQUALIZE THE COMPANIES?

A. EQUALIZE IS AN OKAY WORD. I WOULD ACTUALLY SAY
IT'S USED TO ADJUST THE OTHER DIFFERENCES, THE EFFECTS
OF THE OTHER DIFFERENCES AMONG THE COMPANIES. AGAIN,
TRYING TO GET BACK TO THIS APPLES TO APPLES COMPARISON
OR THIS -- YOU KNOW, THIS DOPPELGANGER COMPARISON. FIRM
WITH AND FIRM WITHOUT.

16 Q. SO GET AWAY FROM THE PUBLIC UTILITIES, NASDAQ, 17 APPLES AND ORANGES?

A. RIGHT, SO ESSENTIALLY WHAT THE REGRESSION WOULD
DO IS IT WOULD LOOK AT THE DATA THROUGH THE GAUSS-TYPE
METHOD THAT I JUST TALKED ABOUT. THEY WOULD LOOK AT THE
DATA AND THEY WOULD SAY, HMM, IT LOOKS LIKE ON AVERAGE
INDEPENDENTLY OF EVERYTHING ELSE PUBLIC UTILITIES HAVE
LOWER RETURNS.

24 SO ESSENTIALLY WHAT THE REGRESSION DOES IS IT 25 WILL THEN ADD BACK IN EXTRA RETURN TO THOSE PUBLIC 26 UTILITIES SO THEY NO LONGER ARE DIFFERENT BECAUSE OF 27 THEIR PUBLIC UTILITY ASPECTS OF IT, RIGHT.

BUT IT'S GOING TO BE DOING THIS FOR EACH OF THE

1 CONTROL OR PREDICTOR VARIABLES THAT YOU TELL THE 2 REGRESSION TO LOOK AT. IT'S CONSTANTLY MAKING THOSE 3 ADJUSTMENTS ON THE DIFFERENT DIMENSIONS WITH THE HOPE OF GETTING BACK TO THIS APPLES TO APPLES WHERE THE ONLY 4 5 DIFFERENCE IS THE BOARD COMPOSITION, AND THEN TRIES TO MAKE SOME INFERENCE ABOUT, OKAY, WHAT COMPONENT OF ANY 6 7 DIFFERENCE IN THE OUTCOME IS DUE TO THE BOARD 8 COMPOSITION ISSUE.

9 Q. WELL, IT MAY BE AN UNFAIR QUESTION, BUT AT THIS 10 EARLY STAGE, IS THE SIMPLE MODEL THAT WE'RE WORKING WITH 11 ABOUT USING AVERAGE RATE OF RETURNS AMONG 638 12 COMPANIES -- 500, IN YOUR EXAMPLE -- AND THEN USING 13 REGRESSION TO ADJUST FOR DIFFERENCES IN THOSE COMPANIES 14 TO MAKE THEM MORE LIKE APPLES AND APPLES, IS THAT THE 15 END OF THE MATTER?

16DOES THAT GIVE US CONFIDENCE THAT WE'RE READY17NOW TO COMPARE THOSE CORPORATIONS WITH THE WOMEN ON18BOARDS AND THOSE THAT DON'T HAVE THE REQUIRED NUMBER?

A. UNFORTUNATELY, NO.

19

20 SO CONCEPTUALLY IF IN OUR REGRESSION MODEL WE HAVE DATA ON EVERY DIFFERENCE, RIGHT, SO THAT THE 21 22 ADJUSTMENTS REALLY GOT US, YOU KNOW, HYPOTHETICALLY, TO 23 THAT, YOU KNOW, FIRM -- THE EXACT SAME FIRM WITH AND 24 WITHOUT -- IF WE HAD DATA ON EVERYTHING, ON EVERY 25 POSSIBLE DIFFERENCE BETWEEN OR AMONG THESE FIRMS, THEN 26 YES, REGRESSION WOULD ALLOW US TO DO THAT. 27 THE PROBLEM IS -- WELL, FIRST THERE'S A PROBLEM 28 IN THAT THERE'S A QUESTION OF DO WE EVEN KNOW WHAT ARE

1 ALL THE VARIABLES ONE NEEDS TO ADJUST FOR, RIGHT. 2 THERE'S NO -- LOOK, I'VE TAUGHT CORPORATE LAW, I'VE TAUGHT CORPORATE GOVERNANCE. I'VE READ BROADLY IN THE 3 4 LITERATURE. ECONOMICS AND FINANCE ISN'T PHYSICS, RIGHT. 5 THE THEORY DOESN'T SAY, HERE ARE THE 25 VARIABLES THAT MATTER, RIGHT, THEORETICALLY. 6 7 WE'VE GOT SOME INTUITIONS. WE'VE GOT SORT OF 8 SOME INSTITUTIONAL KNOWLEDGE, THINGS LIKE THAT. BUT AT 9 THE END OF DAY, WE HAVE TO RECOGNIZE THAT THERE MIGHT BE SOME DIMENSIONS OF ADJUSTMENT THAT WE JUST HAVEN'T 10 11 THOUGHT OF, RIGHT. SO THAT'S ONE PROBLEM. THAT PROBLEM IS AT 12 LEAST TRACTABLE IN A SENSE, RIGHT. YOU THINK -- YOU 13 14 TALK TO PEOPLE, THEY SAY, HEY, FROM MY PERSPECTIVE, YOU 15 MISSED THIS VARIABLE, YOU MISSED THAT VARIABLE, YOU 16 MISSED ANOTHER VARIABLE. IF YOU DO THAT ENOUGH, CAN YOU EVER BE CERTAIN, 17 NO, BUT YOU MAYBE BECOME MORE CONFIDENT. 18 19 HERE'S THE BIGGER PROBLEM. THE BIGGER PROBLEM 20 IS WHAT IF THE FIRMS DIFFER ON THE BASIS OF THINGS THAT ARE UNQUANTIFIABLE. SOMETHING THAT WE JUST DON'T HAVE 21 22 OR COULDN'T HAVE DATA ON. 23 Q. EXAMPLE? 24 IMAGINE THAT WE THINK THAT THERE'S A DIFFERENCE Α. 25 THAT SOME FIRMS ARE PROGRESSIVE AND SOME FIRMS AREN'T, 26 WHATEVER THAT MEANS. 27 I MEAN, THE VERY FACT THAT I SAY WHATEVER THAT 28 MEANS, WE ALL HAVE KIND OF A SENSE OF WHAT WE USE

1	PROGRESSIVE TO MEAN. IF WE SAT DOWN AND THOUGHT, WELL,
2	HOW DO YOU QUANTIFY THAT, IT WOULD BE REALLY HARD,
3	RIGHT.
4	IT MAY WELL BE THE CASE THAT PROGRESSIVE HAS
5	SOME EFFECT ON WHATEVER OUTCOME WE'RE INTERESTED IN. IT
6	MIGHT AFFECT RETURNS. IT MIGHT AFFECT ESG-TYPE STUFF
7	THAT THE FIRM CHOOSES TO DO THAT'S BEEN STUDIED IN THIS
8	CONTEXT, THIS GENERIC KIND OF HOW PROGRESSIVE THE FIRM
9	IS.
10	Q. WHAT IS ESG?
11	A. ENVIRONMENTAL AND SOCIAL ISSUES. IT'S BECOME
12	SORT OF A TERM OF ART, GOVERNANCE ISSUES RELATED TO THE
13	ENVIRONMENT AND SOCIAL ISSUES. AND IT'S BEEN STUDIED IN
14	THIS GENERAL CONTEXT AS BEING ONE OUTCOME PEOPLE WANT TO
15	LOOK AT.
16	SO WE ALL IN THIS ROOM COULD PROBABLY AGREE
17	THERE'S SOMETHING CALLED PROGRESSIVISM. IT PROBABLY
18	BEARS SOME RELATIONSHIP TO VARIOUS OUTCOMES, INCLUDING
19	MAYBE THESE ESG OUTCOMES.
20	WE PROBABLY ALSO MIGHT AGREE THAT MORE
21	PROGRESSIVE FIRMS AGAIN, WHATEVER EXACTLY THAT
22	MEANS MIGHT BE MORE APT TO HIRE MORE WOMEN OR PUT
23	MORE WOMEN ON THE BOARD, RIGHT.
24	HERE'S THE PROBLEM. WHEN WE GO BACK TO THE
25	REGRESSION, HOW DO YOU ADJUST FOR, HOW DO YOU CONTROL
26	FOR HOW PROGRESSIVE THE FIRM IS.
27	YOU LOOK AT ANY DATA SET IN THE WORLD, THERE'S
28	NO VARIABLE IN COMPUSTAT, FOR EXAMPLE, WHICH IS A

GENERAL FINANCIAL DATABASE. THERE'S NO VARIABLE IN 1 2 COMPUSTATE THAT SAYS IN 1996 APPLE HAD A PROGRESSIVE 3 RATING OF SIX. THAT DATA DOESN'T EXIST. IT DOESN'T MEAN 4 5 THEY'RE NOT REAL, RIGHT. WE ALL INTUITIVELY CAN IMAGINE THIS IS PROBABLY SOMETHING THAT'S IMPORTANT, BUT IT'S 6 7 VERY UNOUANTIFIABLE NATURE MAKES IT THE CASE THAT WE 8 CAN'T PUT IT INTO THE REGRESSION. 9 GENERICALLY ECONOMISTS CALL THIS UNOBSERVED HETEROGENEITY, OR MAYBE MORE SIMPLY, UNOBSERVABLES. 10 11 AND HERE'S THE PROBLEM: IF WE HAVE THESE 12 UNOBSERVED VARIABLES THAT MATTER, MATTER FOR THE OUTCOME, THAT WE'RE STUDYING, AND THEY MIGHT BE 13 14 CORRELATED WITH THE VARIABLES THAT WE'RE TRYING TO 15 EXAMINE, RIGHT -- SO IN THIS EXAMPLE, PROGRESSIVISM, WE 16 THINK IT MIGHT MATTER FOR THE FIRM OUTCOME, AND WE CAN ALSO IMAGINE IT'S PROBABLY CORRELATED WITH HOW LIKELY A 17 FIRM IS TO ADD WOMEN TO THE BOARD. 18 19 BUT WE DON'T CONTROL FOR IT. WE DON'T ADJUST 20 FOR THAT IN THE REGRESSION. THE MECHANICS OF THE REGRESSION ARE GOING TO LEAD THE ESTIMATE FOR THE WOMEN 21 22 BOARD COMPOSITION VARIABLE TO INCLUDE BOTH THE TRUE 23 CAUSAL EFFECT OF WOMEN ON BOARDS AND THEN SOME OF THIS 24 PROGRESSISM EFFECT. 25 THIS IS CALLED THE OMITTED VARIABLE BIAS. THIS 26 IS THE UBIQUITOUS PROBLEM THAT SAY GUYS LIKE ME, THAT 27 EMPIRICAL FOLKS ARE CONSTANTLY FACING IN DRAWING CAUSAL 28 INFERENCES WITH EMPIRICAL METHODS.

1	THE COURT: THAT'S THE UNICORN OUESTION.
2	THE WITNESS, I'M SORRY?
2	THE WINESS. I F SOUCH:
S	THE COURT: THAT'S THE UNICORN QUESTION.
4	THE WITNESS: I THINK SO. SOMETIMES I THINK WE
5	DO OKAY WITH IT. MAYBE IT'S NOT QUITE A UNICORN. BUT
6	YES, SO REGRESSION, WHILE IT'S MORE "SOPHISTICATED"
7	MIGHT BE A REASONABLE WORD THAN JUST DOING DESCRIPTIVE
8	STATISTICS, IT TOO HAS ITS LIMITATIONS.
9	WHEREAS WITH DESCRIPTIVE STATISTICS WE ARE NOT
10	ADJUSTING OR WE'RE LIKELY NOT ADJUSTING FOR BOTH
11	OBSERVABLE AND UNOBSERVABLE CHARACTERISTICS. WHEN WE
12	GET TO REGRESSIONS, WE'RE ADJUSTING FOR OBSERVABLE
13	CHARACTERISTICS, BUT WE'RE STILL NOT ADJUSTING FOR
14	UNOBSERVABLE CHARACTERISTICS.
15	Q. WHAT DO WE DO WITH THAT PROBLEM?
16	A. WELL, THAT IS THE UNICORN QUESTION, RIGHT. AND
	SO WE TAKE A LITTLE BIT OF A DEPARTURE HERE AND
17	SO WE TAKE A BITTLE DIT OF A DEFAUTORE HERE AND
17 18	REFERENCE SOME OF THE THINGS I SAID EARLIER.
17 18 19	REFERENCE SOME OF THE THINGS I SAID EARLIER. IN SOME WAYS IN SOME CONTEXTS THERE'S AN EASY
17 18 19 20	REFERENCE SOME OF THE THINGS I SAID EARLIER. IN SOME WAYS IN SOME CONTEXTS THERE'S AN EASY SOLUTION. THE EASY SOLUTION IS TO RUN AN EXPERIMENT.
17 18 19 20 21	REFERENCE SOME OF THE THINGS I SAID EARLIER. IN SOME WAYS IN SOME CONTEXTS THERE'S AN EASY SOLUTION. THE EASY SOLUTION IS TO RUN AN EXPERIMENT. SO, YOU KNOW, USING MY EXAMPLE FROM BEFORE, IMAGINE THAT
17 18 19 20 21 22	REFERENCE SOME OF THE THINGS I SAID EARLIER. IN SOME WAYS IN SOME CONTEXTS THERE'S AN EASY SOLUTION. THE EASY SOLUTION IS TO RUN AN EXPERIMENT. SO, YOU KNOW, USING MY EXAMPLE FROM BEFORE, IMAGINE THAT MERCK DECIDES, HEY, WE'RE GOING TO INVENT A CURE FOR THE
17 18 19 20 21 22 23	REFERENCE SOME OF THE THINGS I SAID EARLIER. IN SOME WAYS IN SOME CONTEXTS THERE'S AN EASY SOLUTION. THE EASY SOLUTION IS TO RUN AN EXPERIMENT. SO, YOU KNOW, USING MY EXAMPLE FROM BEFORE, IMAGINE THAT MERCK DECIDES, HEY, WE'RE GOING TO INVENT A CURE FOR THE COMMON COLD, RIGHT.
17 18 19 20 21 22 23 24	REFERENCE SOME OF THE THINGS I SAID EARLIER. IN SOME WAYS IN SOME CONTEXTS THERE'S AN EASY SOLUTION. THE EASY SOLUTION IS TO RUN AN EXPERIMENT. SO, YOU KNOW, USING MY EXAMPLE FROM BEFORE, IMAGINE THAT MERCK DECIDES, HEY, WE'RE GOING TO INVENT A CURE FOR THE COMMON COLD, RIGHT. AND HERE'S WHAT WE'RE GOING TO DO. WE GET
17 18 19 20 21 22 23 24 25	REFERENCE SOME OF THE THINGS I SAID EARLIER. IN SOME WAYS IN SOME CONTEXTS THERE'S AN EASY SOLUTION. THE EASY SOLUTION IS TO RUN AN EXPERIMENT. SO, YOU KNOW, USING MY EXAMPLE FROM BEFORE, IMAGINE THAT MERCK DECIDES, HEY, WE'RE GOING TO INVENT A CURE FOR THE COMMON COLD, RIGHT. AND HERE'S WHAT WE'RE GOING TO DO. WE GET LIKE YOU DO IN MEDICAL TRIALS, WE'RE GOING TO GET 1,000
17 18 19 20 21 22 23 24 25 26	REFERENCE SOME OF THE THINGS I SAID EARLIER. IN SOME WAYS IN SOME CONTEXTS THERE'S AN EASY SOLUTION. THE EASY SOLUTION IS TO RUN AN EXPERIMENT. SO, YOU KNOW, USING MY EXAMPLE FROM BEFORE, IMAGINE THAT MERCK DECIDES, HEY, WE'RE GOING TO INVENT A CURE FOR THE COMMON COLD, RIGHT. AND HERE'S WHAT WE'RE GOING TO DO. WE GET LIKE YOU DO IN MEDICAL TRIALS, WE'RE GOING TO GET 1,000 PEOPLE THAT HAVE COLDS. WE'RE GOING TO RANDOMIZE THEM,
17 18 19 20 21 22 23 24 25 26 27	REFERENCE SOME OF THE THINGS I SAID EARLIER. IN SOME WAYS IN SOME CONTEXTS THERE'S AN EASY SOLUTION. THE EASY SOLUTION IS TO RUN AN EXPERIMENT. SO, YOU KNOW, USING MY EXAMPLE FROM BEFORE, IMAGINE THAT MERCK DECIDES, HEY, WE'RE GOING TO INVENT A CURE FOR THE COMMON COLD, RIGHT. AND HERE'S WHAT WE'RE GOING TO DO. WE GET LIKE YOU DO IN MEDICAL TRIALS, WE'RE GOING TO GET 1,000 PEOPLE THAT HAVE COLDS. WE'RE GOING TO RANDOMIZE THEM, RIGHT. FIVE HUNDRED OF THE PEOPLE ARE GOING TO GET THE

WE'RE THEN GOING TO FOLLOW THESE PEOPLE FOR THE
 NEXT COUPLE OF WEEKS. WE'RE GOING SEE HOW HAS THE
 OUTCOME IN THIS CASE, INDICATORS OF THE COLD, HOW HAS
 THAT CHANGED FOR THE PEOPLE WHO ARE IN THE TREATMENT
 GROUP, THE PEOPLE WHO ACTUALLY GOT THE -- YOU KNOW, WHO
 GOT THE MEDICINE.
 AND THEN WE'RE GOING TO COMPARE THAT TO THE

8 PEOPLE IN THE CONTROL OR THE COMPARISON GROUP, THE FOLKS
9 WHO JUST GOT THE SUGAR PILL, THE PLACEBO.

NOW, YOU MIGHT ASK, WELL, HOW DOES THAT SOLVE
THE PROBLEM. HERE'S HOW IT SOLVES THE PROBLEM. IF YOU
LOOK AT ARTICLES THAT LOOK AT MEDICAL TRIALS, IT'S NOT
AS THOUGH THEY SUDDENLY CAN CONTROL FOR, ADJUST FOR ALL
THOSE DIFFERENCES WE WERE TALKING ABOUT.

