

Transcript: Improving Equality of Opportunity in America: New Evidence and Policy Lessons

Professor Raj Chetty

March 4, 2015

[Lonnie:] Welcome. I'm Lonnie Berger, the Director of the Institute on Poverty, and on behalf of IRP, the Department of Economics, and the Lampman Family, I want to thank you for joining us for the 16th Robert. J. Lampman Memorial Lecture. Unfortunately, the Lampman family is not able to be with us today, but they have assured me that they are very much looking forward to watching the lecture online as soon as we can get it posted, and they are clearly here with us in spirit. We are grateful to the family for their ongoing support of the University and of this lecture series, and of IRP.

In just a minute I will introduce today's speaker Raj Chetty, but first I want to say a few words about Robert Lampman, who the lecture is named for. Bob was a professor of Economics at UW-Madison for over 30 years, and he was the founder and the guiding spirit of the Institute for Research on Poverty. By all accounts, he was an exceptional and unique scholar, teacher, mentor, and individual. His legacy looms large throughout the halls of the Social Science building on campus, and indeed in poverty research throughout the nation and throughout the world. Bob was well-known as the intellectual architect of the U.S. war on poverty. He served on the Council of Economic Advisers from 1961 to 1963, and he is credited in 1963 for writing a memo on poverty, the income distribution, and the implications of economic expansion in the U.S. on the lives of the poor, that eventually was forwarded to President Kennedy. After that happened, Bob was assigned by the chair of the Council of Economic Advisers to begin examining responses to poverty and designing potential antipoverty programs. So, his subsequent work ended up spanning the gamut of social welfare programs including health, education, means-tested transfers, and social security programs. When evaluating public policies or considering potential policies, he really came to be known for asking the simple question of "What does it do for the poor?" In the early- to mid-1960s, Bob starting working with federal officials in Washington D.C. to establish a poverty research center that would be the nation's first center dedicated to the study of the nature, causes, consequences, and cures of poverty and social inequality. So as a result, on March 23, 1966, the University of Wisconsin was awarded a grant of \$1.7 million to start this center, and \$1.7 million at the time was a pretty considerable sum of money for a research center, and is not a bad sum of money today for a research center. In April of that year, about a month after the grant came out, Bob write, "Certainly one of the justifications for a large-scale grant to a single institution, as opposed to a whole set of small project grants scattered out all over the place, is that you reach a critical mass of research interests, when you get a group of people together who have similar interests, but different backgrounds. We hope that there will be more than merely individual contributions which accumulate. We hope there will be a multiplying process where a chemical reaction between and among the various researchers." Almost 50 years later (we celebrate our 50th anniversary next year), IRP is still going strong, and IRP is doing our best to stay true to Bob's faith.

After Bob's death in 1997, IRP and the Lampman family established the Lampman Lecture Series. We organize it in cooperation with the Department of Economics on campus, and it is done, obviously, in Bob's honor. The series features eminent policy scholars, and it is intended to address the topics which Bob devoted his intellectual career to; poverty and the distribution of income and wealth. Today's speaker, Raj Chetty, is among the very top scholars in the world focusing on these areas. I'm sure most of you, if not all of you, are familiar with at least some of his work. To give you the brief bio, Raj is a Professor of Economics at Harvard, he is codirector of the public economics group at the National Bureau of Economic Research, and he is Editor of the Journal of Public Economics. His research combines empirical evidence and theory to inform the design of more effective government policies, and his work in the areas of tax policy, unemployment, and education has been widely cited in a variety of media outlets, and used in congressional testimony. Raj received his PhD in 2003 from Harvard at the amazingly young age of 23. He is also one of the youngest tenured professors in Harvard's history. He has been named one of the top economists in the world by the New York Times and the Economist magazine. He was awarded a MacArthur genius fellowship in 2012, and he recently became one of the youngest recipients of the John Bates Clark medal, which is given by the American Economic Association to the best American economist under age 40. Much of Raj's recent work has focused on equality of opportunity, and specifically looking at how we can give children from disadvantaged backgrounds better chances of succeeding. Today, he is going to talk about some of this recent work, focused on improving equality of opportunity in America. It is a great pleasure to introduce Raj. Thank you for the amazing work that you do, and thank you for taking the time out of what I know is a very busy schedule to share some of it with us. And now, please join me in welcoming Raj Chetty.

[applause]

[Chetty:] Thanks so much, it's really a pleasure to be here in Madison and it's especially a pleasure to be here to talk about equality of opportunity because Madison in many ways for me has been key in the opportunities that I have because my father actually came to Madison from a small village in India in the 1960s to do his PhD with Arnold Zellner who some of you will know in econometrics here at Madison. My mother did her medical training here. So, my parents, like many other immigrants came to the U.S. in search of the American Dream and so that's what I want to start off talking about. The American Dream means many different things to many different people but I want to distill it here to a simple statistic that we can measure empirically. Let's think about the probability that a child born to parents in the bottom fifth of the U.S. income distribution makes the leap all the way to the top fifth of the U.S. income distribution, so sort of the Horatio Alger rags to riches story.

So what does that probability look like in a few developed countries around the world? In the U.S., that probability is 7.5%, so 7.5% of the kids starting out in the bottom fifth reach the top fifth. That compares with 9% in the United Kingdom, 11.7% in Denmark and 13.5% in Canada. Now, when people look at these statistics initially, some people react by saying, well, even in Canada it doesn't look your odds of success are all that high, right? You have only a 13.5% chance of succeeding, in a sense. But you have to remember of course that, unfortunately, no matter what you do, you can't have more than 20% of people in the top 20%. So, the upper bound on what this statistic could be in a world with perfect mobility where your parents have no impact at all, that number would be 20%. So, actually, these differences are quite large. One way to think about it is that your chances of achieving the American Dream are almost two times higher if you're growing up in Canada than if you're growing up in the United States.

Now, these differences across countries have been the focus of policy discussion. But I think there are a number of issues with making comparisons across countries like that. There are lots of differences

across countries of course, starting with the basic fact that the income distribution are very different in different countries, so it might be easier to go from the bottom quintile to the top quintile in a country with less inequality like Denmark or Canada, relative to the United States. So what I'm actually going to focus on primarily in this lecture is the fact that upward mobility varies even more within the United States than it does across countries.