SOMETIMES THEY DO. SOMETIMES THEY'LL PUT IN A REGRESSION WHERE THEY CONTROL FOR IS IT A MAN, IS IT A WOMAN. THEY WILL PUT A VARIABLE FOR AGE OR SOMETHING LIKE THAT.

BUT OF COURSE, THEY TOO WOULDN'T BE ABLE TO
ADJUST FOR UNOBSERVABLES. WHAT IF -- THERE'S THIS
GENERAL HEALTH VARIABLE CALLED ROBUSTNESS. SOME PEOPLE
ARE MORE ROBUST THAN OTHERS.

THEY DON'T KNOW HOW TO QUANTIFY IT. SO I COULDN'T ADJUST FOR IT IN THE REGRESSION. SO YOU MIGHT SAY, WELL, HOW DOES MERCK IN THE MEDICAL TRIAL SOLVE THIS PROBLEM.

27HERE'S THE GENIUS OF RANDOMIZATION. YOU DON'T28GET A BIAS IF THERE IS NO CORRELATION BETWEEN THE

1	UNOBSERVABLE, TO THE STUFF THAT YOU HAVEN'T ADJUSTED
2	FOR, AND THE STUFF THAT YOU ARE ANALYZING.
3	SO GO BACK TO MY PROGRESSIVE COMPANY EXAMPLE.
4	IF IT TURNED OUT THAT PROGRESSIVE COMPANIES WERE NO MORE
5	OR LESS LIKELY TO PUT WOMEN ON BOARDS, WELL, THEN
6	LEAVING OUT THIS PROGRESSIVISM VARIABLE WOULDN'T
7	GENERATE A BIAS FOR OUR BOARD METRIC, RIGHT.
8	YOU NEED TWO CONDITIONS FOR THIS OMITTED
9	VARIABLE BIAS ISSUE. ONE IS THE OMITTED VARIABLE HAS TO
10	MATTER FOR THE OUTCOME. AND THE SECOND ONE THEY'RE
11	BOTH NECESSARY CONDITIONS THE SECOND CONDITION IS THE
12	OMITTED VARIABLE HAS TO BE CORRELATED WITH THE VARIABLES
13	THAT YOU ARE INCLUDING IN YOUR REGRESSION, BECAUSE
14	INTUITIVELY WHAT HAPPENS IS THE REGRESSION CAN'T TELL
15	THE DIFFERENCE. IS THIS BECAUSE OF THE VARIABLE OR
16	BECAUSE OF THE THING THAT'S LEFT OUT BUT IT'S CORRELATED
17	WITH THE VARIABLE. THAT'S THE PROBLEM.
18	SO NOW GO BACK TO MERCK'S DRUG TRIAL. MERCK
19	SHOWS THE VARIABLE OF INTEREST, DO YOU GET THE MEDICINE
20	OR DO YOU GET THE PLACEBO, BY FLIPPING A COIN, RIGHT.
21	THEY RANDOMIZE IT. THAT MEANS NOBODY ON ANY
22	PARTICULAR CHARACTERISTIC IS NO MORE OR LESS LIKELY TO
23	BE IN THE TREATMENT GROUP.
24	SO WHAT DOES THAT MEAN? THAT MEANS IF WE DON'T
25	ADJUST FOR VARIOUS THINGS, IT DOESN'T MATTER, BECAUSE
26	THOSE THINGS THAT WE'RE LEAVING OUT OF THE ANALYSIS OR
27	THE REGRESSION ARE NOT GOING TO BE CORRELATED WITH THE
28	TREATMENT VARIABLE; I.E., DID YOU GET THE MEDICINE.

1 THAT'S THE FIRST PART OF GENIUS OF AN 2 EXPERIMENT. 3 RANDOMIZATION ESSENTIALLY SOLVES THIS OMITTED VARIABLES BIAS PROBLEM. 4 5 THE SECOND GENIUS PART OF THE EXPERIENCE IS 6 THIS SO-CALLED CONTROL GROUP OR COMPARISON GROUP, RIGHT. 7 IT'S A NECESSARY COMPONENT OF AN EXPERIMENT. IMAGINE WE RAN THE EXPERIMENT, BUT WE DIDN'T HAVE A CONTROL GROUP. 8 9 WE JUST GAVE EVERYBODY THE MEDICINE. 10 SO WE GIVE ALL 1,000 PEOPLE THE MEDICINE? WE 11 CHECK BACK IN IN FOUR WEEKS AND WE SAY, HALLELUJAH, 12 100 PERCENT OF PEOPLE ARE CURED OF THEIR COLD. PERFECT EFFICACY OF THIS DRUG. 13 WELL, INTUITIVELY THAT'S PROBLEMATIC BECAUSE 14 15 YOU SAY I SUSPECT SOME OF THOSE PEOPLE WOULD HAVE GOTTEN 16 BETTER INDEPENDENTLY OF -- YOU KNOW, LOTS OF TIMES WE DON'T TAKE DRUGS AND THE COLD GETS BETTER. 17 18 WHAT WE REALLY WANT TO DO IS WE WANT TO SEE THE 19 OUTCOME VERSUS ITS COUNTERFACTUAL. WE CAN'T DO THAT. 20 WE CAN'T OBSERVE YOU WITH THE MEDICINE AND THEN REWIND THE TAPE AND THEN OBSERVE YOU WITHOUT THE MEDICINE. 21 22 SO WHAT THE CONTROL OR COMPARISON GROUP DOES IS 23 IT SERVES AS A PROXY FOR THE COUNTERFACTUAL. SO IF I GO 24 AND I LOOK AT 100 PERCENT OF THE PEOPLE WHO GOT THE 25 DRUG, THEIR COLD WENT AWAY, AND THEN I GO LOOK IN THE CONTROL GROUP, AND IF 100 PERCENT OF THE PEOPLE IN THAT 26 27 GROUP GOT BETTER TOO, WE CAN'T SAY ANYTHING ABOUT THE 28 EFFICACY OF THE DRUG.

1 IF INSTEAD IN THE CONTROL GROUP ONLY 80 PERCENT 2 OF PEOPLE GOT BETTER, WE WOULD SAY, OKAY, THIS IS 3 EVIDENCE OF A CAUSAL EFFECT OF THE DRUG ON THE COLD 4 OUTCOME. 5 O. IS THAT THE SAME THING AS SAYING THAT BY ADDING 6 WOMEN ON BOARDS AND JUST LOOKING AT ADDING WOMEN ON ALL 7 THE BOARDS OF THE CALIFORNIA HEADOUARTERED COMPANIES THAT ARE SUBJECT TO THIS LAW, PERFORMANCE GOES UP, WELL 8 9 IT MUST BE DUE TO SB-826, THE LAW? 10 A. YEAH, THAT'S --11 O. IS THAT THE SAME THING AS THE THOUSAND OR 500? 12 13 YEAH. THAT'S THE FUNCTIONAL EQUIVALENT OF Α. DECLARING THAT IF NOBODY HAS A COLD AFTER SIX WEEKS IT 14 15 MUST BE BECAUSE OF THE MEDICINE. 16 MR. STICHT: YOUR HONOR, IT'S 3:00. IT'S A 17 GOOD TIME TO BREAK. THE COURT: I AM ENTHRALLED. THE TIME JUST 18 19 PASSED SO QUICKLY. 20 I'D LIKE TO THANK YOU. WE'LL TAKE A SHORT 21 BREAK. 22 THE QUESTION I HAVE IS WE'RE PLANNING TO FINISH 23 THIS WITNESS TODAY OR NO? 24 MR. STICHT: NO. 25 THE COURT: OKAY. WE'RE FINE. FIFTEEN 26 MINUTES. THANK YOU VERY MUCH. 27 28 (COURT IS IN RECESS.)

1 2 THE COURT: WE'RE BACK ON THE RECORD IN THE 3 MATTER THAT WE'RE CURRENTLY IN TRIAL ON. I'LL NOTE FOR THE RECORD THAT THE WITNESS 4 5 KLICK -- ARE YOU CALLED DR. KLICK OR ARE YOU CALLED MR. KLICK OR ARE YOU CALLED COUNSEL KLICK? 6 7 THE WITNESS: YOU CAN CALL ME JOHN OR YOU CAN 8 CALL ME PROFESSOR KLICK OR DR. KLICK. 9 THE COURT: OKAY, PROFESSOR KLICK. 10 PROFESSOR KLICK, THANK YOU VERY MUCH. 11 AND JUST FOR THE RECORD, I WANT TO MAKE A 12 RECORD TO SHOW THAT THE COURT HAS ADDRESSED AN ISSUE REGARDING THE WEARING OF A MASK BY A WITNESS AND THE 13 14 COURT HAS GRANTED THE RIGHT NOT TO WEAR A MASK DURING 15 THE TIME. THE COURT WOULD LIKE TO ALSO NOTE THAT THE 16 COURT AS THE JURY INSTRUCTION INDICATES, HAS A RIGHT AND A NEED FROM TIME TO TIME, TO OBSERVE THE TONE, TENOR AND 17 18 DEMEANOR OF A WITNESS' TESTIMONY AND IN THIS REGARD THIS 19 IS SUCH TECHNICAL TESTIMONY AND -- AND QUITE RELEVANT 20 BASED ON THE DEFENSE IN THIS MATTER THAT THE COURT WANTS 21 TO OBSERVE THE TONE 10:00 OR AND DEMEANOR OF THIS 22 WITNESS, I CANNOT DO THAT WITH A MASK ON. 23 THE COURTROOM IS BASICALLY EMPTY. WE CAN HAVE 24 ACCOMMODATE '69 PEOPLE IN THIS COURTROOM. THERE IS ONLY 25 ONE, TWO, THREE, FOUR, FIVE, SIX, SEVEN, EIGHT, NINE, 10, 1112, 13, 14, 15 PEOPLE, INCLUDING ALL THE COURT 26 27 STAFF IN THE COURTROOM, INCLUDING THE WITNESS. 28 I'VE GIVEN PERMISSION FOR EVERYONE TO MOVE BACK

1 TO ANYWHERE THEY'D LIKE WHERE IT MAKES THEM FEEL
2 COMFORTABLE WITH THEIR MASKS ON AND EVERYONE UNDERSTANDS
3 THAT THE MASKS WILL BE UP UNLESS THEY'RE DRINKING
4 SOMETHING AND IN THIS REGARD ANYONE WHO HAS WATER OR
5 ANYTHING THAT THEY NEED TO DRINK AS LONG AS IT HAS A LID
6 ON IT OR, YOU KNOW WE'RE NOT DOING STRAWS, THEY CAN PULL
7 THEY'RE MASK ON AND DRINK OBVIOUSLY.

8 BUT I'D LIKE THE RECORD REFLECT THAT WE HAVE 9 DISCUSSED THIS REQUEST BY COUNSEL -- A COUPLE OF COUNSEL 10 ON THE DEFENSE. AND I HAVE NOW ADDRESSED IT. ALSO, AT 11 ANY GIVEN TIME IF THIS IS NOT A LAWYER WHO IS HANDLING A 12 PARTICULAR WITNESS, I AM NOT AT ALL OFFENDED IF SOMEONE 13 CHOOSES TO STEP OUTSIDE FOR THE CONCLUSION OF THIS 14 PARTICULAR WITNESS AND THEN REVIEW THE TRANSCRIPT.

AS TO ANY PERIOD OF TIME THAT THEY'VE MISSED IF16 THEY'RE JUST ON THE DEFENSE TEAM.

17 I ALSO WILL NOTE THAT I'VE ORDERED DAILIES SO
18 THAT ALL THE PARTIES -- OR I'M GETTING DAILIES SO ALL
19 THE PARTIES HAVE ACCESS TO THE DAILY TRANSCRIPTS.

20 OKAY. THANK YOU VERY MUCH AND LET'S PROCEED.

21

22

28

DIRECT EXAMINATION (CONTINUED)

23 BY MR. SEFERIAN:

Q. PROFESSOR KLICK BEFORE THE BREAK WE WERE
TALKING ABOUT THE METHODOLOGIES OR STATISTICAL ANALYSES
THAT OCCUR, WE TALKED ABOUT DESCRIPTIVE STATISTICS, WE
TALKED ABOUT REGRESSION.

I'M NOT SURE WE REALLY FINISHED WITH

1 REGRESSION, BUT WE LEFT OFF BASICALLY TRYING TO ACCOUNT 2 FOR VARIABLES AND WHY DON'T YOU PICK UP FROM WHERE YOU 3 LEFT OFF AND TELL US WHAT YOU'D LIKE -- TELL US ABOUT 4 THOSE UNOBSERVABLE EVENTS AND HOW YOU ACCOUNT FOR THOSE? 5 Α. SURE. SO AS WE LEFT OFF, IN MANY HARD SCIENCE MEDICAL 6 7 FIELDS DRUG TRIALS, THINGS OF THAT NATURE, WE CAN SORT 8 OF SIDESTEP THIS PROBLEM BY RUNNING RANDOMIZED 9 EXPERIMENTS, RANDOMIZED TRIALS. AND JUST TO SUMMARIZE FROM BEFORE: THE REASON OMITTED VARIABLE BIAS IS NOT AN 10 11 ISSUE THERE IS BECAUSE THE TREATMENT OF ISSUE, DO YOU 12 GET THE DRUG OR DO YOU NOT IS ASSIGNED RANDOMLY. 13 ONE DEFINITION OF RANDOM ASSIGNMENT MEANS THERE'S NO CORRELATION BETWEEN THE ASSIGNMENT OF THE 14 15 DRUG AND ANY OTHER CHARACTERISTIC THAT A PERSON COULD 16 HAVE. YOU COULD IMAGINE IF WE VIOLATED RANDOM 17 18 ASSIGNMENTS SO IN THAT HYPOTHETICAL SITUATION I SPOKE OF 19 WE GET 1,000 PEOPLE WITH COLDS. IMAGINE WE DON'T ASSIGN 20 THE DRUG RANDOMLY, WE GIVE THE DRUG TO ALL THE WOMEN, WE DON'T GIVE IT TO THE MEN, RIGHT, THAT OBVIOUSLY WOULDN'T 21 22 BE A RANDOM ASSIGNMENT AND ANY DIFFERENCE WE WOULD SEE 23 BETWEEN THE TREATMENT AND CONTROL GROUP WE WOULD WONDER 24 IS IT BECAUSE THEY GO THE DRUG OR IS IT BECAUSE OF THE 25 SEX DIFFERENCE. 26 SO IN THAT INSTANCE, THAT WOULD BE A FAILURE OF 27 THAT EXPERIMENT. SO WE DO THE RANDOMIZATION SO WE DON'T 28 HAVE TO WORRY THAT ANY OF OUR CHARACTERISTICS, INCLUDING

1 THE UNOBSERVABLE CHARACTERISTICS ARE CORRELATED WITH THE 2 DRUG ASSIGNMENT. AND THAT'S HOW WE SIDESTEP THE OMITTED VARIABLE BIAS ISSUE. SO YOU MIGHT ASK YOURSELF WELL 3 4 GEE, IF WE KNOW HOW TO DO THIS, WHY DON'T WE RUN 5 EXPERIMENTS FOR EVERYTHING. AND IN ECONOMICS WE HAVE STARTED RUNNING SOME FIELD EXPERIMENTS FOR SOME ISSUES 6 7 BUT THE PROBLEM IN SAY A LEGAL OR A POLICY SETTING IS RUNNING THE EXPERIMENT, YOU KNOW, IT MIGHT BE JUST NOT 8 9 FEASIBLE. YOU COULD IMAGINE THAT THE LEGISLATURE PASSES THE LAW AND SAYS, ONLY IT'S ONLY GOING TO APPLY TO SOME 10 11 COMPANIES AND TO DETERMINE WHETHER IT APPLIES TO YOU WE'RE GOING TO FLIP A COIN. THAT WOULD OBVIOUSLY INVITE 12 ALL SORTS OF CONSTITUTIONAL CHALLENGES AND 13 14 ADMINISTRATIVE LAW CHALLENGES AND THINGS OF THAT NATURE. 15 SO WE JUST CAN'T RUN WHAT WE WOULD LIKE TO DO, METHODOLOGICALLY WE CAN'T RUN IT FOR THIS WHOLE SET OF 16 POLICY OR LEGAL QUESTIONS AND MANY ECONOMICS QUESTIONS 17 AS WELL. 18 19 SO WE'RE OUT OF LUCK THERE. WE CAN'T DO THE 20 THING THAT WE WOULD LIKE TO THAT WOULD GIVE US THE MOST CONFIDENCE IN MAKING A CAUSAL INFERENCE ABOUT THE EFFECT 21 22 OF SAY A LAW OR THE EFFECT IN THIS CASE OF A PARTICULAR 23 ASPECT OF CORPORATE GOVERNANCE. 24 SO ARE WE LEFT WITHOUT SOMETHING TO DO? Q. 25 LUCKILY NOT, OTHERWISE I'D BE UNEMPLOYED FOR Α. 26 THE MOST PART, I GUESS. 27 NO, STARTING IN SORT OF THE MID 1990'S, LATE 28 1990'S, SOCIAL SCIENTISTS, MANY ECONOMICS BUT ALSO

1	SOCIAL SCIENTISTS FROM SOME OTHER AREAS, FIGURED OUT
2	THAT ONE COULD ESSENTIALLY LEVERAGE THE EXPERIMENTAL
3	INTUITION IN THE REAL WORLD. NOW YOU COULDN'T YOU
4	CAN'T YOU CAN'T ACTUALLY MANIPULATE WHAT RULE OR WHAT
5	CORPORATE GOVERNANCE MECHANISM APPLIES TO A PARTICULAR
6	ENTITY IN THE REAL WORLD BUT WHAT PEOPLE STARTED
7	RECOGNIZING IS SOMETIMES THE WORLD ITSELF GIVES US THESE
8	EXPERIMENTS, RIGHT. AND SO THIS IS BY LIKE I SAID,
9	THE LATE 1990'S, MANY SOCIAL SCIENTISTS STARTED FOCUSING
10	ON THESE, THEY CALLED THEM QUASI RANDOM DESIGNS OR
11	NATURAL EXPERIMENTAL DESIGNS. AND THAT'S REALLY BEEN
12	MOST OF MY CAREER AND IT'S BEEN IT'S BEEN SORT OF MUCH
13	OF PUBLIC POLICY ANALYSIS SINCE, YOU KNOW IN THE PAST 20
14	YEARS.
15	Q. OKAY, SO BEFORE WE GO TOO FAR DOWN THAT PATH,
16	TO USE YOUR MERCK EXAMPLE, COMPANY MERCK?
17	A. SURE.
18	Q. AND THE THOUSAND DIVIDED UP AMONG A RANDOMIZED
19	PLACEBO VERSUS THE DRUG?
20	A. RIGHT.
21	Q. IS THERE AN ANALOGY YOU CAN DRAW THAT IS, AT
22	LEAST FOR PURPOSES OF THE LESSON WE'RE GETTING FROM YOU,
23	TO THE CORPORATE CONTEXT IN WHICH WE'RE FINDING
24	OURSELVES WITH THE WOMEN ON BOARDS ISSUE?
25	A. SO IF WE WERE ABLE TO RUN AN EXPERIMENT, THEY
26	WOULD CALL IT AN IF WE'RE RUNNING AN EXPERIMENT IN
27	THE FIELD, WE WOULD CALL IT A FIELD EXPERIMENT. IT
28	WOULD ESSENTIALLY BE WE WOULD TAKE THE RELEVANT

UNIVERSE OF COMPANIES, WE WOULD RANDOMIZE AND SAY, OKAY,
 50 PERCENT OF THE COMPANIES WE'RE GOING TO INJECT THREE
 EXTRA WOMEN ON THE BOARD.

AND THE OTHER COMPANIES WE WOULD KEEP -- WE 4 5 WOULD KEEP CONSTANT AND THEN ESSENTIALLY FOLLOW THE MERCK INTUITION. NOW THERE WOULD STILL BE A PROBLEM, 6 7 EVEN IF WE SET ASIDE ALL THE LEGAL AND POTENTIALLY ETHICAL AND CERTAINLY FUNDING PROBLEMS WITH SUCH A FIELD 8 9 EXPERIMENT, THE PROBLEM THERE IS WHAT WOULD BE THE 10 EQUIVALENT OF A PLACEBO, RIGHT OF COURSE -- OF COURSE 11 THE COMPANIES THAT DIDN'T GET THE INJECTION OF WOMEN TO 12 THEIR BOARD ARE A COMPARATOR BUT WE MIGHT WONDER, WE MIGHT WORRY IS IT THE CASE THAT THE TWO GROUPS, BECAUSE 13 14 THEY KNOW WELL, I'M A TREATMENT OR I'M A CONTROL, THEY 15 MIGHT ACT DIFFERENTLY THAN THEY WOULD IN THE ACTUAL 16 WORLD. Q. BUT THAT'S NOT THE CASE IN THE MEDICAL 17 18 EXPERIMENT, RIGHT THEY DON'T KNOW? 19 A. RIGHT THEY'RE BLINDED. 20 Q. RIGHT. THESE ARE BLIND STUDIES AND JUST FOR CLARIFICATION OF YOUR RECORD, IS THE CONTROL GROUP THE 21 22 COUNTER FACTUAL THAT YOU REFERRED TO? 23 Α. YEAH. IT'S WHAT WE'RE USING AS THE COUNTER 24 FACTUAL. 25 Q. SO PLEASE CONTINUE ABOUT THE WOMEN ON BOARDS 26 CONTEXT? 27 A. SURE. SO IT WOULD BE DIFFICULT BECAUSE

28 ALTHOUGH WE WOULD HAVE A COMPARISON GROUP WE MIGHT WORRY

1 THAT FIRMS IN THAT INSTANCE MIGHT ACT DIFFERENTLY 2 BECAUSE THEY KNOW WHAT'S BEEN STUDIED, RIGHT? SO 3 IMAGINE YOU HAD SOME PROGRESSIVE FIRMS THAT REALLY 4 THOUGHT THERE SHOULD BE MORE WOMEN ON THE BOARD AND IF 5 THEY KNOW THEY'RE IN THE PLACEBO GROUP, YOU KNOW, YOU COULD IMAGINE MAYBE THEY SHIRK TO MAKE THEMSELVES LOOK 6 7 WORSE TO ESSENTIALLY FIX THE -- YOU KNOW, FIX THE 8 RESULTS IN A PARTICULAR WAY. OR IF YOU THOUGHT THAT YOU 9 DIDN'T QUANTITY WOMEN ON THE GROUP AND YOU WERE IN THE -- YOU WERE IN THE TREATMENT GROUP YOU SHIRKED IN 10 11 THAT INSTANCE.

THAT WOULD BE A REAL WORRY. AND THAT'S ONE OF 12 THE REASONS WHY WE DO BLIND PARTICIPANTS IN THE MEDICAL 13 14 TRIALS. WE DON'T -- WE DON'T JUST NOT GIVE YOU A MEDICINE IF YOU'RE IN THE CONTROL GROUP OR THE 15 16 COMPARISON GROUP. WE GIVE YOU THE SUGAR PILL THAT LOOKS IDENTICAL SO THAT WE'RE NOT WORRIED THAT OH, YOU'RE 17 18 GOING TO ACT DIFFERENTLY OR YOU'RE GOING TO HAVE A --19 YOU KNOW, REASON TO, YOU KNOW, DO THINGS DIFFERENTLY 20 THAN YOU WOULD IN THE WORLD WHERE YOU DIDN'T KNOW.

SO THAT WOULD BE HARD TO DUPLICATE IN A FIELDEXPERIMENT.

Q. ARE YOU SUGGESTING THOUGH THAT JUST BY THE
SHEAR DEFINITION OF THE PROBLEM, THE CORPORATE CONTEXT
ADDING WOMEN TO THE BOARDS, IT WOULD BE UNFEASIBLE TO
USE THAT KIND OF AN EXPERIMENTAL FIELD STUDY?
A. FOR ALL SORTS OF REASONS, YES.
Q. I CAN IMAGINE CORPORATIONS, YOU'RE COMING IN

1	AND TELLING THEM WOULD YOU PARTICIPATE IN A BLIND STUDY.
2	CHANGE YOUR BOARD.
3	A. RIGHT.
4	Q. RIGHT. WE CAN ALL CONJURE UP THE POTENTIAL
5	PROBLEMS THAT MIGHT OCCUR WITH THAT KIND OF A REQUEST?
6	A. WELL, IT MIGHT EVEN BE A VIOLATION OF A
7	FIDUCIARY DUTY IF A IF A COMPANY AGREED TO DO SO.
8	Q. SO WHAT DO WE DO WHEN WE DON'T HAVE THE FIELD
9	EXPERIMENT TO SORT OF ACCOUNT FOR THESE UNOBSERVABLE
10	EVENTS?
11	A. SO AGAIN, WE STARTING IN THE MID 1990'S THERE
12	WAS THIS BIG FOCUS ON TRYING TO FIND THESE NATURAL
13	EXPERIMENTS.
14	THE QUASI RANDOMIZATION THAT HAPPENS OUT IN THE
15	WORLD, IT'S A REAL WORLD SITUATION, SOMETHING HAPPENS
16	THAT LEADS TO ONE GROUP BEING AFFECTED, THE OTHER GROUP
17	NOT BEING AFFECTED AND YOU ESSENTIALLY TREAT IT AS IF
18	IT'S AN EXPERIMENT.
19	NOW THERE'S A LOT THAT NEEDS TO BE DONE AND WE
20	CAN TALK ABOUT THERE, BUT THAT'S THE BASIC INTUITION
21	Q. IS THAT THE HERSHEY EXAMPLE THAT YOU WERE
22	REFERRING TO?
23	A. SO THAT WOULD BE AN EXAMPLE OF A NATURAL
24	EXPERIMENT, RIGHT. SO IF WE TRY TO TURN ON AN ACTUAL
25	FIELD EXPERIMENT, YOU GO TO A BUNCH OF COMPANIES AND
26	SAY, OKAY, WE'RE GOING TO RANDOMIZE YOU BETWEEN BEING
27	EXPOSED TO THE MARKET FOR CORPORATE CONTROL AND NOT,
28	THAT JUST WOULDN'T BE POSSIBLE. BUT BECAUSE OF HERSHEYS

1 IDIOSYNCRATIC BACKGROUND AND BECAUSE OF THIS 2 FUNCTIONALLY RANDOM DECISION BY THE STATE ATTORNEY 3 GENERALS OFFICE TO COME IN AND TELL HERSHEY NO YOU'VE GOT TO SWITCH, IT WASN'T AS IF HERSHEY ITSELF SAID, OH, 4 5 FOR FIRM PERFORMANCE REASONS WE'RE GOING TO SWITCH. NO 6 IT WAS ESSENTIALLY IMPOSED UPON THEM ALMOST LIKE A 7 RESEARCHER DECIDING A AT A RANDOM TIME BOOM WE'RE GOING 8 TO CHANGE YOU'RE TREATMENT ESSENTIALLY. THAT'S THE 9 INTUITION. 10 Q. OKAY. 11 SO PLEASE CONTINUE A LITTLE MORE ON THIS 12 NATURAL EXPERIMENT. 13 Α. SURE. SO OFTEN TIMES, I CAN TALK ABOUT IT IN GENERAL 14 15 AND I CAN GIVE SOME EXAMPLES AND MAYBE IT'S BETTER TO 16 START WITH SOME EXAMPLES SO THAT WE'VE GOT SOME CONCRETE THINGS IN MIND. 17 18 AND I'LL PULL AWAY FROM THE CORPORATE CONTEXT 19 JUST SO THAT WE CAN SO IT DOESN'T CONFUSE ANY OF OUR 20 THOUGHTS ON ANYTHING ELSE. 21 SO I'VE DONE SOME CRIME WORK FOR EXAMPLE AND 22 PROBABLY MY MOST IMPACTFUL PAPER IS A PAPER THAT WE DID 23 LOOKING AT THE EFFECT OF POLICE ON CRIME. AND YOU KNOW 24 THE PROBLEM THAT YOU WOULD HAVE HERE THERE HAVE BEEN

26 DESCRIPTIVE STATISTICS AND TRY TO SAY, WELL LOOK, YOU

LOTS OF ARTICLES THAT WOULD RUN A REGRESSION OR EVEN DO

27 KNOW IF WE REGRESS CRIME OUTCOMES ON SOME MEASURE OF

25

28 POLICE AND THEN WE TRY TO CONTROL FOR A BUNCH OF OTHER

STUFF, YOU KNOW, HAVE WE GOTTEN TO CAUSALITY, CAN WE BE
 CONFIDENT OF THAT CAUSALITY AND OF COURSE THE PROBLEM IS
 IS THAT POLICE AREN'T RANDOMLY ASSIGNED TO
 JURISDICTIONS, THE CITIES.
 Q. NOT TO INTERRUPT YOU, BUT JUST TO SET THE STAGE

A LITTLE CLEARER, WHEN YOU SAY THE EFFECT OF POLICE ON
CRIME, ARE YOU SUGGESTING THAT A MODEL OF DO MORE POLICE
LEAD TO LOWER CRIME?

9 A. RIGHT. SO IF WE HAVE A GENERAL DETERRENCE
10 MODEL WHERE WE THOUGHT HAVING MORE POLICE DETERRED CRIME
11 AND SO THE HYPOTHESIS, GETTING TO YOUR AT THE VERY
12 BEGINNING OF OUR DISCUSSION, MY HYPOTHESIS MIGHT BE FOR
13 POLICE LEADS TO LESS CRIME.