In recent work with my colleagues Nathan Hendron, Pat Klein, and Emmanuel Saez, we calculate upward mobility for every metro and rural area in the United States, and here we're using anonymous earnings records on 40 million children in the U.S. born between 1980 and 1993. This is an example of, I think, a very important trend in economics, which is the application of big data to public policy questions, which I think increasingly, we'll see more and more of in the coming years. So, in what I'm going to show you -- I'm going to show you a lot about variation in outcomes across areas, across neighborhoods -- one thing to keep in mind throughout is that I'm going to classify children based on where they grew up. So, when I refer to locations, I mean "where did someone grow up," not where they're living as adults. And, of course, that's important, because you might have grown up in Madison, but you're living in New York as an adult when we're measuring your income. So in that case, we're going to count you as part of the Madison data in what I'm going to show you.

So, what we do is divide the U.S. into 740 metro and rural areas -- what are called "commuting zones." And in each of those areas, we compute the same statistic that I started out showing you: what are your odds of moving from the bottom fifth to the top fifth, conditional on growing up in that area. And this heat map shows you the results of that analysis. Lighter colored areas are areas with higher levels of upward mobility, and the darker red areas are areas with lower levels of upward mobility. So you can see that places with the top decile in the middle of the map -- for instance, rural Iowa or other parts of the Great Plains -- in some of those areas, your odds of moving from the bottom fifth of the national income distribution to the top fifth exceed 16.8%, so that's higher than the numbers we saw for Canada and Denmark, for example, right? But on the other end of the spectrum, if you look at places like Atlanta or Charlotte or the Milwaukee metro area, your odds of moving from the bottom to the top are something like 4.5%, lower than any developed country for which we currently have data. So even within the United States, there's this incredible amount of variation and much of this variation, as you can see in this map, is across broad regions, like the Southeast versus the Great Plains versus the West Coast, but there's also quite a bit of variation across relatively nearby areas. So look for instance at Ohio versus Pennsylvania, two states that we think have relatively similar demographic and economic structures, yet Ohio is very low in terms of rates of upward mobility and Pennsylvania actually looks quite good. Another example to illustrate the granularity of the data; take a look at South Dakota and North Dakota -- those two states generally have the highest levels of upward mobility in the U.S. with a few exceptions in the lower left corner of South Dakota which are the darkest red colors on the map. What are those places? They're some of the largest Native American reservations in the U.S. where we know, historically, there's chronic intergenerational poverty and you're picking that up in these data.

Now, we can zoom in further, and here I'm zooming in to an area that's relevant for today's talk -- here in the Milwaukee and Madison area. We're constructing these statistics now by county in this area. And what you can see is Milwaukee County actually has exceptionally low levels of upward mobility. We've looked at these statistics for many places and 3.2% is one of the lowest numbers you'll see. So only 3% of kids who start out in the bottom fifth in Milwaukee end up making it to the top fifth of the U.S. income distribution, but that looks really different in quite nearby counties. So, if you take Waukesha for example, that probability is 13.2%, so dramatically higher, 4 times higher, your odds of moving from the bottom fifth to the top fifth. And I should emphasize that I'm focusing on this statistic of the bottom fifth

to the top fifth just for concreteness, but the patterns are very similar if you were to use other measures of upward mobility. So, “what are your chances of crossing the national median,” or “what is your average income level,” all of these things would generate very similar patterns. What we wanted was one concrete set of statistics to focus on. So, even in relatively small areas, there’s substantial variation in children’s chances of moving up in the income distribution, just as a descriptive fact in the data.

Naturally the question of interest, both for academic research and for public policy is: “Why does upward mobility differ so much across areas and what might we be able to do about it from a policy perspective?” The first main point I want to make here is that much of the variation in upward mobility that we’ve seen in these maps across areas is due to the causal effects of childhood environment. So, importantly, there are two important points in that statement: causal effects and childhood environment. So, first of all, you might have thought that maybe some of these differences across areas are just because different people live in different places. So, for example, Atlanta has very low rates of upward mobility. Salt Lake City or San Jose are much higher. Well, the people who live in Salt Lake City are very different from the people who live in Atlanta, so maybe this is about differences in racial backgrounds or demographics or the way in which people are sorting and not something about the causal effect of growing up in different places. So, what I’m going to show you now is a series of results that actually suggests that if you take a given child and put that child in Salt Lake City, or in Waukesha County, instead of Milwaukee County, you are going to see very different outcomes for a given child; it’s not just about different people living in different places. The second thing I’m going to show you is that this variation seems to be driven primarily by differences in childhood environment as opposed to differences in local labor markets, or the types of jobs that are available, or things like that.

So, how are we going to do that analysis and make those two points? We document these results in a paper that we’ll release in a couple weeks by studying families that move between different counties. One way you can think about the analysis that I’m going to show you in simple, nontechnical terms is to ask whether children who move from Milwaukee to Waukesha end up doing better as adults. In particular, what we’re going to do here is to study 8 million families that move across counties in the U.S. with children of different ages. We’re going to exploit variation in the age of children when families move between areas. Let me show you how this works with a simple example: let’s say we take a set of kids whose families move from Milwaukee County to Waukesha County, and let’s denote the average outcome in Milwaukee County -- say the average level of income -- by zero on this graph, and Waukesha County, which has better outcomes on average, let’s call that 100%. For example, if mean income, just to make up a number in Milwaukee is 30 thousand dollars for kids who grow up there and Waukesha is 40 thousand dollars, then zero corresponds to \$30,000 and 100% corresponds to \$40,000; that’s the way you want to think about this. Now on the x-axis here is the age of the child when the parents make that move from Milwaukee to Waukesha County. I’m using this particular example just to be concrete, but now I’m going to use data from all such moves, the 8 million moves in the dataset. The first thing we see is that children whose families move from Milwaukee to Waukesha (or from one area to another area more generally) when they’re exactly 9 years old; they get 70% of the gain from growing up in Waukesha from birth. So if the kids who grow up in Waukesha from birth are earning \$40,000, and the kids who grow up in Milwaukee are earning \$30,000, then the kids who move when they’re nine years old between those two places end up earning \$37,000 on average; they get 70% of the gain. Now let’s repeat that analysis for all the other ages of children when the parents move. What do you see? You see this very clear declining pattern. So, if you move at later ages, you get less and less of the gain from moving to that better neighborhood. And if your parents move after you’re around 22 or 23 years old, you get no gain at all. What we call this is childhood exposure effects. Every extra year that you spend in

the better environment seems to make you look more like the place to which you move. You get more and more of the gains of growing up in that place.

A couple of points about this: first, notice that this relationship looks very linear until age 23. So what that means is every extra year matters, not just the earliest years. We're not able to go before age 9 at this point because our dataset only starts in 1996, and so we're not able to go further back at the moment, but insofar as we have data, it looks like every extra year matters when you're an adolescent or when you're younger. That's a very important point for those of you interested in child development, because it suggests that it's not merely the earliest years that matter. You hear a lot about early childhood education and pre-K interventions and so forth, and while all of that is quite likely to be very valuable, this evidence also suggests that if you improve environments when kids are 15 years old, you end up having quite a substantial impact.

A second point to make here is; essentially what we're doing if you think about how we're interpreting this, we're comparing families who move from place A to place B with kids of different ages, and we're interpreting that age variation as telling us something about the causal effect of arriving one year earlier. So what's the assumption you need in the background to interpret those estimates as causal -- it's that the people who move when their kids are young are comparable to the people who move when their kids are older. Does that hold or not? That's something we've spent a lot of time investigating; let me give you one piece of evidence that I think suggests that that assumption does in fact hold, and this reflects a causal effect. What we do is we compare siblings within the same family. So, take a family that's making this move and they have a 5-year-old and a 10-year-old. We compare the outcomes of the 5-year-old to the 10-year-old and we ask, when you move to a better area, does the 5-year-old do better than the 10-year-old, and, in particular, is the difference in the siblings' outcomes proportional to the gap in their ages? It turns out you find exactly this pattern when you exploit strictly within-family cross-sibling variation. And so that type of variation knocks out the most basic selection effects that you might worry about, that the types of families who move when their children are young are different from the families who move when their kids are older. More generally, this seems to be a very robust pattern in the data, where we've done a number of other placebo tests and so on, and we're convinced that this actually reflects the causal effect of spending another year in a better or worse environment. It appears to be the case that childhood environment, childhood exposure, really plays a significant role in explaining this variation in mobility across areas. So, that's useful to know, but what you ultimately want to know then, is what is it that the lighter colored areas are doing that are making kids' outcomes much better, that are generating much better kids' outcomes, than the darker colored areas like Milwaukee County.

So, what are the characteristics of high-mobility areas? I'm going to start with just a descriptive analysis by showing a you a set of correlates of differences in upward mobility. Before getting into causal mechanisms, I'm just going to start with a set of correlations. We've looked at many correlations in the data and I'm going to present the five strongest correlates we've found.

The first is segregation. We find that places with more racial and income segregation are associated with significantly less mobility. Another way to look at this is sprawling cities, cities with longer commute times, places like Atlanta which is a really big car city basically; they tend to have much lower levels of upward mobility. This pattern is very easy to see in the data and I just want to make this point visually to you by showing you what segregation patterns look like in a couple of cities. This map here is a dot map constructed by a demographer named Dustin Cable using Census data, and it shows you visually what racial segregation looks like in the City of Milwaukee. The way this map is constructed is every person in Milwaukee is represented by one dot, and the dots are colored so that whites are blue, blacks are green,

Asians are red, and Hispanics are orange. What you can see here is that Milwaukee is an incredibly segregated city, right? The green dots are completely in one area, not interspersed at all with the blue dots, and then the orange dots are in a completely different part of the city, and so Milwaukee -- naturally, you can construct various indices of segregation -- however you measure it, is going to look like an extremely segregated city, and consistent with that, it has one of the lowest rates of upward mobility in our data. Now compare that with Sacramento: Sacramento is one of the cities with the highest levels of upward mobility in the U.S., and Sacramento has a similar minority share to Milwaukee; the total number of Hispanics and Blacks in Sacramento is similar to Milwaukee. But look at the dots here, they're much more interspersed. Sacramento is a much more integrated city than Milwaukee. And that's the type of pattern -- I'm giving you examples with two cities here, but more generically -- we find a strong relationship between the degree of integration in cities and rates of social mobility. That's the first correlated factor that we identify.

The second factor is income inequality. We find that places with a smaller middle class, that is, fewer people between the 25th and 75th percentiles of the national income distribution, have much less upward mobility. That fact is particularly important, I think, for the political debate, because it suggests that there might potentially be a link between inequality and social mobility. And that's relevant because, as you all know, there's increasing inequality in the U.S., and some people have the view that we should try to do something about that and redistribute resources more equally. And other people have the view that if you made a lot of money, you're entitled to keep it; the government shouldn't be trying to do too much to reduce ex post inequality. So I think you can reasonably have different perspectives on that debate. But, no matter what your view on that, most Americans believe in the ideal of social mobility and equality of opportunity. That is, a child's chances of succeeding shouldn't depend too heavily on which parents they happen to be born to. So, if there's a link between social mobility and inequality, as this relationship suggests, it suggests that even if you don't care about ex post inequality per se, you might care about it insofar as it's reducing social mobility and affecting the American Dream. Now, an interesting caveat to that is that we don't find a very strong correlation between another measure of inequality -- upper tail inequality, so, take top 1% income shares for example -- they don't seem to be that strongly correlated with differences in mobility across areas. And you could kind of see that from the map I put up initially. So think about the Bay Area in California for example, has some of the highest rates of social mobility in the U.S., but, as you all know, the Bay Area also has some of the richest people in the U.S. thanks to the tech boom, so you can see why you don't end up with such a strong correlation between those two variables.

The third factor that's strongly correlated with differences in mobility is school quality. As might be intuitive, we find that places that spend more on public schools, or places that have smaller classes, or higher test scores conditional on income, these places tend to have higher levels of social mobility consistent with the view that schools might play a key role here, that human capital really matters.

The last two factors we identify both come from the sociology literature. It turns out the single strongest correlation in the data is with the fraction of single parents in an area. Areas with more single parents have substantially lower levels of social mobility. The correlation between various measures of social mobility, upward mobility, and the fraction of single parents is something like 0.7 -- very high correlation for one variable like this. Now, in interpreting that fact, it's important to keep an important caveat in mind, which is you might think that the explanation is just that growing up in a two parent family is beneficial for you, and so when you live in an area with a lot of single parents, because your own parents, you yourself are growing up in a single parent household rather than a two parent household, maybe that's why you have a lower chance of moving up in the income distribution and succeeding. So,

while some of that effect is clearly there in the data, it's important to note that you find a very strong correlation even for kids whose own parents are married. What I mean by that is, if you take a child whose own parents are married, he's growing up in a two parent household, and you put that child in a community where there are a lot of single parents, that child is less likely to move up in the income distribution. So this is not just the direct of whether your own parents are married or not, it again seems to have something to do with properties of the community.