14 AND THE ALTERNATIVE HYPOTHESIS MIGHT BE MORE15 POLICE HAS NO EFFECT ON CRIME.

SO THOSE ARE MY TWO HYPOTHESES.

16

YOU CAN IMAGINE TRYING TO JUST RUN REGRESSIONS 17 18 ON THIS. YOU WOULD GET CRIME OUTCOME DATA FROM BUREAU 19 OF JUSTICE STATISTICS OR SOMETHING LIKE THAT. AND YOU 20 WOULD GET REGRESSIVE ON SOME MEASURE OF POLICE PER CAPITA IN A JURISDICTION BUT YOU MIGHT SAY, WELL IT'S 21 22 NOT JUST POLICE RIGHT THERE ARE OTHER THINGS THAT AFFECT 23 CRIME THAT ARE DIFFERENT SO WE MIGHT WANT TO CONTROL FOR 24 SOME DEMOGRAPHIC FACTORS WE MIGHT WANT TO CONTROL FOR 25 ECONOMIC FACTORS WE MIGHT WANT TO CONTROL FOR BACKGROUND 26 TIME TRENDS RIGHT. WE KNOW THAT CRIME DECLINED IN THE 27 90'S. WE KNOW THAT CRIME SORT OF STARTED PICKING UP A 28 LITTLE BIT NOW. AND THAT SEEMS TO BE FAIRLY GENERIC

ACROSS THE COUNTRY. SO IT'S PROBABLY NOT RELATED TO 1 2 POLICING LEVELS, SO WE WOULD LIKE TO ADJUST FOR THAT IF 3 WE'RE LOOKING AT DATA OVER TIME OR SOMETHING LIKE THAT. 4 THESE PAPERS RUN INTO THE PROBLEM WE TALKED 5 ABOUT EARLIER THOUGH. WHAT IF THERE ARE UNOBSERVABLE CHARACTERISTICS. 6 7 WHAT IF -- WHAT IF THERE'S JUST A BACKGROUND, YOU KNOW, 8 CONFORMITY TO LAW AND ORDER. SOME JURISDICTIONS, YOU 9 HAVE MORE OF IT, SOME JURISDICTIONS YOU HAVE LESS OF IT. CONCEPTUALLY THINK ABOUT WHAT THAT MEANS BUT HOW WOULD 10 11 YOU ADJUST IF THERE'S NO DATA ON THAT. 12 FURTHERMORE YOU MIGHT WORRY, WELL JURISDICTIONS THAT ARE SORT OF MORE OF A CONFORMITY TO LAW AND ORDER 13 14 MIGHT POLITY COLLIE SUPPORT HIRING MORE POLICE SO THEN 15 YOU'VE GOT A CORRELATION BETWEEN HOW MANY POLICE YOU HAVE AND THIS BACKGROUND GENERAL PROPENSITY THAT YOU 16 CAN'T ADJUST FOR SO THE REGRESSION APPROACH IN THIS 17 LITERATURE IS JUST NOT YES VERY CREDIBLE. IT'S NOT JUST 18 19 VERY RELIABLE. AND SO WHAT WE DID IN ONE OF OUR PAPERS 20 IS WE LOOKED AT THE PERIOD IN WASHINGTON D.C. SOON AFTER THE 2001 TERROR ATTACKS. SO IF YOU RECALL BACK IN 2001 21 22 HOMELAND SECURITY CREATED THIS TERROR ALERT SYSTEM WHERE 23 IT WAS BLUE AND GREEN AND YELLOW AND ORANGE AND ALL 24 THOSE SORTS OF THINGS AND THE DETERMINATION OF THE 25 TERROR ALERT LEVEL WAS MADE BY HOMELAND SECURITY BASED 26 ON INTELLIGENCE INFORMATION. 27 DID WE HEAR ANY CHATTER ON THE INTERNET. DID 28 WE GET SORT OF REPORTS FROM THE C.I.A. IT HAD NOTHING

1 TO DO WITH CRIME IN WASHINGTON D C, RIGHT. HOWEVER 2 SERENDIPITY, WASHINGTON D.C. LIKE MANY CITIES IN THIS 3 TIME PERIOD DECIDED THAT WHEN THERE WERE TERROR CONCERNS 4 THEY WANTED TO HAVE MORE FIRST RESPONDERS ON THE STREET 5 SO ESSENTIALLY THEY TOLD ALL THE POLICE IN WASHINGTON WHEN THE TERROR ALERT GOES FROM YELLOW TO ORANGE, WHEN 6 7 IT GETS RAISED, EVERYONE HAS TO WORK AN EXTRA FOUR 8 HOURS. AND FURTHERMORE YOU HAVE TO WORK THOSE EXTRA 9 FOUR HOURS IN DISTRICT ONE OF THE CITY, RIGHT. SO DISTRICT ONE IN WASHINGTON IS WHERE THE WHITE HOUSE IS 10 11 AND THE CAPITAL BUILDING, THINGS LIKE THAT.

12 SO THINK ABOUT WHAT THIS DOES. YOU HAVE WEIRD 13 TERRORISM CONCERNS BASICALLY FLIPPING A SWITCH THAT THEN 14 HAS AN IMPACT ON RACING THE NUMBER OF POLICE IN ONE AREA 15 OF THE CITY BUT LEAVING IT UNCHANGED IN OTHER AREAS OF 16 THE CITY.

AND SO WE USE THAT AS OUR NATURAL EXPERIMENT. 17 18 WE'RE ABLE TO LOOK AT EVERY TIME THE TERROR ALERT SWITCH 19 ED WHAT HAPPENED TO CRIME IN DISTRICT ONE BEFORE AND 20 AFTER THE SWITCH OCCURRED, THAT'S THE TREATMENT ANALOGUE, RIGHT SO THIS IS LIKE THE TREATMENT GROUP IN 21 22 THE MERCK TRIAL GETTING THE DRUGS OR COMPARING THEIR 23 OUTCOME IN THIS CASE CRIME BEFORE AND AFTER. BUT THEN 24 WE ALSO NEED THE COUNTER FACTUAL WE ALSO NEED THE 25 COMPARATOR. AND SO WE WERE ABLE TO USE THE OTHER AREAS 26 THAT THE CITY THE COMPARATOR, SO LOOK THOSE GUYS, THOSE 27 PARTS OF THE CITY WERE NOT EXPERIENCES ANY CHANGE IN 28 POLICING AND SO WE SHOULDN'T FIND ANY POLICING EFFECT ON

1	CRIME IN THAT AREA. IF IT TURNS OUT IN THE REST OF THE
2	CITY IF WE SEE THE SAME CHANGE IN CRIME AS WE SAW IN
3	DISTRICT ONE WE'D HAVE TO SAY IT PROBABLY ISN'T BECAUSE
4	OF THE POLICE. IT MUST BE SOMETHING ELSE. SOME OTHER
5	UNOBSERVABLE.
6	BUT IF INSTEAD WE SEE A BIG CHANGE IN DISTRICT
7	ONE AND WE DON'T SEE IT IN THE REST OF THE CITY, WELL
8	THAT'S LIKE COMPARING THE TREATMENT AND CONTROL GROUP IN
9	THE MERCK TRIAL AND SO THAT'S AN EXAMPLE OF A NATURAL
10	EXPERIMENT.
11	Q. IN THAT EXAMPLE, THE PRESUMABLY ALREADY DONE A
12	DESCRIPTIVE AND REGRESSION ABOUT A YOU EVEN LOOK AT THAT
13	NATURAL EVENT, RIGHT?
14	A. SURE.
15	Q. AND THE NATURAL EVENT ARE YOU SAYING THEN,
16	SERVES AS THE BLIND EXPERIMENT?
17	A. THE RANDOMIZATION, THAT'S RIGHT.
18	Q. BECAUSE DISTRICT ONE DOESN'T KNOW NECESSARILY
19	THAT I DIDN'T KNOW UNTIL YOU TOLD US, THAT THEY
20	INCREASED THE POLICE PRESENCE IN DISTRICT ONE WHEN WE
21	WENT TO YELLOW AND ORANGE?
22	A. RIGHT.
23	Q. OR A PARTICULAR COLOR?
24	A. RIGHT.
25	Q. AND THEN YOU WOULD ADJUST, WOULD YOU NOT FOR IF
26	THEY HAD TO CALL POLICE FROM DISTRICT THREE TO ASSIST IN
27	DISTRICT ONE YOU'D HAVE TO ADJUST FOR THAT?
28	A. YOU WOULD HAVE TO ADJUST FOR IT AND YOU MIGHT

I

1 EVEN SAY WELL THEN THAT SO CONTAMINATES THE CONTROL 2 GROUP THAT THIS IS JUST NOT A NATURAL EXPERIMENT. SO 3 LUCKILY WHAT WE FOUND IS THERE WAS NO CHANGE IN POLICING 4 IN THE REST OF THE CITY. SO THAT WAS IMPORTANT TO US TO 5 SERVE AS SORT OF A VALID COMPARISON GROUP OR CONTROL GROUP. EVERYBODY JUST HAD TO WORK EXTRA TIME IT'S NOT 6 7 AS THOUGH THEY PULLED PEOPLE FROM SOUTHEAST AND MOVED 8 THEM TO DISTRICT ONE THEY TOLD ALL THE SOUTHEAST COPS 9 WORK YOUR NORMAL EIGHT HOUR SHIFT AND THEN COME AND WORK THE EXTRA FOUR HOURS ON THE MALL FOR EXAMPLE OR 10 11 SOMETHING LIKE THAT. 12 Q. SO WHAT HAPPENED, WHAT DID YOU FIND OR HOW DID IT WORK OUT? 13 WE FOUND A BIG DECREASE IN CRIME SO POLICE 14 Α. 15 APPEARED TO HAVE A BIG DETERRENT EFFECT AT LEAST ON PROPERTY CRIMES, SO WE SAW BIG DECLINE IN BURGLARIES, 16 BIG DECLINE IN AUTOMOBILE THEFT, WE DIDN'T FIND MUCH OF 17 A CHANGE FOR MURDERS OR RAPES. 18 19 Q. ONCE AGAIN, WHAT WAS THE HYPOTHESIS GOING IN? 20 THE HYPOTHESIS IS THAT MORE POLICE LEAD TO LESS Α. 21 CRIME. 22 WHICH A LOT OF PEOPLE BELIEVE, RIGHT? Ο. 23 BUT OTHER PEOPLE DO NOT. SO IT'S A Α. 24 CONTESTED -- YOU CAN COME UP WITH THEORIES SORT OF 25 SUPPORTING BOTH BEYOND THAT, YOU CAN CERTAINLY HAVE LOTS 26 OF PEOPLE HAVE DIFFERENT INTUITIONS THERE, THAT'S WHY 27 BRINGING THE DATA TO THE QUESTION IS IMPORTANT: BEYOND 28 THAT, THOUGH, EVEN IF YOU SAID, WELL, KLICK, KIND OF

1	EVERYBODY BELIEVES THIS ALREADY WHAT'S THE POINT OF IT,
2	WELL IT ALLOWED US TO GET AN ACTUAL METRIC FOR HOW MUCH,
3	RIGHT. SO IF POLICE LEADS TO A LITTLE BIT DECLINE IN
4	CRIME BUT COST A LOT OF MONEY THAT MIGHT NOT BE WORTH
5	IT. AND SO YOU NEED TO KNOW HOW MUCH OF DECLINE IN
6	CRIME THERE IS TO DO SORT OF A WELFARE ANALYSIS.
7	Q. RIGHT.
8	DID YOU DO THAT STUDY AS A CONSULTANT OR A
9	PROFESSOR?
10	A. JUST AS A PROFESSOR.
11	Q. SO WHAT'S ANOTHER EXAMPLE OF A NATURAL
12	EXPERIMENT OR WHAT YOU CALLED A SHOCK THAT OCCURS
13	NATURALLY THAT MIMICS OR MIRRORS A NATURAL EXPERIMENT?
14	A. SURE.
15	SO VERY RECENT PAPER THAT WE HAD THAT WE
16	PUBLISHED IN THE JOURNAL OF QUANTITATIVE CRIMINOLOGY,
17	STICK WITH THE CRIME EXAMPLE.
18	CRIME IS AN AREA THAT'S BEEN ALMOST WHOLLY
19	REWRITTEN THROUGH THESE NATURAL EXPERIMENTS SO IT'S A
20	GOOD SOURCE OF EXAMPLES.
21	WHAT WE DID IS WE WERE INTERESTED IN ESTIMATING
22	WHAT IS THE EFFECT OF ALCOHOL ON CRIME. SO YOU KNOW
23	WE'VE GOT THEORETICAL REASONS WE EVEN HAVE SOME CLINICAL
24	REASONS TO THINK, YOU KNOW, PEOPLE DRINK, MORE CRIME, IT
25	REDUCES INHIBITIONS ALL THAT SORT OF THING BUT AGAIN
26	THERE ARE OTHER PEOPLE THAT SAY, NO IT'S NOT A BIG
27	EFFECT. IN FACT THEY SAY MAYBE IT'S THAT THE PEOPLE WHO
28	TEND TO COMMIT CRIMES ALSO TEND TO DRINK A LOT, SO IT'S

NOT A CAUSAL RELATIONSHIP, IT'S A SELECTION EFFECT SO
 IT'S A LIVE EMPIRICAL ISSUE.

THE PROBLEM IS, AGAIN IF YOU WOULD JUST DO 3 4 REGRESSION, SO IMAGINE YOU WERE TO COMPARE CRIME IN UTAH 5 WHICH HAS SORT OF LOWER PER CAPITA DRINKING MOSTLY BECAUSE OF THE MORMON INFLUENCE TO CRIME IN ARIZONA OR 6 7 SOMETHING LIKE THAT, YOU'D WORRY AND SAY MAYBE UTAH AND 8 ARIZONA HAVE LOTS OF OTHER DIFFERENCES AS WELL. SO YOU 9 ATTEMPT TO CONTROL FOR THEM, BUT AGAIN YOU RUN INTO THE UNOBSERVABLE CHARACTERISTICS PROBLEM AND SO WE CAME UP 10 11 WITH ANOTHER NATURAL EXPERIMENT IN THIS AREA.

12 SO FOR MAJOR LEAGUE BASEBALL ALL THE TEAMS 13 STOP SELLING ALCOHOL AT SOME POINT DURING THE GAME. WE 14 FOCUSED ON PHILADELPHIA THAT'S WHERE WE ARE.

SO WE DID THE PHILLIES, PHILLOES STOP SELLING
ALCOHOL IN THE SEVENTH INNING. NOW HERE'S THE NICE
THING. YOU MIGHT SAY, WELL, HOW IS THAT A
RANDOMIZATION, BUT THINK ABOUT THE WAY BASEBALL IS
TIMED.

20 IT'S NOT A TIMED GAME, RIGHT. SO THE SEVENTH INNING TO THE END OF THE GAME COULD BE 20 MINUTES IT 21 22 COULD BE FOUR HOURS IF IT GOES INTO EXTRA INNINGS OR IF 23 THE PITCHES ARE REALLY REALLY SLOW OR SOMETHING LIKE 24 THAT SO ESSENTIALLY FOR REASONS WHOLLY UNRELATED TO 25 CRIME, YOU END UP HAVING SOME PERIOD, SOME DAYS WHERE THE FANS GO FOR A LONG PERIOD WITHOUT ALCOHOL VERSUS A 26 27 SHORT PERIOD WITHOUT ALCOHOL SO WE'RE ABLE TO COMPARE 28 SORT OF THIS LONG VERSUS THE SHORT THAT GETS THE SHOCK

THIS RANDOMIZATION FROM HAVING A LONG PERIOD A LONG GAME
 AFTERWARDS.

AND THERE SO WE CAN ALSO THEN AGAIN WORRY YOU ALWAYS WANT TO WORRY WHAT IS YOUR CONTROL GROUP WHAT IS YOUR COMPARATOR WE WERE FOCUSING ON CRIME AROUND THE BALLPARK THE NEIGHBORHOOD AROUND THE BALLPARK WHERE MOST OF THE FANS WOULD BE GOING AFTER THE GAME.

8 WE CAN COMPARE THAT TO OTHER AREAS IN THE CITY 9 PROBABLY THE -- WHAT I THINK WAS PROBABLY THE MOST NOVEL 10 COMPARISON WE DID IS WE COMPARED THE SAME GAMES TO AREAS 11 AROUND THE CITY WHERE THERE WERE SPORTS BARS. THE MOST FAMOUS SPORTS BARS IN THE CITY. BECAUSE THINK ABOUT IN 12 A SPORTS BAR, YOU HAVE ROUGHLY THE SAME KIND OF 13 14 CLIENTELE PEOPLE ARE INTERESTED IN THE GAME BUT THERE'S 15 NO CEASING -- YOU KNOW, ALCOHOL SALES DON'T GET STOPPED IN THE SEVENTH INNING IN THE SPORTS BAR, SO WE HAVE SORT 16 OF THE NICE COMPARISON. 17

AND SO WE USE THAT AS OUR SHOCK. AS OUR NATURAL EXPERIMENT IN THAT INSTANCE AND WE FOUND, YOU KNOW, ANTICIPATING SINCE YOU'RE INTERESTED IN THE LAST RESULT, WE FOUND MAYBE SOMEBODY, WE FOUND THAT THE LONGER THAT YOU WENT WITHOUT ALCOHOL, THE LESS CRIME THERE WAS IN THE AREA OF THE CITY AROUND THE BALLPARK VERSUS THE OTHER COMPARABLES AREAS AROUND THE CITY.

ONE LAST SORT OF NATURAL EXPERIMENT WITHIN OUR
NATURAL EXPERIMENT IN OUR SAMPLE PERIOD, COMCAST WHICH
HAS A BIG PRESENCE IN PHILADELPHIA, ESSENTIALLY BUILT A
SPORTS BAR COMPLEX IN THE PARKING LOT OF THE BASEBALL

PARK.

1

2 Ο. IN RESPONSE TO THE SEVENTH INNING RULE? NO, NO, JUST BECAUSE COMCAST IS A MONEY MACHINE 3 Α. AND DECIDES THERE WAS MONEY TO BE MADE BUT IT HAPPENS 4 5 RIGHT IN THE MIDDLE OF OUR SAMPLING PERIOD AND THE INTERESTING THING ABOUT THIS THING IS EFFECTIVELY UNDOES 6 7 THE -- UNDOES THE SEVENTH INNING RULE, RIGHT, BECAUSE 8 YOU CAN LEAVE THE BALLPARK AND IMMEDIATELY START 9 DRINKING AGAIN 100 FEET FROM THE BALLPARK AND WATCH THE 10 GAME ON JUST AS BIG SCREENS WITH JUST AS MANY PEOPLE.

11 AND SO IF OUR EARLIER RESULT THAT SORT OF THE 12 LONGER GAMES AFTER THE SEVENTH INNING LED TO LESS CRIME WAS NOT DUE TO THE ALCOHOL AND THE ALCOHOL POLICY, 13 14 RIGHT, YOU MIGHT EXPECT, WELL, THEN WE SHOULD STILL FIND 15 THAT RESULT AFTER COMCAST OPENS IN THE PARKING LOT, BUT 16 IF IT WAS DUE TO THE ALCOHOL POLICY AND EFFECTIVELY COMCAST UNDOES IT, YOU SHOULD SEE OUR RESULT GO AWAY AND 17 18 IF FACT WE DID, SO THAT'S AN EXAMPLE WHERE WE SORT OF 19 HAD A DOUBLE NATURAL EXPERIMENT IN SOME SENSE.

Q. NOT BY CHOICE BUT JUST LOOKING FOR THE RIGHT -A. SERENDIPITY.

Q. AND AGAIN, BACK TO THE MERCK EXAMPLE, THAT'S
ANOTHER EXAMPLE OF THE BLIND STUDY IT'S BLIND BECAUSE
IT'S JUST AN INCIDENCE THAT OCCURS AND NOBODY KNOWS
ABOUT IT IN THE GROUP THAT YOU'RE STUDYING?
A. THAT'S RIGHT AND THERE'S NO ANTICIPATION OF IT,
IT'S NOT AS THOUGH THAT IT'S TRIGGERED BY CRIME ISSUES,

28 RIGHT IT'S NOT AS THOUGH THERE'S YOU KNOW, SOME

2 THE ALCOHOL CONSUMPTION TO CHANGE. IT'S INSTEAD JUST THIS RANDOMIZED SHOCK THAT 3 4 AFFECTS SOME GAMES BUT NOT OTHERS. 5 Ο. DID IT MATTER WHAT THE POPULATION AT THE GAME WAS? 6 7 SO IT'S INTERESTING. YOU MIGHT BE AWARE YOU Α. 8 MIGHT SAY, WELL, KLICK YOU KNOW IF GAMES GO LONGER, MORE 9 FANS LEAVE AND SO THERE'S LESS PEOPLE FEWER PEOPLE AROUND OR SOMETHING LIKE THAT. SO WHAT WE'RE ABLE TO DO 10 11 IS WE'RE ABLE TO DO THINGS LIKE LOOK AT VERY CLOSE 12 GAMES. GAMES THAT WERE ONE OR TWO RUN GAMES. YOU MIGHT THINK ALL THINGS EQUAL, PEOPLE ARE LESS LIKELY TO LEAVE 13 ON A VERY CLOSE GAME THAN A BIG GAME. AND WE DIDN'T SEE 14 15 ANY DIFFERENCE, YOU KNOW, AMONG THOSE GAMES VERSUS THE 16 OTHERS. WE DON'T THINK -- WE DON'T THINK IT WAS THAT 17 18 PEOPLE WERE LEAVING OR THAT THERE WAS SOMETHING ABOUT 19 THE GAME PER SE THAT LED TO THE CHANGE. WE'RE PRETTY

UNOBSERVED VARIABLE THAT'S SORT OF CAUSING -- CAUSING

21 THAT'S THE WAY SCIENCE WORKS.

1

20

YOU RAISED AN INTERESTING POSSIBILITY. WELL,
OKAY, WHAT IF THERE'S SOMETHING DIFFERENT ABOUT THESE
GAMES. YOU CLAIM IT'S A RANDOMIZATION BUT OF COURSE,
IT'S NOT EXACTLY RANDOMIZATION IT'S NOT AS THOUGH WE'RE
FLIPPING THE COIN. SO WE NEED TO GO AND CHECK, AND ARE
THERE ANY OTHER WAYS TO FALSIFY OUR IDEA. AND THAT'S
THE WAY SCIENCE WORKS. YOU SORT OF TAKE YOUR BEST

CONFIDENT THAT IT WAS IN FACT THE ALCOHOL ISSUE. BUT

1	DESIGN AND THEN YOU THINK OF ALL THE WAYS THAT IT COULD
2	FAIL AND YOU TRY TO TEST IT AGAINST THOSE AS WELL.
3	Q. SO IS THE QUICKLY, COULD YOU JUST DESCRIBE
4	WHY THE DESCRIPTIVE MODEL WOULD NOT WORK FOR THOSE ONE
5	OR TWO EXAMPLES?
6	A. SURE.
7	SO JUST TAKE DESCRIPTIVELY IF YOU SAY, YOU
8	KNOW, I THINK THAT THERE ARE IF I WANTED TO TEST ALCOHOL
9	AND CRIME I MIGHT SAY, WELL WHY DON'T YOU JUST COMPARE
10	DESCRIPTIVELY CRIME IN THE BLOCKS THE CITY BLOCKS AROUND
11	BARS.
12	RIGHT. AND SAY WELL GEE, IN MOST CITIES THIS
13	IS TRUE THE BLOCKS AROUND BARS HAVE HIGHER CRIME RATES
14	AND YOU MIGHT SAY WELL DOESN'T THIS PROVE THAT ALCOHOL
15	LEADS TO CRIME.
16	WELL OF COURSE IT DOESN'T PROVE BECAUSE IT MAY
17	WELL BE THAT THOSE BARS WERE LOCATED IN PARTICULAR AREAS
18	BECAUSE OF UNOBSERVED CHARACTERISTICS THAT ARE RELATED
19	BOTH TO CRIME AND TO ALCOHOL.
20	RIGHT. YOU KNOW, IT MAY WELL BE THAT THERE'S
21	SOME UNDERLYING YOU KNOW, I DON'T WANT TO I DON'T SAY
22	THIS NORMATIVELY BUT JUST AS A SHORTCUT SOME UNDERLYING
23	DYSFUNCTION VARIABLE THAT LEADS BOTH TO PEOPLE TO COMMIT
24	CRIMES AND TO DRINK A LOT AND SO THERE'S NOT THAT
25	IT'S NOT THAT ALCOHOL IN THIS SCENARIO IT'S NOT THAT
26	ALCOHOL LEADS TO CRIME IT'S THAT SOME THIRD VARIABLE
27	LEADS BOTH TO CRIME AND ALCOHOL AND YOU NEED TO RULE
28	THAT POSSIBILITY OUT.

1 THE DESCRIPTIVE STATISTICS APPROACH IS NEVER 2 GOING TO ALLOW YOU TO DO THAT. 3 AND WHY WOULD NOT REGRESSION DO THAT? Q. WELL SO AGAIN IF WE HAD DATA ON EVERYTHING THAT 4 Α. 5 MATTERED REGRESSION WOULD. HERE'S THE PROBLEM THOUGH: TAKE MY DYSFUNCTION VARIABLE. 6 7 YOU KNOW WE CAN ALL KIND OF INTUITIVELY SEE, MAYBE THAT'S -- THAT'S THAT AND MAYBE WE CAN KIND OF 8 9 AGREE I KIND OF KNOW WHAT DYSFUNCTION IS, BUT I 10 CHALLENGE YOU TO QUANTIFY IT. I CHALLENGE YOU TO BE 11 ABLE TO GET DATA THAT ACTUALLY TRACKS OR INDEXES, DYSFUNCTION BY CITY BLOCK FOR EXAMPLE. 12 IF YOU DON'T HAVE DATA FOR IT, YOU CAN'T PUT IT 13 IN THE REGRESSION, IF YOU CAN'T PUT IT IN THE REGRESSION 14 YOU CAN'T ADJUST FOR THAT DIFFERENCE. AND THEN ANY 15 16 ESTIMATES THAT YOU GET FROM THAT REGRESSION YOU CAN'T BE SURE WHETHER THEY'RE DUE TO THE THING THAT WAS IN THE 17 18 MODEL OR DUE TO THE STUFF THAT YOU LEFT OUT. Q. RIGHT. SO YOU GAVE US A COUPLE OF EXAMPLES IN 19 20 THE CRIME MODEL WHICH ARE PRETTY CRYSTAL CLEAR ACTUALLY ON THE USE OF THE SO CALLED NATURAL EXPERIMENT OR THE 21 22 SHOCK THAT OCCURS NATURALLY TO HELP CREATE THAT MERCK 23 EFFECT OF THE CONTROL GROUP AND THE TEST GROUP AND THEN 24 YOU HAVE A FAIR COMPARISON? 25 Α. UH-HUH. 26 Q. I'VE NOTICED IN YOUR TELLING US THIS, THAT YOUR 27 HYPOTHESIS NEVER -- YOU NEVER REVISITED YOUR HYPOTHESIS. 28 IT'S ALMOST AS IF YOU SET A HYPOTHESIS AND THEN YOU GO
1 OUT AND TEST IT AND YOU FOUND DATA AND ACCUMULATE THE 2 DATA.

3 SO WHERE DOES THE OBJECTIVITY OR THE
4 SUBJECTIVITY IN THE HYPOTHESIS PLAY A ROLE IN THIS?

A. WELL SO FOR THE PAPERS THAT I JUST TOLD YOU
ABOUT THE WASHINGTON D.C. PAPER OR THE PHILADELPHIA
PAPER, YOU KNOW, THE DATA -- THE DATA ARE ALL PUBLIC
DATA AND THE METHODOLOGIES ARE ALL EASILY REPLICABLE.

9 AND SO I SUPPOSE IF THERE WERE SOME
10 SUBJECTIVITY OR LACK OF OBJECTIVITY, YOU KNOW IN THE
11 DESIGNS, YOU KNOW PEOPLE COULD PROBE AND PEOPLE HAVE
12 PROBED VARIOUS PAPERS OF MINOR OTHER PEOPLES TRY TO REDO
13 THEM OR USE A DIFFERENT NATURAL EXPERIMENT TO TRY TO
14 ANALYZE THE SAME THING AND DO YOU FIND SORT OF SIMILAR
15 YOU KNOW SIMILAR TYPE OF RESULTS.

16 SO IT'S INTERESTING, THE -- THE ONE THING THAT 17 WE KNOW FROM SOCIAL SCIENCE IS THAT WHILE THERE ARE NO 18 PERFECT DESIGNS, THERE ARE MORE CREDIBLE DESIGNS 19 RESEARCH DESIGNS THAN OTHERS.

BUT EVEN FOR THE VERY CREDIBLE RESEARCH DESIGNSREPLICABILITY IS SORT OF A USEFUL TOOL FOR CONFIDENCE.

22 SO FOR OUR -- OUR PAPER ON THE WASHINGTON D.C. 23 TERROR ALERTS, IT TURNS OUT THAT SORT OF AROUND THE SAME 24 TIME THAT WE WROTE OUR PAPER, ANOTHER SET OF RESEARCHERS 25 DID A SIMILAR THING IN BUENOS AIRES. THERE HAD BEEN A 26 TERRORIST ATTACK THERE AND THEY -- THEY DECIDED --27 BUENOS AIRES DECIDED TO PUT EXTRA POLICE ON EVERY BLOCK 28 THAT HAD SYNAGOGUE OR A MOSQUE BECAUSE THE TERRORISM HAD

A RELIGIOUS BAY LENS TO IT, SO IF YOU THINK THAT -- THAT 1 2 SYNAGOGUES AND MOSQUES WERE NOT NECESSARILY LOCATED IN A PARTICULAR PLACE BECAUSE OF CRIME, THIS AGAIN ALSO SEEMS 3 4 LIKE A SHOCK. ALL OF A SUDDEN THESE CERTAIN PARTS IN 5 THE CITY GET EXTRA COPS JUST BECAUSE OF SOMETHING TOTALLY UNRELATED TO CRIME, IT WAS BECAUSE OF TERRORISM, 6 7 BUT THEY WERE ABLE TO DO THE SAME KIND OF DESIGN. LOOK 8 AT BEFORE AND AFTER THE PLACES THAT GOT MORE COPS 9 COMPARE THEM TO BEFORE AND AFTER TO COMPARABLE PLACES THAT DIDN'T HAVE HAPPEN TO HAVE A SYNAGOGUE OR A MOSQUE 10 11 AND SO IT DIDN'T GET MORE COPS ABOUT 10 YEARS LATER SOME 12 BRITISH RESEARCHERS DID OUR SAME DESIGN IN LONDON, LONDON HAD HAD A TERRORIST ATTACK, ESSENTIALLY THE SAME 13 14 THING HAPPENED THEY DID THE SAME ACTUAL EXPERIMENT AND 15 FOUND ROUGHLY THE SAME RESULTS.

NOT JUST THE SAME RESULTS IN TERMS OF POLICE 16 GENERATING DETERRENTS, BUT EVEN THE QUANTITATIVE EFFECT 17 18 WAS COMPARABLE. AND THAT'S HOW SCIENCE WORKS YOU COME 19 UP WITH THE MOST PLAUSIBLE THE MOST COMPELLING, THE MOST 20 RIGOROUS DESIGN THAT YOU CAN AND SEE WHAT YOU CAN FIND. YOU ADDRESS POTENTIAL ALTERNATIVE HYPOTHESIS AS BEST YOU 21 22 CAN AND THEN YOU WAIT TO SEE IF OTHER PEOPLE SORT OF 23 EFFECTIVELY REPLICATE THE WORK.