The fifth factor, which is related to that, is what sociologists call "social capital", an idea that Coleman discussed in economics, Bob Putnam has popularized. The idea of social capital is to measure the amount of social cohesion in an area or to what extent will other people help you out if you're not doing well. One way to think about it is the old adage that it takes a village to raise a child. If you're not doing well, maybe somebody else will help you out. This concept of social capital as I mentioned was popularized by my colleague Bob Putnam in a very well known book called *Bowling Alone*, and the reason for the title of that book is that one of the proxies for social capital that Bob Putnam uses is the number of bowling alleys in an area. And to my amazement, the number of bowling alleys in an area is in fact very highly correlated with rates of social mobility in our own data. But that also illustrates, I think, a very important nuance in what I'm showing you here in that all of these are correlations rather than causal effects, because I'm pretty sure the policy lesson here is not that we should be building more bowling alleys to increase upward mobility in the U.S.

And so, motivated by that, what I want to turn to next is that critical question of what policy changes can actually improve social mobility? What are the causal mechanisms that are at play here? There are lots of policies that one can consider and I think there's a lot of research remaining to be done in understanding essentially the model that describes why different areas have different levels of social mobility, and there are some people here in the audience who are doing important work related to these issues. For illustrative purposes, I'm going to focus here on two types of policies that are motivated by some of the correlations that I showed you. So, I'm going to think about policies that try to reduce segregation; essentially affordable housing policies. We spend about 50 billion dollars per year on various affordable housing efforts, things like subsidized housing vouchers, housing projects, tax credits for developers, things like that. Broadly, I want to get a sense of whether those types of policies can be effective in changing social mobility. And then second, I'm going to go in a different direction and talk about schools and improving education, and I'm going to focus specifically on one angle there, which is teacher effectiveness and show you how that also can have, I think, quite significant causal effects on rates of social mobility. Now I also want to stress that while I'm focusing on these two policies here, this is not to imply that the other factors might not be important. It could well be that family stability and social capital are even more important than these two things. There's nothing I'm going to show you that would contradict that. But one challenge is that these other factors historically have proven much harder to change through policy than things like schools or maybe even the degree of segregation. You might think that family stability really matters or you might think that social capital really matters but what are you going to do in terms of government policies to try to make families more stable or to try to improve the amount of social capital in an area. I think we're not as clear yet on what those policy solutions might be. So that's part of the reason I focus on these two things which are really part of the current policy set.

Let's talk about affordable housing and integrating neighborhoods, trying to reduce segregation. So, one way you might go about trying to do that, which is related to policies we try to implement in the U.S., is to give low-income families subsidized housing vouchers and try to encourage them to move to better areas. In the U.S. we have a program called Section 8 Housing Vouchers on which we spend about \$20

billion a year, and these essentially give families subsidized housing vouchers that allow them to rent apartments in areas where they otherwise might not have been able to afford to live. Now there is a very famous social science experiment called the Moving to Opportunity experiment implemented by the Housing and Urban Development Agency which sought to evaluate the effects of such vouchers. And it did that using, essentially, a scientific experiment; giving vouchers to families using a randomized lottery. This experiment involved 4,600 families in five major cities in the U.S., and was implemented in the mid-1990s, and it's been studied for a very long time by a number of economists and sociologists. Traditionally, people have thought that the MTO experiment did not have very significant impacts on economic outcomes. But I'm going to present a reanalysis of that data which actually is more consistent with the quasi-experimental findings I was showing you earlier, which suggests that the MTO experiment did in fact have quite significant effects.

To make this concrete, let me show you what the MTO experiment actually did. Let's take one of the sites, New York. In New York, Families who were randomly assigned to the control group, they typically lived -- one common place they lived -- was the Martin Luther King Towers in Harlem which is a very big public housing development in Harlem. Families who were assigned to experimental voucher group -- these families received a subsidized housing voucher worth about \$6,000 on average -- they were required to move to a census tract with less than a 10% poverty rate in order to use this voucher. So it came with this requirement that you had to move to a lower poverty area. A common destination for those families was an area called Wakefield in the North Bronx, which is about 10 miles away from Harlem. The point here is that these are very local moves. Essentially what I'm asking is how different are the outcomes of kids who by chance ended up growing up in Wakefield in the Bronx, as opposed to in Harlem. The way we analyze that question is by taking the MTO data, linking it to tax records -- the large dataset that I started out with -- in order to be able to look at the long term effects of this experiment on children's outcomes twenty years later. Here's what we find, to give you just a brief summary: we find that children who move to these lower poverty areas, for example, the Bronx, when they were young -- for instance below age 13 -- they do much better as adults. In particular, they have 30% higher earnings on average relative to kids in the control group, the kids who stayed in the MLK towers, that 30% gain translates to a cumulative -- if that 30% increase is sustained over a lifetime -- that would add up to an increase in earnings for the average kid of \$300,000. If you were to discount that back to present value at the point of the intervention, it would be worth \$100,000, so it's like generating \$100,000 up front from this move. They're also -- these kids -- on a spectrum of other outcomes they're doing much better: they're 27% more likely to attend college, they're 30% less likely to become single parents, they're living in better neighborhoods themselves as adults, so in a variety of ways they look a little more like middle class families and their own kids as a result are growing up in better environments. So some of the impacts of this initial intervention look like they're going to persist to the grandkids of the original MTO participants. So, quite substantial effects on the youngest children who were involved in the Moving to Opportunity experiment. That set of findings, which we'll have out in a paper in a couple of weeks was not evident in prior research which had concluded that the MTO experiment did not have that much of an effect. Now, consistent with prior work, we find that moving had little effect on the outcomes of children who were older at the point of the move, so children who were teenagers for example. And that, if you'll recall, is very consistent with that downward sloping pattern I was showing you, that it's all about childhood exposure. If you move when you're young, you get much larger benefits than if you move at older ages. Now we also find that moving had no effect on the parents' earnings, which is a fact that's been documented before and, as I've said, this reinforces the conclusion that it's really childhood exposure that seems to be a key determinant of differences in upward mobility; the childhood environment's really critical.

What can we learn from the Moving to Opportunity experiment in terms of housing policy or how we might be able to integrate neighborhoods to improve outcomes? I think one lesson you can draw from this is that moving to a mixed income neighborhood improves outcomes for low-income children. That's one piece of the puzzle, but I don't think it's adequate to conclude that that's actually a good policy, because you've got to think about what the equilibrium effects of such a policy might be and what the spillover effects might be on the people who were already living in these areas. Now, it's very difficult to analyze that experimentally, because if you move one person in and you try to detect the effects on all the other people who were already living there, it's like you have a needle in a haystack type of problem -- you can't detect the effect on everyone else. So while I can't show you evidence directly from the MTO Experiment on how it's affecting higher income people, what I can tell you is this: from our quasi-experimental analysis, where we estimate the causal effects of growing up in each county in the U.S., we find that mixed income areas produce better outcomes for children from low-income families -- that's consistent with the findings from the MTO experiment. We also find that those mixed income areas produce, if anything, slightly better outcomes for kids growing up in high-income families. So it does not look like living in a more integrated neighborhood, at least based on that evidence, is detrimental for kids from rich families. So it doesn't look like a zero-sum game, where you bring one guy in and he does better, and then the guy who was already there ends up doing worse. This looks more consistent with, maybe, a role model effect where if you've got a bunch of people doing well, they help others move up without being hurt themselves; that's one potential model that would be consistent with the data. The bottom line here is that it's possible -- although I don't think the evidence is fully conclusive at this point to make this statement -- it seems potentially possible that integration could help the poor without hurting the rich, which obviously is quite useful to know from a policy perspective. So, what would the implication of that be? It's that subsidized housing vouchers, in particular targeted subsidized housing vouchers that encourage families with young kids to move -- that's very important, the encouraging families with young kids to move, because the way the policy works in the U.S. is that we just give subsidized housing vouchers and don't have any particular restrictions. What actually ends up happening very often is that families end up getting put on a wait list which can be quite long to actually get one of these housing vouchers, and so ultimately your kids basically become older by the time you end up moving and that is exactly the wrong way to be implementing such a policy. So, trying to move kids at relatively young ages and, in particular, trying to steer families to these lower poverty mixed income neighborhoods potentially can be quite valuable. More broadly, you might think that changes in urban planning, or the architecture of cities, or if we design cities that are more integrated, might have significant positive effects on upward mobility based on the types of evidence we're seeing in these data. Now, that's all fine, and I think is worth thinking about in terms of policy solutions, especially given the amount we already spend on such policies, we might as well do that in a more effective way. But I think there are limits to the scalability of such policies. At the end of the day your only policy solution can't just be that you're just going to move people around. And the reason for that, you can see that with our Harlem vs. Bronx example, suppose you took everyone in Harlem and moved them to the Bronx, probably you would not have a significant positive effect because it's not like it's the soil in an area or something that's generating better upward mobility, it's probably the people who live there. Alternately, if you're thinking about policy solutions, you need to think about things that might improve existing neighborhoods, rather than just trying to move people around. Although moving, I think, can be a useful short term solution to keep in mind.

That's what I want to turn to next: what types of interventions might we be able to implement within a given neighborhood to improve kids' prospects of moving up in the income distribution. I'm going to focus in particular on education policy, and I'm going to zoom in even further than that, and focus specifically on teachers' impacts. I'm going to describe a study with John Friedman and Jonah Rockoff,

which is another illustration of using big data to study public policy issues. In this case we're using data from school district records from the largest urban school district in the United States, where we get data on 2.5 million children who've written 18 million tests over the 20 year period that we study. We take that data and link it to federal income tax returns so that we can see these kids' earnings, where they went to college, whether they had teenage birth or not -- so various outcomes when they become adults. So, essentially the type of question we could ask is, "how does the quality of your 3rd grade teacher affect how much you're earning 25 years later," using these data.

In order to answer that question, the first thing we've got to talk about is how we are going to measure the quality of teachers. If I want to show you that high quality teachers can have significant impacts on later earnings and on upward mobility, we've got to first establish some way of measuring the quality of teachers. So I'm going to discuss here one very prominent way to measure the quality of teachers which is highly relevant to the current education policy debate in the U.S., which are called teacher value-added measures. The idea of teacher value-added measures -- while there are some complicated statistics in the background in how you construct these things, the concept is really very simple -- essentially, we're asking how much does a teacher raise his or her students' test scores on average? So for example, if I'm a 4th grade teacher, the way we calculate my value added is take my students' end of 4th grade test scores and subtract their end of 3rd grade test scores. If on average, my students' test scores are going up, I'm a high value-added teacher and if, on average, my students' test scores are going down relative to the mean, we'd say I was a low value-added teacher. Now I want to think about the effects -- the causal effects -- of having a high value-added teacher vs. a low value-added teacher when you're, say, in 3rd or 4th grade. So what is the ideal experiment that I would use to answer that question? Ideally, what I would do, is I would take a set of teachers who've been teaching in a school for a long time. I would use their historical data to measure each teacher's value added, and then I would bring in a new set of students and I would tell the next cohort of students, I would tell their parents, "look, we want to answer this really important social policy question of whether teacher quality matters for your kids' long term success. So we're going to run an experiment where we randomly assign some of your children to teachers whom we've identified as low value added and some as high value added and let's see how your kids do in 20-25 years." Now, as you can imagine, most parents don't want to be involved in that type of experiment, and so what I'll show you here is how you can essentially approximate that experiment in observational data, taking advantage of quasi-experimental methods that essentially rely on the size of these data sets.

So, the way this quasi-experiment works is, we're going to think about tracking a given school over time, and we're going to follow this school -- let's focus specifically on 4th grade, for example -- and, we're going to look at different school years. So there's a set of kids that get to school around 1993, 1994, 1995, and so on. And, what happens at the end of the '95 school year, before the start of the '96 school year, is that there's a teacher who comes in, who, based on historical data, is in the top 5% of the distribution of value added. So there's a superstar teacher, who enters and starts teaching in 4th grade in this school. Looking at quasi-experiments like this, averaging over thousands of such events in our dataset, let's look at what the data actually look like. So, before that teacher comes in, test scores are kind of bouncing around the median before the teacher enters. And then, as soon as this high value-added teacher enters, test scores immediately jump up, and continue to stay high as that teacher continues to teach subsequent cohorts of students. Now, exactly like in a science experiment, what I've shown you here can be thought of as the treatment group, right? The set of people who were actually affected by the change in teacher quality, but you want to make sure nothing else changed at the same time. For example, you might be worried that when that better teacher came in, maybe the school got more resources and the principal started to do other things that might be helping kids' performance,

which would bias this analysis. So, in this context, there's a very natural control group that one can look at, which is test scores in the prior grade -- in 3rd grade -- because nothing changed in 3rd grade in terms of teacher quality in this experiment. The better teacher came in in 4th grade; she should not have had any effect on 3rd grade test scores. And, in fact, when you plot 3rd grade test scores, they're completely flat around the point when that teacher enters. So, that type of evidence and many other versions of this, lead us to conclude that it looks like high value-added teachers do in fact have a causal effect on students' test score achievement.