Q. SO IN YOUR TERROR ALERT EXAMPLE, IF THE RESULT
HAD BEEN THAT CRIME DID NOT REDUCE IN THAT AREA, WHAT
HAPPENS TO THAT PAPER IF THE PREMISE OR HYPOTHESIS WAS
MORE POLICE EQUALS LOWER CRIME?

A. YOU KNOW YOU WRITE-UP THOSE RESULTS AND SAY

28

HERE'S WHAT THE HYPOTHESIS, THE HYPOTHESIS WAS REJECTED 1 2 BY THE DATA. AND YOU WRITE THE PAPER AND ATTEMPT TO 3 PUBLISH IT. NOW THERE IS -- AS A PRACTICAL MATTER, DEPENDING ON WHAT FIELD AND DEPENDING ON WHAT THE TOPIC 4 5 IS, SOME RESULTS ARE MORE LIKELY TO GET PUBLISHED THAN 6 OTHER RESULTS AND THAT'S MAYBE WHERE SOME OF THE 7 SUBJECTIVITY COMES IN NOT FROM THE AUTHOR'S STANDPOINT 8 BUT JUST FROM THE VENUE STANDPOINT.

9 EDITORS THEMSELVES MIGHT HAVE THEIR OWN
10 PREFERENCES OR BIASES OR PRIOR BELIEFS, AND MAY BE MORE
11 LIKELY TO ACCEPT A PAPER THAT HAS A CERTAIN RESULT THAN
12 A PAPER THAT DOESN'T.

SO YOU DO HAVE TO WORRY ABOUT THAT A LITTLE 13 BIT. AND THERE IS A LITERATURE, SORT OF ALMOST A 14 15 FORENSIC LITERATURE ON TRYING TO FIGURE OUT WHICH 16 JOURNALS AND WHICH FIELDS AND WHICH TOPICS HAVE BEEN AFFECTED BY THIS PUBLICATION BIAS SO TO SPEAK. BUT FROM 17 18 MY PERSPECTIVE AS A RESEARCHER IF WE HAD DONE THAT WORK 19 AND FOUND NO RESULT WE WOULD HAVE WRITTEN IT UP ROUGHLY 20 THE SAME. THE CONCLUSION OBVIOUSLY WOULD HAVE BEEN DIFFERENT BUT THAT WOULD HAVE BEEN THE PAPER. 21 22 DESCRIPTIVE DIDN'T SOLVE THE PROBLEM, Ο. 23 REGRESSION COULDN'T BE USED BECAUSE OF THE DATA IN YOUR 24 EXAMPLES? 25 Α. YEP.

26 Q. BUT YOU LOOKED TO THESE NATURAL EXPERIMENTS,27 WHICH WAS THE SEVENTH INNING RULE?

A. SURE.

Q. AND THE DISTRICT ONE IN D C OR -- YEAH, THE SEVENTH INNING RULE IN PHILLY AND THE DISTRICT ONE IN D.C.

4 DOES THAT END THE MATTER? I MEAN, IS THAT THE 5 END OF THE STUDY IN THAT INSTANCE OR ARE THERE OTHER 6 TOOLS THAT ARE AVAILABLE TO YOU TO USE TO ACCOUNT FOR 7 SOME OF THOSE VARIABLES THAT HADN'T BEEN ACCOUNTED FOR 8 IN THE DESCRIPTIVE REGRESSION OR THE NATURAL EXPERIMENT?

9 Α. SO THESE NATURAL EXPERIMENT TYPE APPROACHES, AT LEAST CATEGORICALLY ARE ABOUT THE BEST THAT WE HAVE NOW 10 11 SO GENERICALLY IT'S NOT AS THOUGH THERE'S ANOTHER SET OF 12 TOOLS. IT'S NOT -- I GET WHERE YOU'RE GOING THAT YOU START WITH THE REALLY SIMPLISTIC AND THEN THERE'S 13 ANOTHER TOOL THAT'S MORE SOPHISTICATED AND ANOTHER TOOL 14 15 THAT'S MORE SOPHISTICATED, BUT IT TURNS OUT THAT SORT OF THE NATURAL EXPERIMENT OR QUASI EXPERIMENTAL METHODS ARE 16 ABOUT THE END OF THE LINE RIGHT NOW. 17

18 Q. HOW DO WE -- HOW DO I -- IF I WANT THIS STUDY 19 TO BE DONE AND YOU DO IT AND YOU REPORT BACK TO ME THAT 20 CRIME REDUCED WHILE THERE WAS MORE POLICE PRESENCE, HOW 21 CAN I BE CONFIDENT IN THAT RESULT?

A. LIKE I SAID, IN AND OF ITSELF, AS THE SINGLE
PAPER, IF YOU -- IF YOU THINK THE DESIGN IS PLAUSIBLE,
AND THAT'S AN INTERESTING DISCUSSION, YOU KNOW WHAT
MAKES A DESIGN PLAUSIBLE.

26 WELL A DID DESIGN LIKE THAT, BASICALLY WHAT WE 27 HAVE TO ASSUME IS -- AND THERE ARE ALWAYS ASSUMPTIONS IF 28 YOUR LOOKING FOR A RESEARCH DESIGN WITHOUT ASSUMPTIONS, THERE IS NO RESEARCH DESIGN WITHOUT ASSUMPTIONS.

1

2 THE PLAUSIBILITY INOUIRY IS ABOUT HOW PLAUSIBLE 3 THE ASSUMPTIONS ARE OR ARE THERE SMALLER ASSUMPTIONS THAT ONE COULD MAKE AND YOU ATTEMPT TO SORT OF EITHER 4 5 NARROW OR MAKE THE MOST PLAUSIBLE ASSUMPTIONS. BUT BACK TO YOUR QUESTION, IF YOU ROAD MY TERROR ALERT PAPER YOU 6 7 MIGHT SAY THE FIRST THING YOU MIGHT DO IS TRY TO COME UP 8 WITH ALTERNATIVE HYPOTHESIS. SO IN FACT WE DID IN THE 9 PAPER. WE SAID HERE'S A CONCERN. YOU KNOW, WE TRY TO DO SOME SELF CRITICISM. HERE'S A CONCERN: THOSE OF US 10 11 IN THIS ROOM ARE OLD ENOUGH TO KIND OF REMEMBER THE TIME 12 PERIOD WE WERE ON THE EAST COAST YOU RECALL IN THIS TIME PERIOD PEOPLE WERE SCARED, RIGHT. AND SO MAYBE WHAT 13 14 YOU'RE WORRIED ABOUT IS WELL MAYBE IT DOESN'T HAVE 15 ANYTHING TO DO WITH POLICE AND DETERRING CRIME MAYBE IT 16 HAS TO DO WITH POTENTIAL VICTIMS, I.E. TOURISTS, LET'S SAY, JUST DECIDED TO STAY AWAY FROM WASHINGTON ON THESE 17 18 HIGH ALERT DAYS, SO THAT'S A PLAUSIBLE ALTERNATIVE 19 HYPOTHESIS THAT WOULD GENERALLY ROUGHLY THE SAME 20 RESULTS. SO THE NEXT STEP THAT YOU WOULD DO IS YOU WOULD ATTEMPT TO RULE IT OUT. HOW DO YOU RULE IT OUT. 21 22 IS YOU TRY TO GET SOME DATA.

SO WHAT WE DO IS WE GOT DATA ON METRO
RIDERSHIP, FOR EXAMPLE. RIGHT. IF PEOPLE WERE STAYING
AWAY FROM THE CITY YOU SHOULD HAVE SEEN METRO RIDERSHIP
DECLINE ON THOSE DAYS AND SO WE WERE ABLE TO PUT THAT
DATA RIGHT INTO THE REGRESSION THAT WE USED TO STUDY THE
NATURAL EXPERIMENT AND IT DIDN'T AFFECT OUR RESULT, BUT

1	THAT IS THE APPROACH, RIGHT? YOU TRY TO THINK OF
2	ALTERNATIVE HYPOTHESES.
3	Q. WHERE DOES THE IDEA OF A STATISTICAL
4	SIGNIFICANCE FIT INTO ALL THIS?
5	A. SURE. SO IF WE THINK BACK GEE, PROBABLY ABOUT
6	TWO HOURS NOW, CONCEPTUALLY WHAT I SAID IS IMAGINE IF WE
7	HAD TWO COMPANIES THEY'RE IDENTICAL IN EVERY WAY EXCEPT
8	ONE HAS MORE WOMEN ON THE BOARD ONE HAS NO WOMEN ON THE
9	BOARD LET'S SAY.
10	BUT WE'RE CONVINCED THEY'RE IDENTICAL IN EVERY
11	OTHER WAY.
12	WE THEN GO AND LOOK. IS THEIR PERFORMANCE
13	DIFFERENT. IF THEIR PERFORMANCE IS DIFFERENT THERE ARE
14	TWO POSSIBLE REASONS FOR IT TO BE DIFFERENT. ONE IS THE
15	POURED COMPOSITION. RIGHT. THAT'S PRESUMABLY THE THING
16	THAT PEOPLE ARE STUDYING IN THIS AREA.
17	THAT'S ONE POSSIBILITY. THE OTHER POSSIBILITY
18	IS MAYBE THIS IS RANDOM. RIGHT. YOU KNOW. WE HAVE
19	IDENTICAL TWINS FOR EXAMPLE A WHO ARE GENETICALLY
20	COMPARABLE AND RAISED IN THE SAME ENVIRONMENT AND YET,
21	YOU KNOW ONE IS GOOD AT BASKETBALL AND ONE IS NOT. JUST
22	A RANDOM CHANCE EXISTS IN THE REAL WORLD AND SO WITH THE
23	EXAMPLE OF TWO COMPANIES THAT ARE IDENTICAL IN EVERY WAY
24	WE STILL WORRY WHAT IF IT'S JUST RANDOM THAT THE OUTCOME
25	WAS DIFFERENT BETWEEN THESE TWO COMPANIES.
26	NOW I SAID EARLIER IF INSTEAD OF LOOKING AT TWO
27	IDENTICAL COMPANIES WE LOOK THE 1,000 IDENTICAL
28	COMPANIES, RIGHT THE LIKELIHOOD OF RANDOMNESS OR

COINCIDENCE DRIVING THAT DIFFERENCE BECOMES LESS AND
 LESS. THIS IS GENERICALLY TRUE IN STATISTICS. IT'S A
 RESULT OF SOMETHING KNOWN AS THE LAW OF LARGE NUMBERS
 IT'S ALSO THE RESULT OF SOMETHING KNOWN AS THE CENTRAL
 LIMIT THEOREM. IT ESSENTIALLY MEANS AS YOUR SAMPLE SIZE
 GETS LARGER THE INFLUENCE OF RANDOM CHANCE GETS SMALLER.
 PROBABILISTICALLY SMALLER.

8 PUT INTO INTUITION INTO A FUNCTIONAL TOOL AND
9 THAT'S WHERE WE GET THIS IDEA OF STATISTICAL
10 SIGNIFICANCE.

11 SO THE IDENTICAL OF STATISTICAL SIGNIFICANCE IS 12 HOW LIKELY IS IT THAT THE RESULT THAT WE'VE ESTIMATED, THE EFFECT OF POLICE ON CRIME OR THE EFFECT OF ALCOHOL 13 ON CRIME OR THE EFFECT OF WOMEN ON BOARDS ON FIRM 14 15 PERFORMANCE HOW LIKELY WOULD WE BE TO OBSERVE WHATEVER 16 WE ESTIMATED JUST BY RANDOM CHANCE, RIGHT. THAT'S THE QUESTION. FIRMS RETURNS THEY GO UP AND THEY GO DOWN. 17 18 YOU CHECK YOUR PORTFOLIO EVERY DAY. ONE DAY IT GOES UP 25 BASIS POINTS ONE DAY IT GOES DOWN 43 BASIS POINTS, 19 20 RIGHT.

21 NOT REALLY ANYTHING HAPPENING THAT DAY. JUST, 22 YOU KNOW RANDOM, ANIMAL SPIRITS, JUST RANDOM CHANCE. 23 BUT THERE'S A LIMIT ON HOW MUCH RANDOMNESS 24 THERE CAN BE. IF THE STOCK MARKET DECLINED BY 25 25 PERCENT TOMORROW IT'S UNLIKELY THAT WOULD HAVE BEEN 26 RANDOM CHANCE. MAYBE IT WOULD BE SOMETHING JUST 27 CATASTROPHIC THAT HAPPENED. AND SO THE FACT THAT THERE 28 IS LIMITS ON HOW MUCH RANDOMNESS THERE CAN BE AND THE