Now, by the way, this works in the opposite direction as well. If you have this teacher -- who's in the bottom 5% of the distribution and doesn't want to be there -- he comes in and immediately pulls down test scores relative to the prior grade right when he arrives, unfortunately. So, it works linearly across the distribution. These results just show you that teachers have causal effects on students' test score achievement, but now I want to tie this back to the issues of upward mobility and students' long term success. So, the more important results are the fact that teachers actually end up having a significant causal effect on kids' probability of attending college, of having a teenage birth, and on their long term earnings. I want to just summarize those results here by thinking about the following hypothetical policy exercise to give you a sense of the magnitudes. So let's take the bell curve of teacher quality and let's identify the teachers who are estimated to be in the bottom 5 percent of the distribution of value added shown in the highlighted yellow here. Let's say we implement a policy that either replaces those teachers, or trains those teachers in some way, to bring them up to the average level of quality in this school district. What impact will that have on students' long term outcomes? So, by implementing an analysis exactly like what I showed, using other outcomes like earnings and so on, we estimate that every such replacement of a teacher in the bottom 5% of the distribution with a teacher of average quality, would increase the undiscounted lifetime earnings of the average child by \$50,000. That translates to \$1.4 million dollars per classroom of 28 students, which is the average size of U.S. classrooms. If you discount that back to present value at a 5% interest rate, that's worth about \$250,000. Just to give you a sense of the orders of the magnitudes here, this is \$250,000 in present value for a classroom of about 25 students, so it's about \$10,000 per kid in present value, from having an average teacher instead of a teacher in the bottom 5% of the distribution. Remember that the MTO experiment generated about a \$100,000 gain from moving to a better neighborhood, but that was the effect of living in a better neighborhood for about 10 or 12 years, starting when you were around 8 years old until about age 18. So you can think about that, it's about 10 times as large an effect, right? But in a sense it's also ten times as large a treatment, because this is the effect of having a better teacher -- a much better teacher -- for one year whereas that's the effect of living in a better neighborhood for ten years. Both of these things seem to matter quite a bit, and it's plausible that the teachers' impacts would also accumulate like that, so if you improved schools not just in 2nd and 3rd grade, but also in 4th grade, 5th grade, 6th grade and so on -- one would have to study the dynamics, the complementarities across grades to really understand that fully -- but it's plausible that you could end up with impacts of a similar magnitude.

The bottom line from both of these policies is that I think you can actually have quite substantial effects both by moving people around, and by trying to, in specific, well defined ways, change the quality of neighborhoods, change the quality of schools -- it could have substantial effects on the rates of upward mobility.

The last set of results I want to show you thinks about these issues of equality of opportunity from a different perspective. So, traditionally, the argument for greater social mobility in the U.S. and other countries is based on principles of justice, so kind of the ideal of equality of opportunity, it shouldn't

matter, the birth lottery shouldn't matter. But, I want to show that improving opportunities for upward mobility can also potentially be of interest just from the perspective of economic growth -- that is in terms of increasing the size of the economic pie. In particular, what I'd like to show is that one child's success need not necessarily come at another's expense. It's not necessarily true that we should think of this as a zero-sum game, kind of musical chairs, where if we make it easier for low-income kids to succeed, they're just going to take the spots that the high-income kids would have had and the total size of the pie stays fixed. And to illustrate why I think that's not necessarily true, I'm going to focus on one specific pathway to upward mobility: innovation, which I think based on much economic theory and other research, plausibly has a causal effect on the total size of the economic pie, that is, on total economic growth. So, what we're going to do in the next set of results I'm going to show you here, drawn from a different study, we analyze the lives of 750,000 patent holders in the United States by linking the patent data to the tax data, and essentially ask where the inventors come from and what can we learn about social mobility by looking at inventors in this context. So let me start with this chart here which shows you patent rates, the fraction of kids who have a patent by time they're in their mid 30s, versus their parents' income. So the x-axis is your parents' income percentile, there are 100 dots and each dot corresponds to a 1 percentile bin, and the y-axis is the number of inventors per 10,000. So you can see that if you're born to parents who are below the median, your probability of having a patent by the time you're in your mid 30s, is about 2.2 per 10,000. But if you happen to be born to parents in the top 1% of the income distribution, you are 10 times as likely to have a patent -- you're 10 times as likely to have an invention, if we're going to use patents as a proxy for innovation.

So, why is there such a vast gap in rates of innovation related to parent income? There are a number of different explanations you might think about. One explanation is that maybe this has something to do with differences in childhood environment of the type that I've been talking about here in this lecture. Another view, which I think at least is plausible and should be considered carefully, is that maybe this is about genetics and the persistence of ability across generations. So you might think that the parents who are in the top 1% of the income distribution are also higher ability people on average and maybe ability persists -- we think ability does persist to some extent across generations -- and so those kids are just smarter and that's why they have higher levels of innovation. One way we can try to get at that is by coming back to the test score data, the school district data that I was talking about before, and use test scores as a rough measure of ability at early ages. So, this chart here, to start this analysis, shows you again the fraction of inventors on the y-axis, but now the x-axis is 3rd grade math test scores. The way this figure is constructed; each dot corresponds to 5 percent of the test score distribution, so there are 20 dots, and we're showing the rate of innovation within each of those bins. The first thing you can see from this graph is that if you're below roughly the 85th percentile of your 3rd grade class, basically you're not going to invent anything, it looks quite unlikely. And, if you're above the 85th percentile, the probability of innovation rises very sharply. That again shows you that these early child 3rd grade test scores for example, they have a decent amount of predictive content in them. Now, more relevant for what I'm talking about here, is let's replicate this analysis, splitting the data by your parents' income. So here, the blue series is for kids with parent income below the median, and the red series is for parent income above the median. There are 10 dots in each of the series, so each dot corresponds to 10% of the test score distribution. So what do we see here? Up to the 90th percentile -- everything except the last dot -- the two series look relatively similar to each other. If you take the highest ability kids, the kids who are scoring in the top 10% of their 3rd grade class, you see clearly that high ability children are much more likely to become inventors, if they happen to be born to parents who are high income rather than low income. If you're born to a low-income family, your probability of becoming an inventor actually is not all that much higher if you're in the top 10% than if you're below that level, but the odds really shoot up if you happen to be from a high-income family. So, even conditioning on a measure of

ability, it looks like there's this big gap in rates of innovation by parent income, which looks more consistent with the view that this might be something about differences in the types of resources or environments in which these kids are growing up, as opposed to just raw differences in ability between rich and poorer kids. Now, furthermore, it turns out that these gaps in test scores grow very rapidly as children grow older. So, in 3rd grade, the low-income kids are not that far behind relative to the high-income kids. But what we see is that if you look at 4th grade, 5th grade, 6th grade and so on, test scores start to explain more and more of the gap in innovation. That is, the low-income kids are basically falling behind in terms of achievement relative to high-income kids and, by the time you get to the end of high school or to college, you're not all that surprised to see that kids from low-income backgrounds are much less likely to become inventors than kids from high-income families. Which, again, looks consistent with the view that the innovation gap may be driven by differences in childhood environments. I don't think it necessarily nails it in this context, but it's consistent with some of the earlier evidence that I was showing you. So, what's the implication of this analysis? It suggests, I think, that improving equality of opportunity is not just of interest to people who are starting out at the bottom of the income distribution, but potentially to all families. That is, having kids from low-income families do well might end up getting more people who invent something which obviously could potentially have a benefit to everyone, and not just those kids themselves.

Let me conclude by talking about some policy lessons, and I'm happy to discuss this further during the question and answer session. So, one very simple policy lesson that comes out of this is that I think it makes sense to think about issues of social mobility at a local rather than national level. So, there's a lot of discussion typically about, is America the land of opportunity? How can we restore the American Dream, and so on. But, really, I think it makes much more sense, when you look at these data, to think about how can you restore the American Dream in Milwaukee, because it's actually just fine if you look at some other places, even neighboring Waukesha County or places like San Jose and so on. So, thinking about the problem at a much more local level I think can be very constructive. Second, what do we want to do concretely? I've emphasized that I think the key here is focusing on the childhood environment, and I want to stress that it's improving the childhood environment at all ages, not just the earliest ages which are receiving the most attention in the current policy debate. How might you go about improving the childhood environment? One short-term solution is to build on our existing subsidized housing voucher programs, try to help families move to better areas. A longer-term solution, which is where I think we really need to turn in the end, is to improve neighborhoods. I've talked about one potential angle on that which is improving the quality of schools, but I think there are presumably a number of other dimensions in which one can think about trying to improve specific neighborhoods. Third, and most generally, I think what this work illustrates -- and a number of others' work illustrates in recent research in economics -- is that harnessing big data to evaluate policy scientifically, and measuring local progress and performance can be quite valuable. In particular in this context, we can use these databases to identify which neighborhoods are in the greatest need of improvement, and which policies actually seem to work. So to facilitate some of that analysis, in our group what we've done is taken all of the statistics I've been discussing here -- like county level statistics on social mobility -- and we've posted them on this website, equality-of-opportunity.org. The idea being that other researchers can then download these data and, maybe you're aware of some policy that was enacted in the Madison area where something changed in the schools or the tax system or something like that, and without going back to the raw tax return data you can look at these measures of social mobility by birth cohort and ask "does it look like this policy change ended up having an effect?" And I think that's the way in which hopefully as a broader research community, we can build the type of knowledge base that would be very useful in understanding the determinants of social mobility and what the relevant underlying models are.

Let me conclude by coming back to the statistics that I started out with. The odds of rising from the bottom fifth to the top fifth. As I said in the U.S., on average, that number is 7.5 percent, less than most other developed countries. One way you can look at that is from a pessimistic point of view, you can kind of have the interpretation that the U.S. does not really look like the land of opportunity, doesn't look like it gives kids from low-income families great chances of succeeding. But I actually take a more positive interpretation of these data; I think what they present are an opportunity and a challenge. So the reason there's an opportunity I think here is the fact that there's so much local variation across places that are relatively similar, and we see that when people move to some of these areas, they do much better. That suggests that you can actually change social mobility through policy in quite meaningful ways. This is not just something you're stuck with, it's actually something you can do something about, so there's a real opportunity there. The challenge for us as researchers is to figure out what exactly is driving these differences in mobility across areas, what the causal determinants are, and so on. The challenge for those interested in implementing these policy solutions is to take some of those ideas from research and ultimately implement them in policy. Thanks very much.

[applause]

[Lonnie:] We have plenty of time for questions. Raj will call on people and I'll run around with a microphone.

[Chetty:] Yes, Steve.

[Durlauf:] So, a narrower question and a broader question or comment. It seems that what's key in your identification strategy for locational effects is the finding with respect to siblings of different ages. But there's another way to think about that which is that if parents improve with age, that the 3-year old has more years with a good parent than the older one. And put differently, the very fact that the parent delays moving until a certain age would suggest some heterogeneity. The bottom there, I think is that in thinking that the argument is incontrovertible for fixed effects for parents, but if we think about exposure to parents versus exposure to the neighborhoods, it's not quite as clear so I think that's probably something I think you'll want to - I would like to have your reactions to that. Second, the broader one has to do with the difference between absolute and relative mobility, and the talk moved between those. It seems to me that there's actually a principal difference that matters in the following sense, which is that my vision of a good society is one where if the parents are doing well, they can lock their kids in, but if they aren't doing well, there's still substantial possibility for mobility. But that's actually antithetical to the locations and percentiles, so I'll put that on the tables as well.

[Chetty] Alright, those are excellent questions and we have indeed thought about both. I didn't present a lot of the details of that quasi-experimental paper here but the bulk of it is focused on exactly the issue you raise. I showed this evidence on siblings just to recap Steve's point, the sibling evidence showed that when a family moves from a worse area to a better area, the younger kid does better in proportion the difference in ages. So, a potential confounding factor there is suppose something else changes like, you get a better job or you get married, something changes in the family, that directly affects the kids in proportion to the difference in their ages. So, for instance, you move to a better neighborhood at the same time something improves in your family, the younger child is going to be exposed to that better family environment for a longer time than the older child and you would potentially end up seeing a difference in outcomes between the two kids even if neighborhoods did not have a causal effect. So that's essentially the type of thing we're concerned about, an omitted variables bias. We deal with that in the following ways: First one simple thing you can do as you might expect is control for changes in parent income, changes in parent marital status, some of the most obvious things