1	LIMITS ARE A FUNCTION OF THE SAMPLE SIZE ALL OTHER
2	THINGS EQUAL, LARGER SAMPLE SIZE SMALLER LIMIT ON HOW
3	MUCH RANDOMNESS CAN BE DRIVING A RESULT, SMALLER SAMPLE
4	SIZE, LARGER LIMITS ON HOW MUCH RANDOMNESS CAN BE
5	DRIVING A RESULT.
6	SO STATISTICAL SIGNIFICANCE IS THE ATTEMPT TO
7	PUT SOME PROBABILISTIC STATEMENT ON THAT LEVEL OF
8	UNCERTAINTY. SO TO MAKE IT MORE CONCRETE WHEN WE IN
9	THE IN THE CONTEXT OF THE POLICE AND CRIME IN
10	WASHINGTON WHEN WE ESTIMATED THAT CRIME GENERICALLY SORT
11	OF DROPS LET'S SAY BY 10 PERCENT EVERY TIME THE TERROR
12	ALERT WENT UP, THE TWO CHOICES THERE ARE IT WAS EITHER
13	BECAUSE OF THE POLICE OR BECAUSE OF JUST RANDOM
14	VARIATION, RIGHT.
15	NOW, WE ESTIMATED THE ABOUT, I THINK IN OUR
15 16	NOW, WE ESTIMATED THE ABOUT, I THINK IN OUR PAPER I THINK WE HAD THREE OR FOUR TERROR ALERT, SO WHEN
15 16 17	NOW, WE ESTIMATED THE ABOUT, I THINK IN OUR PAPER I THINK WE HAD THREE OR FOUR TERROR ALERT, SO WHEN THE TERROR ALERT WEPT OFF AND OFF, SO THAT'S AVERAGING
15 16 17 18	NOW, WE ESTIMATED THE ABOUT, I THINK IN OUR PAPER I THINK WE HAD THREE OR FOUR TERROR ALERT, SO WHEN THE TERROR ALERT WEPT OFF AND OFF, SO THAT'S AVERAGING THREE OR FOUR OF THEM SO WE'RE AVERAGING OVER SIX OR
15 16 17 18 19	NOW, WE ESTIMATED THE ABOUT, I THINK IN OUR PAPER I THINK WE HAD THREE OR FOUR TERROR ALERT, SO WHEN THE TERROR ALERT WEPT OFF AND OFF, SO THAT'S AVERAGING THREE OR FOUR OF THEM SO WE'RE AVERAGING OVER SIX OR EIGHT DIFFERENT SHOCKS, RIGHT AND SO THAT THAT SORT
15 16 17 18 19 20	NOW, WE ESTIMATED THE ABOUT, I THINK IN OUR PAPER I THINK WE HAD THREE OR FOUR TERROR ALERT, SO WHEN THE TERROR ALERT WEPT OFF AND OFF, SO THAT'S AVERAGING THREE OR FOUR OF THEM SO WE'RE AVERAGING OVER SIX OR EIGHT DIFFERENT SHOCKS, RIGHT AND SO THAT THAT SORT OF AS THE SAMPLE SIZE GETS BIGGER THE LESSEN EXCERPT YOU
15 16 17 18 19 20 21	NOW, WE ESTIMATED THE ABOUT, I THINK IN OUR PAPER I THINK WE HAD THREE OR FOUR TERROR ALERT, SO WHEN THE TERROR ALERT WEPT OFF AND OFF, SO THAT'S AVERAGING THREE OR FOUR OF THEM SO WE'RE AVERAGING OVER SIX OR EIGHT DIFFERENT SHOCKS, RIGHT AND SO THAT THAT SORT OF AS THE SAMPLE SIZE GETS BIGGER THE LESSEN EXCERPT YOU HAVE IN YOUR ESTIMATE. ESSENTIALLY WHAT WE NEED TO THEN
15 16 17 18 19 20 21 22	NOW, WE ESTIMATED THE ABOUT, I THINK IN OUR PAPER I THINK WE HAD THREE OR FOUR TERROR ALERT, SO WHEN THE TERROR ALERT WEPT OFF AND OFF, SO THAT'S AVERAGING THREE OR FOUR OF THEM SO WE'RE AVERAGING OVER SIX OR EIGHT DIFFERENT SHOCKS, RIGHT AND SO THAT THAT SORT OF AS THE SAMPLE SIZE GETS BIGGER THE LESSEN EXCERPT YOU HAVE IN YOUR ESTIMATE. ESSENTIALLY WHAT WE NEED TO THEN DO IS SAY, HOW DOES THIS VARIATION IN OUR ESTIMATE HOW
15 16 17 18 19 20 21 22 23	NOW, WE ESTIMATED THE ABOUT, I THINK IN OUR PAPER I THINK WE HAD THREE OR FOUR TERROR ALERT, SO WHEN THE TERROR ALERT WEPT OFF AND OFF, SO THAT'S AVERAGING THREE OR FOUR OF THEM SO WE'RE AVERAGING OVER SIX OR EIGHT DIFFERENT SHOCKS, RIGHT AND SO THAT THAT SORT OF AS THE SAMPLE SIZE GETS BIGGER THE LESSEN EXCERPT YOU HAVE IN YOUR ESTIMATE. ESSENTIALLY WHAT WE NEED TO THEN DO IS SAY, HOW DOES THIS VARIATION IN OUR ESTIMATE HOW DOES THAT COMPARE SO JUST THE GENERAL VARIATION IN
15 16 17 18 19 20 21 22 23 24	NOW, WE ESTIMATED THE ABOUT, I THINK IN OUR PAPER I THINK WE HAD THREE OR FOUR TERROR ALERT, SO WHEN THE TERROR ALERT WEPT OFF AND OFF, SO THAT'S AVERAGING THREE OR FOUR OF THEM SO WE'RE AVERAGING OVER SIX OR EIGHT DIFFERENT SHOCKS, RIGHT AND SO THAT THAT SORT OF AS THE SAMPLE SIZE GETS BIGGER THE LESSEN EXCERPT YOU HAVE IN YOUR ESTIMATE. ESSENTIALLY WHAT WE NEED TO THEN DO IS SAY, HOW DOES THIS VARIATION IN OUR ESTIMATE HOW DOES THAT COMPARE SO JUST THE GENERAL VARIATION IN CRIME. SO IF IT TURNED OUT ON ANY GIVEN DAY IN I'M
15 16 17 18 19 20 21 22 23 24 25	NOW, WE ESTIMATED THE ABOUT, I THINK IN OUR PAPER I THINK WE HAD THREE OR FOUR TERROR ALERT, SO WHEN THE TERROR ALERT WEPT OFF AND OFF, SO THAT'S AVERAGING THREE OR FOUR OF THEM SO WE'RE AVERAGING OVER SIX OR EIGHT DIFFERENT SHOCKS, RIGHT AND SO THAT THAT SORT OF AS THE SAMPLE SIZE GETS BIGGER THE LESSEN EXCERPT YOU HAVE IN YOUR ESTIMATE. ESSENTIALLY WHAT WE NEED TO THEN DO IS SAY, HOW DOES THIS VARIATION IN OUR ESTIMATE HOW DOES THAT COMPARE SO JUST THE GENERAL VARIATION IN CRIME. SO IF IT TURNED OUT ON ANY GIVEN DAY IN I'M SORRY IN DISTRICT ONE, CRIME REGULARLY CHANGED BY
15 16 17 18 19 20 21 22 23 24 25 26	NOW, WE ESTIMATED THE ABOUT, I THINK IN OUR PAPER I THINK WE HAD THREE OR FOUR TERROR ALERT, SO WHEN THE TERROR ALERT WEPT OFF AND OFF, SO THAT'S AVERAGING THREE OR FOUR OF THEM SO WE'RE AVERAGING OVER SIX OR EIGHT DIFFERENT SHOCKS, RIGHT AND SO THAT THAT SORT OF AS THE SAMPLE SIZE GETS BIGGER THE LESSEN EXCERPT YOU HAVE IN YOUR ESTIMATE. ESSENTIALLY WHAT WE NEED TO THEN DO IS SAY, HOW DOES THIS VARIATION IN OUR ESTIMATE HOW DOES THAT COMPARE SO JUST THE GENERAL VARIATION IN CRIME. SO IF IT TURNED OUT ON ANY GIVEN DAY IN I'M SORRY IN DISTRICT ONE, CRIME REGULARLY CHANGED BY 10 PERCENT, WELL THEN THE FACT THAT IT CHANGED ON
15 16 17 18 19 20 21 22 23 24 25 26 27	NOW, WE ESTIMATED THE ABOUT, I THINK IN OUR PAPER I THINK WE HAD THREE OR FOUR TERROR ALERT, SO WHEN THE TERROR ALERT WEPT OFF AND OFF, SO THAT'S AVERAGING THREE OR FOUR OF THEM SO WE'RE AVERAGING OVER SIX OR EIGHT DIFFERENT SHOCKS, RIGHT AND SO THAT THAT SORT OF AS THE SAMPLE SIZE GETS BIGGER THE LESSEN EXCERPT YOU HAVE IN YOUR ESTIMATE. ESSENTIALLY WHAT WE NEED TO THEN DO IS SAY, HOW DOES THIS VARIATION IN OUR ESTIMATE HOW DOES THAT COMPARE SO JUST THE GENERAL VARIATION IN CRIME. SO IF IT TURNED OUT ON ANY GIVEN DAY IN I'M SORRY IN DISTRICT ONE, CRIME REGULARLY CHANGED BY 10 PERCENT, WELL THEN THE FACT THAT IT CHANGED ON 10 PERCENT BY THE DAY OF THE TERROR ALERT IS NOT

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1 CRIME CHANGES IS BY A COUPLE OF PERCENT, WELL THEN MAYBE 2 10 PERCENT LOOKS REALLY BIG AND STATISTICAL SIGNIFICANCE 3 IS AN ATTEMPT TO SORT OF COMPARE YOUR ESTIMATED RESULT TO ESSENTIALLY THE VARIATION IN THAT BACKGROUND OUTCOME. 4 5 AND SO WHEN SOMEONE SAYS A RESULT IS STATISTICALLY SIGNIFICANT, WHAT THEY'RE SAYING IS THE RESULT THAT 6 7 THEY'RE ESTIMATING IS RELATIVELY LARGE AS COMPARED TO 8 THE NORMAL BACKGROUND VARIATION IN THE DATA, RIGHT.

9 NOW OF COURSE YOU'VE GOT TO PUT SOME MORE
10 PARAMETERS ON IT. OFTEN TIMES IN THE SOCIAL SCIENCE
11 LITERATURE, YOU MIGHT SAY, WELL MY RESULT WAS STATISTICS
12 COLLIE SIGNIFICANT AT THE 5 PERCENT LEVEL THAT'S SORT OF
13 A COMMON CONVENTIONAL STANDARDS.

14 IF YOUR RESULT IS STATISTICALLY SIGNIFICANT AT 15 THE 5 PERCENT LEVEL WHAT THAT ESSENTIALLY MEANS IS IN 16 THE NORMAL DAY-TO-DAY VARIATION OF YOUR DATA, YOU WOULD 17 NOT FIND A CHANGE AS BIG AS YOU'VE ESTIMATED IN ANY MORE 18 THAN 5 PERCENT OF THE OTHER -- THE OTHER OBSERVATIONS.

19 THAT'S ESSENTIALLY WHAT STATISTICAL 20 SIGNIFICANCE IS DOING. AGAIN IT'S A STATEMENT ABOUT YOUR CERTAINTY OR LEVEL OF UNCERTAINTY AS TO WHETHER OR 21 22 NOT YOU CAN DISTINGUISH YOUR ESTIMATED RESULT FROM NORMAL BACKGROUND VARIATION. AND I GUESS ONE LAST THING 23 24 I WOULD LIKE TO PUT IN THERE IS STATISTICS SIGNIFICANCE 25 NEED NOT MEAN PRACTICAL SIGNIFICANCE. RIGHT YOU COULD 26 HAVE A RESULT THAT IS STATISTICALLY SIGNIFICANT BUT NOT 27 TO PRACTICALLY IMPORTANT. I SUPPOSE YOU COULD HAVE A 28 RESULT THAT SEEMS, IF IT WERE TRUE, IT WOULD BE

1	PRACTICALLY IMPORTANT, IT'S JUST NOT STATISTICALLY
2	SIGNIFICANT.
3	SO DON'T CONFUSE PRACTICAL SIGNIFICANCE TO
4	STATISTICAL SIGNIFICANCE. STATISTICAL SIGNIFICANCE IS
5	REALLY ABOUT HOW BIG IS YOUR ESTIMATE RELATIVE TO
6	BACKGROUND VARIATION.
7	Q. IN YOUR TWO STUDIES THAT YOU JUST USED AS
8	EXAMPLES, THE TERROR ALERT AND THE ALCOHOL AND CRIME
9	STUDIES, DID YOU GET TO STATISTICAL SIGNIFICANCE?
10	A. YES, IN BOTH OF THOSE IN THE TERROR ALERT PAPER
11	OUR ESTIMATES WERE STATISTICALLY SIGNIFICANT AT THE
12	5 PERCENT LEVEL. IN THE ALCOHOL PAPER, OUR RESULTS WERE
13	STATISTICALLY SIGNIFICANT OFTEN AT THE 1 PERCENT LEVEL.
14	SO AGAIN JUST TO KIND OF FLESH THAT OUT, WHAT
15	WE OBSERVED IN THE WASHINGTON D.C. DATA OUR RESULT WAS
16	BIGGER THAN SAY ALL BUT 5 PERCENT OF THE NORMAL
17	VARIATION IN THE DATA.
18	IN OUR ALCOHOL PAPER, OUR ESTIMATE WAS SORT OF
19	LARGER IN A MAGNITUDE THAN ALL BUT SAY 1 PERCENT OF THE
20	VARIATION IN THE DATA.
21	Q. DOES THAT GIVE YOU MORE CERTAINTY TO THE
22	RESULT?
23	A. YEAH, ALTHOUGH SOCIAL SCIENTISTS OFTEN, YOU
24	KNOW, WE'RE SORT OF A MORE CONSERVATIVE LOT AT LEAST
25	PHILOSOPHICALLY AND METHODOLOGICALLY BECAUSE WHAT IT
26	DOES IS IT GIVES LESS UNCERTAINTY
27	Q. OKAY. SO HOW DO WE TAKE YOUR TWO EXAMPLES AND
28	MAKE THEM A LITTLE MORE CONCRETE IN THE WOMEN ON BOARDS

1 CONTEXT?

2	A. SURE.
3	SO YOU PROBABLY DON'T HAVE ANYTHING QUITE AS
4	INTUITIVELY RANDOM AS THOSE TWO EXAMPLES, BUT WHAT
5	WHAT SOCIAL SCIENTISTS WILL SOMETIMES DO IS THEY WILL
6	SOMETIMES USE LAW CHANGES AS THEIR ESSENTIALLY THEIR
7	NATURAL EXPERIMENT AND SO FOR EXAMPLE, THERE'S A BIT OF
8	A LITERATURE ON NORWAY. WHEN NORWAY IT ITS RULES ABOUT
9	WOMEN ON BOARDS AND SO SOME RESEARCHERS ATTEMPTED TO
10	LOOK AT WHAT HAPPENS BEFORE AND AFTER IN NORWAY ONCE THE
11	LAW COMES IN AND STARTS TO BE BINDING AND THAT
12	LITERATURE, YOU KNOW SOMETIMES FOR ITS CONTROL GROUP OR
13	COMPARATOR GROUP WOULD USE OTHER SCANDINAVIAN COUNTRIES
14	YOU KNOW COMPANIES IN OTHER SCANDINAVIAN COUNTRIES AS
15	COMPARATORS OR COUNTERFACTUALS.
16	Q. BEFORE YOU GO THERE, LET'S GO BACK TO YOUR IN
17	INTUITIVE IDEAL, I THINK YOU CALLED IT?
18	A. YEAH.
19	Q. WHICH IS YOU WOULD HAVE APPLES TO APPLES
20	COMPARISONS?
21	A. SURE.
22	Q. AND YOU WOULD HAVE TWO EQUAL COMPANIES?
23	A. UH-HUH.
24	Q. I THINK YOU TOLD US MAYBE ABOUT TWO HOURS AGO,
25	RIGHT?
26	A. YEAH.
27	Q. YOU HAD TWO COMPANIES THAT WERE OTHERWISE EQUAL
28	AND THEN YOU COULD COMPARE THEM IF ONE HAD THE WOMEN ON

1	THE BOARD, THE OTHER ONE HAD MEN ON THE BOARD?
2	A. SURE.
3	Q. RIGHT?
4	A. SURE.
5	Q. COULD YOU SORT OF ADD A LITTLE MORE MEAT TO
6	THAT SKELETON AFTER HAVING GIVEN US A LESSON ON THESE
7	TWO EXAMPLES OF HOW DESCRIPTIVE REGRESSION AND
8	ACCOUNTING FOR ALL OF THESE UNOBSERVABLE EVENTS WOULD
9	OCCUR. COULD YOU PUT MORE MEAT ON THAT FLESH OR DO WE
10	NEED TO DEVELOP SOMETHING DIFFERENT
11	A. NO, I THINK WE CAN WALK IT THROUGH THE SAME
12	SEQUENCE. SO IF YOU SEE SOMEBODY COLLECTING DATA AND
13	SAY, WE LOOKED AT A SAMPLE OF COMPANIES AND WE BROKE THE
14	SAMPLE UP BETWEEN COMPANIES THAT HAD YOU KNOW X NUMBER
15	OF WOMEN ON THE BOARD VERSUS COMPANIES THAT DIDN'T
16	HAVE HAD FEWER THAN X NUMBER OF WOMEN ON THEIR BOARDS
17	AND THEN WE FIGURED OUT THE AVERAGE, YOU KNOW RETURN,
18	STOCK RETURN OR THE AVERAGE ACCOUNTING RETURN OR THE
19	AVERAGE, YOU KNOW VALUATION, TOW BINS KEY, SOMETHING
20	LIKE THAT.
21	THAT WOULD BE A DESCRIPTIVE STATISTICS
22	APPROACH.
23	BUT IT WOULD IMMEDIATELY RAISE THE QUESTION OF
24	WELL, THE COMPANIES THAT HAVE WOMEN VERSUS THE COMPANIES
25	THAT DON'T HAVE WOMEN ON THEIR BOARD, THEY'RE VERY
26	DIFFERENT. THEIR THE INDUSTRY COMPOSITION IS MUCH
27	DIFFERENT SO FOR EXAMPLE, I BELIEVE THE DESCRIPTIVE DATA
28	SAYS THAT ENERGY SECTOR COMPANIES HAVE VERY FEW WOMEN

AND SO YOU MIGHT SAY, WELL MAYBE DEPENDING ON WHAT TIME
 PERIOD WE'RE TALKING ABOUT, ENERGY HAD, YOU KNOW
 PARTICULARLY GOOD OR BAD RETURNS OR SOMETHING LIKE THAT
 SO YOU WOULD NEED TO ACCOUNT FOR THAT.

5 AND SO WHAT SOME PEOPLE MIGHT DO TO STAY IN THE 6 DESCRIPTIVE STATISTIC THEY MIGHT SAY, OKAY, INSTEAD 7 WE'LL BREAK DOWN THE COMPANIES BY INDUSTRY. BUT WITHIN 8 EACH INDUSTRY WE'LL DO HERE'S THE AVERAGE FOR THE 9 COMPANIES THAT HAVE A CERTAIN NUMBER OF WOMEN VERSUS 10 HERE'S THE AVERAGE FOR THE COMPANIES THAT DON'T HAVE A 11 CERTAIN NUMBER OF WOMEN.

12 BUT THEN OF COURSE, THAT RAISES THE QUESTION, 13 WELL ON THE OTHER DIMENSIONS ARE THESE COMPANIES 14 COMPARABLE. AND MAYBE IT TURNS OUT THAT THE COMPANIES 15 THAT HAVE A LOT OF WOMEN TEND TO BE YOUNGER COMPANIES 16 AND LIFE CYCLE MIGHT SUGGEST THAT RETURNS ARE HIGHER IN 17 SORT OF EARLY YEARS. SO YOU CAN'T TO ACCOUNT FOR THAT.

18 WELL AT THIS POINT ONCE YOU START TRYING TO CUT 19 THE DATA ON MULTIPLE DIMENSIONS, SIMULTANEOUSLY, 20 DESCRIPTIVE STATISTIC PROBABLY ISN'T GOING TO WORK 21 ANYMORE.

22 SO THEN YOU WOULD NEED TO MORE INTO THE 23 REGRESSION CONTACT. SO THE REGRESSION CONTEXT YOU WOULD 24 ATTEMPT TO MODEL YOUR OUTCOME AS A FUNCTION OF WHATEVER 25 WAY THAT YOU'RE MEASURING THE BOARD VARIABLE. THE 26 DIVERSITY ON THE BOARD VARIABLE. AND PUT IN AGE OF THE 27 COMPANY AND PUT IN SOME INDUSTRY VARIABLES AND PUT IN 28 ALL THE OTHER THINGS YOU CAN THINK ABOUT AND THEN SEE

1	WHAT IS THE COEFFICIENT WHAT IS THE PARAMETER ESTIMATE
2	ON THE WOMEN ON THE BOARDS VARIABLE. RIGHT, DOES IT
3	SEEM TO GENERATE STATISTICALLY SIGNIFICANT
4	SYSTEMATICALLY HIGHER RETURNS, LOWER RETURNS ZERO
5	RETURNS THAT SORT OF THING. BUT OF COURSE THEN THAT
6	RUNS INTO OUR PROBLEM ABOUT UNOBSERVABLES, RIGHT SO THE
7	EXAMPLE I USED EARLIER IN THE DAY, WHAT IF IT TURNS OUT
8	THAT EVEN WITHIN AN INDUSTRY AND EVEN WITHIN AN INDUSTRY
9	COMPARING COMPANIES OF SIMILAR VINTAGE, MAYBE THERE ARE
10	JUST SOME COMPANIES THAT ARE MORE PROGRESSIVE THAN
11	OTHERS. AND PROGRESSIVISM MIGHT BE RELATED TO BOTH YOUR
12	LIKELIHOOD OF PUTTING MORE WOMEN ON YOUR BOARD AND
13	VARIOUS OUTCOMES THAT YOU WOULD WANT TO STUDY.
14	GO GET DATA ON PROGRESSIVISM BUT WE'D HAVE TO
15	DEFINE IT AND MEASURE IT AND PROBABLY IS UNQUANTIFIABLE
16	SO OOPS, WE'RE STUCK IN OUR PROBLEMS OF UNOBSERVABLE
17	CONFOUNDERS.
18	UNOBSERVABLE DATA, UNOBSERVABLE VARIABLES JUST
19	AREN'T IN THE REGRESSION AND SO ESSENTIALLY THEY'RE
20	BIASING THE LACK OF HAVING THEM IN THEIR REGRESSION IS
21	BIASING OUR OTHER ESTIMATES FROM THE REGRESSION.
22	Q. SO WHAT DO WE DO?
23	A. WE LOOK FOR NATURAL EXPERIMENTS.
24	Q. EXAMPLE?
25	A. AND SO LIKE I SAID, I DON'T KNOW OF IN THE
26	LITERATURE ANY NATURAL EXPERIMENT THAT IS QUITE AS
27	COMPELLING AS WE MIGHT HAVE IN SOME OTHER AREAS. YOU
28	KNOW, LOTS OF GOOD NATURAL EXPERIMENTS IN THE CRIME

1 AREA. IN THIS CONTEXT I DON'T KNOW IN THE LITERATURE 2 ANY, QUITE AS COMPELLING NATURAL EXPERIMENT. BUT MAYBE 3 SLIGHTLY LESS COMPELLING, BUT STILL BETTER THAN JUST THE REGRESSION APPROACH WOULD BE TO LOOK AT LAW CHANGES AND 4 5 ATTEMPT TO SEE, OKAY, WE'RE GOING TO COMPARE A PERFORMANCE BEFORE AND AFTER, ONCE THE LAW SORT OF 6 7 APPLIES, BUT OF COURSE WE STILL NEED TO WORRY ABOUT OUR 8 COUNTERFACTUAL SO IMAGINE PASSING A LAW THAT SORT OF 9 SHOCKS THE NUMBER OF WOMEN ON A BOARD IN A PERIOD WHERE THE STOCK MARKET IS JUST GOING UP. RIGHT. IT'S GOING 10 11 UP INDEPENDENTLY. IF WE JUST LOOK AT BEFORE AND AFTER WE SAY OH, MY GOODNESS STOCK PRICES ROSE. WELL, IS IT 12 BECAUSE OF THE BACKGROUND TREND OR IS IT BECAUSE OF 13 14 ADDING WOMEN. SO WE REALLY DO NEED THAT COUNTERFACTUAL 15 OR THAT PLACEBO GROUP THAT CONTROL GROUP TO ESSENTIALLY 16 NET OUT THAT BACKGROUND TREND. THAT'S WHAT THE COMPARATORS ARE GOING TO BE FOR HERE. AND SO AS I 17 18 STARTED TO SAY, IN THE HALF A DOZEN PAPERS OR SO 19 EXAMINATION HIGH QUALITY PIPERS THAT EXAMINE SORT OF THE 20 NORWEGIAN EXPERIENCE THERE ARE A COUPLE OF DIFFERENT APPROACHES, SOME OF THE APPROACHES IN SOME OF THE PAPERS 21 22 USED OTHER SCANDINAVIAN COUNTRIES AS -- COMPANIES IN 23 OTHER SCANDINAVIAN COUNTRIES AS THEIR COMPARATORS, I 24 BELIEVE ONE OF THE PAPERS USED FIRMS THAT FOR 25 IDIOSYNCRATIC REASONS WEREN'T QUITE AFFECTED BY THE --26 BY THE NORWEGIAN RULE. 27 SIMILARLY THERE'S A PAPER THAT WAS IN THE 28 JOURNAL OF CORPORATE FINANCE IN 2020 THAT ATTEMPTED TO

ESSENTIALLY USE THIS APPROACH FOR THE CALIFORNIA RULE
 AND SEEING WHAT THE MARKET REACTION WAS TO CALIFORNIA'S
 PASSAGE OF THE DIVERSITY REGULATION FOR BOARDS.

4 AND THERE ARE TWO YOU NEED TO MAKE SURE -- YOU 5 NEED TO MAKE SURE THAT YOU'VE GOT YOUR COMPARATORS, RIGHT. YOU NEED TO MAKE SURE THAT WHAT IF THERE WAS 6 7 JUST GENERAL CHANGES IN CORPORATE RETURNS IN THIS TIME 8 PERIOD SO YOU NEED MAYBE NONE CALIFORNIA COMPANIES TO 9 ACCOUNT FOR THAT. OR IT MIGHT EVEN BE PARTICULARLY 10 TRICKY, YOU MIGHT SAY, WELL THE -- THE LAW IN CALIFORNIA 11 ALTHOUGH IT CHANGED BOARD COMPOSITIONS, PERHAPS IT ALSO 12 SENT A SIGNAL TO THE MARKET ABOUT OTHER STUFF RIGHT SO YOU COULD IMAGINE, YOU COULD IMAGINE PEOPLE BEING 13 14 WORRIED TO SAY MAYBE THIS MEANS CALIFORNIA I SHOULDN'T 15 SAY WORRY, I DIDN'T PUT A NORM GIVE VIEW ON THIS, THEY 16 MIGHT JUST EXPECT THAT THIS SPEAKS TO OTHER CHANGES IN THE FUTURE. 17

AND SO YOU MIGHT WORRY THAT WELL OKAY, WE NEED TO ACCOUNT FOR THAT POSSIBILITY SO MAYBE THE NONE CALIFORNIA COMPARISON COMPANIES MIGHT NOT BE QUITE ADEQUATE BECAUSE YOU NEED TO BE ABLE TO PULL OUT THIS HEY THE MARKET THINKS THAT CALIFORNIA IS GOING TO BE DOING OTHER THINGS AND IS REACTING TO THE BOARD RULE AS A PROXY FOR A BUNCH OF OTHER STUFF.

Q. IS THE CHANGE IN THE LAW COMPARISON WHEN YOU
TALK ABOUT NORWAY VERSUS THE OTHER SCANDINAVIAN
COUNTRIES, IS IT ALSO SIMILAR TO SUGGEST MAYBE COMPARING
IT TO OTHER STATES THAT HAVEN'T MADE THESE CHANGES IN

1 THEIR LAWS?

2 Α. SURE. ALTHOUGH, AGAIN YOU NEED TO BE WORRIED 3 SO IN THE CALIFORNIA EXAMPLE, THERE HAVE BEEN SOME CORPORATE LAW SCHOLARS WHO RESPONDED TO THE PAPER 4 5 LOOKING AT THE CALIFORNIA LAW CHANGE WHO SAID WE CAN'T -- WE CAN'T ESSENTIALLY SAY THAT ALL OF THE CHANGE 6 7 IN THE STOCK RETURNS WAS DUE TO THE BOARD RULE BECAUSE 8 THESE CORPORATE SCHOLARS SUGGEST, FOR EXAMPLE MY 9 COLLEAGUE AT PENN SUGGESTED MAYBE WHAT THIS DOES IS THIS 10 SIGNALS -- SIGNALS TO THE MARKET THAT CALIFORNIA IS --11 IS PULLING BACK ON THE INTERNAL AFFAIRS DOCTRINE OR 12 SOMETHING LIKE THAT THE.

AND SO -- SO IT MIGHT NOT TELL YOU EXACTLY WHAT THE BOARD RULE DID, IT MIGHT BE A COMPOSITE OF WHAT THE BOARD RULE DID AND WHAT THE BOARD RULE SIGNAL TO THE MARKET OTHERWISE.

SO AGAIN THIS REALLY UNDERLINES THE IDEA THAT 17 18 YOU NEED TO HAVE GOOD COMPARATORS, GOOD COUNTERFACTUALS 19 THAT CREATE THAT COUNTERFACTUAL SITUATION, RIGHT. THE 20 WAY THE JOURNAL OF CORPORATE FINANCE ARTICLE ATTEMPTS TO HANDLE IT IS THEY TRY TO DO SOME COMPARISONS BETWEEN 21 22 CALIFORNIA FIRMS THAT WERE ALREADY IN COMPLIANCE LARGELY 23 INDEPENDENTLY OF THE LAW. THEY JUST ALREADY HAD WOMEN 24 ON THE BOARDS. COMPARE THEM, COMPARE THOSE FIRMS TO 25 FIRMS THAT WERE NOT IN COMPLIANCE AND THAT WAS THEIR 26 ATTEMPT TO GET THE COUNTERFACTUAL. 27 Q. SO I WOULD LIKE TO LEAD INTO -- OUR TIME IS

28 RUNNING VERY SHORT NOW, BUT I'D LIKE TO TRANSITION INTO

1 THE MARKET OF STUDIES THAT ARE OUT THERE ON THE SUBJECT 2 IN WHICH YOU HAVE ALSO DONE A LOT OF RESEARCH AND 3 WRITTEN UPON, INCLUDING FOR YOUR ASSIGNMENT IN THIS 4 CASE, IS THERE SOMETHING YOU CAN TEASE US WITH, AS YOU 5 TAKE US FROM THE STUDIES -- DO WE HAVE A TEASER TO QUIT HERE, WHICH MEANS -- LIKE I TRIED TO LEAD YOU RIGHT NOW, 6 7 DID YOU FIND THAT SOME OF THESE STUDIES OVERALL, IS 8 THERE A GENERAL STATEMENT YOU WANT TO MAKE WE CAN START 9 WITH IN THE MORNING?

SO I WOULD SAY THE GENERAL OVERALL STATEMENT I 10 Α. 11 WOULD LIKE TO MAKE IS THE LITERATURE GENERICALLY ON 12 WOMEN AND BOARDS IS RELATIVELY UNSOPHISTICATED SO WHAT I MEAN BY THAT IS THESE SORT OF NATURAL EXPERIMENTS THAT I 13 14 WAS TALKING ABOUT, WHICH AGAIN BY THE LATE 1990'S HAD 15 BECOME, YOU KNOW, CERTAINLY THE STANDARD IN EMPIRICAL POLICY ANALYSIS, EMPIRICAL INFERENCE ARE LARGELY ABSENT 16 IN THE LITERATURE AS A WHOLE. 17

AND, YOU KNOW, SO THERE ARE SOME COMMENTATORS
WHO HAVE ATTEMPTED TO FOCUS ON PANEL DATA STUDIES, SORT
OF SUGGESTING THAT PANEL DATA IS SOMEHOW A SOPHISTICATED
APPROACH TO GET PAST SOME OF THESE PROBLEMS.

22 PANEL DATA IS A NECESSARY BUT NOT SUFFICIENT
23 CONDITION FOR ENGAGING IN A LOT OF THESE SOPHISTICATED
24 TECHNIQUES THAT I'VE TALKED ABOUT.

SO I MIGHT AS WELL HIT PAUSE ON THE TEASE. TO
DEFINE PANEL DATA, OR SOMETIMES REFERRED TO AS
LONGITUDINAL DATA, ESSENTIALLY MEANS THAT YOU'VE GOT
MULTIPLE OBSERVATIONS FOR AN ENTITY OVERTIME SO IN A

1 CORPORATE FINANCE CONTEXT IT MIGHT MEAN THAT YOU'VE GOT 2 YEARLY OBSERVATIONS FOR A FIRM. SO I'VE GOT 600 FIRMS 3 YEARLY OBSERVATIONS OVER 10 YEARS, AND SO I'VE GOT 6,000 OBSERVATIONS, RIGHT. THAT'S WHAT PANEL DATA MEANS. IT 4 5 JUST MEANS I'VE GOT MULTIPLE OBSERVATIONS FOR A GIVEN ENTITY OVER TIME. 6 NOW YOU CAN SEE WHY THIS LONGITUDINAL DATA IS 7 IMPORTANT FOR DOING THE KIND OF MORE SOPHISTICATED 8 9 METHODOLOGIES I'VE TALKED ABOUT. JUST THE WAY WE TALKED 10 ABOUT THEM, BEFORE AND AFTER, THAT AUTOMATICALLY MEANS 11 THAT YOU'VE AT LEAST GOT TWO OBSERVATIONS FOR A GIVEN 12 ENTITY. RIGHT. SO PANEL DATA IS GOING TO BE AN IMPORTANT 13 COMPONENT OF THESE ANALYSES, BUT IT'S NOT ENOUGH, RIGHT, 14 15 BECAUSE WHAT YOU NEED IS YOU NEED THIS OUASI 16 RANDOMIZATION OR THIS SHOCK SO TO SPEAK, THIS NATURAL EXPERIMENT SO TO SPEAK SUCH THAT THE BEFORE AND AFTER 17 18 ARE MEANINGFUL IN ANY REAL SENSE. 19 Ο. WHY DON'T WE PICK IT UP IN THE MORNING IF THE 20 COURT WOULD LIKE TO TERMINATE NOW. 21 THE COURT: WE WILL. 22 IF WE COULD RECONVENE AT 10:00 TOMORROW 23 MORNING. 24 THE COURT IS IN RECESS. THANK YOU. 25 26 27 28