that you think might be confounding this analysis. That doesn't change anything. But I think the more important set of tests is the following -- what I briefly mention is a set of placebo tests. The way these work is as follows: So it turns out that I was focusing on the mean effects of different places, their effects on average income. But there's lots of heterogeneity in the causal effects of places. So it turns out that some places are particularly good for girls and other places are particularly good for boys. Some places seem to be particularly good in helping you reach the upper tail. For example, Boston and San Francisco, on average you reach the 46th percentile in those two cities if you started out with parents at the 25th percentile. But San Francisco generates a lot more variance than Boston. San Francisco is more likely to put you in the upper tail and also more likely to put you in the lower tail, relative to Boston so there's a lot of heterogeneity across places relative to the distribution of kids' outcomes. Finally there's also a lot of distribution across places -- there's some variation across birth cohorts. Some places are getting better over time, some places are getting worse over time. Why am I mentioning all this heterogeneity? All of this heterogeneity gives us really useful additional predictions, essentially overidentification tests that we can use to evaluate whether neighborhoods really have a causal effect. What we find is if a family moves with a daughter and a son to a place that's particularly good for girls, then their daughter does better in proportion to the number of years that she lives there, but not the son. Now if you think about whether there could have been some other factor like a change in the family's wealth that changed in the family that happened to affect the daughter but not the son, that feels less plausible than if there's a causal effect that's related to the fact that girls are doing particularly well in this environment rather than boys. Similarly we find the same thing that if you across the distribution of outcomes. So, if you move to an area like San Francisco, not only do you do better on average if you were moving from a place like Atlanta, but the entire distribution of outcomes converges to the distribution in San Francisco. You're more likely to be above the 90th percentile, you're more likely to be below the 10th percentile, so there's an entire distributional convergence. Finally, you find the same type of convergence across birth cohorts. If you look at the causal effects of place by birth cohort, we show that it's exactly your own birth cohort's place effect that predicts how you end up doing that has this exposure effect. Not the birth cohort before or after, that even though these things are highly correlated, you have enough data so that you can pick out the causal effect of each of these cohorts. So, on all these dimensions, there's a complete convergence of your outcomes to the outcomes of the destination to which you move. And if you think about what that other omitted factor within the family would have to be, it would have to somehow be a thing that exactly reproduces the characteristics of the neighborhood to which you're moving on all these dimensions. While theoretically that's possible, that seems less likely than the simpler explanation than that there are these causal neighborhood effects. That's the way we try to handle that.

On the second question of relative versus absolute mobility: So, naturally these are very different concepts. Relative mobility the way I think about it is essentially how different are the outcomes of kids growing up in low versus high income families. You can think of it as if you're plotting the kids income versus the parents' income, what is the slope of that relationship. Absolute mobility you can think of as how well are kids from low income families doing on average? How much are they earning on average? These two things are different, right? So, increases in absolute mobility, holding fixed the outcomes of the rich, generally we would think of as desirable because that would essentially be a Pareto improvement, if you can get better outcomes for kids from low income families with hurting kids from high income families, most people would think that's a good thing. Increases in relative mobility, it's much less clear, right? One way in which you can increase relative mobility is by just randomly allocating, for example, admission to college. That would generate a lot of relative mobility because your parents' would end up not mattering because we're just flipping a coin and letting people into better jobs, better colleges, things like that and obviously the people would think that's probably not a very

good solution even though without increases relative mobility. In what I've been showing you you're right that I've moved a little bit back and forth between relative and absolute mobility. One thing I want to stress is in all of the local geographic variation we were measuring kids ranks in the national income distribution to you essentially want to think of those as measures of absolute rather than relative mobility. That is, you can think of those thresholds as been fixed from the perspective of any one give an area because in any one small area if I were to rank people within that area and if somebody goes up in the distribution in Madison, somebody else has got to come down, right? Whereas if I'm final ranking people in the national income distribution it's essentially like them asking what are your odds of earning more than \$50,000 if you live in Madison so there's no baked in relative mobility effect there. So I think, my sense is that absolute mobility is of particular interest to some of these policies I was talking about like the MTO experiment the school effects while we have conclusive evidence on the spillover effect it looks like it could plausibly have some effect on absolute mobility, so I'll leave it there.

[Questioner:] Do you have any evidence that teachers in Dubuque are better than teachers in Milwaukee? And I'm very worried about the implications, as an educator, I'm very worried about the implications of telling teachers just raise those test scores, whatever you need to do, do it.

[Chetty:] As I discussed earlier, there are correlations between the quality of public schools it's difficult to systematically measure the quality public schools across areas but as best you can do it with things like expenditures, class size, test scores controlling for various things you tend to see the places that have better schools of do produce better outcomes. Now is that because there happen to be better teachers in Dubuque then another place my guess is that's probably not what's driving the very large differences. So there are probably lots of other factors that matter quite a bit. The reason I talk about the teachers is that something we can change, whereas some of these other factors like families are more stable in Dubuque then another place it harder to know what to do about that right and you can change a child's teachers. Changing a child's parents is obviously a harder thing to do. On the point of how might you actually go about improving teacher quality do you want to really focus on test scores? I don't think that's clear and what the analysis shows is that teachers impacts on test scores do seem to be correlated with their long-term impacts. Now when you start using that for policy however that can generate potentially negative behavioral responses where if you tell teachers, look, what's really critical for whether you get to keep your job or not is if you get students above a certain test score threshold of course you can induce undesirable responses like teaching to the test or even cheating in some extreme cases and so my view is that at the end of the day, that's an empirical question. How big are those effects, can you mitigate them and so forth. but I think the lesson here, bigger picture than that, people get very focused, that's a very controversial debate how you might use this test scores. the bigger picture point if it is just that teachers matter a lot and so are you going to evaluate them with test scores are maybe are you going to evaluate them using principal ratings or some other method. I don't know what the best method is and that something hopefully we'll study a lot more and in the coming years but whatever method we use I think trying to keep talented teachers in the classroom seems like it could be really useful, that's the lesson I want to draw from that.

[Questioner:] I was struck by that one graph you showed earlier on when you were looking at the impact on kids from families that moved from bad areas to good areas and the fraction of the gains that they were obtaining declined with their age, the age with which it moved, or declined. Now, suppose you reversed it and looked at kids, so families that moved from good areas to bad areas. Is that same linear relationship, only in reverse? Does it go the other way?

[Chetty:] it's totally symmetrical exactly. So this data is based on all moves both up and down I was to make it simpler just explaining it is moving from a worse to a better place but you can do this analysis

for people move to worse places and people move to better places you find totally symmetric effects every extra year you spend in the worse environment, you see this linear decay there as well they argue, you see worse and worse outcomes. You can actually look at people who moved multiple times so let's say you lived in Milwaukee County for two years between 9 and 11 and then you move somewhere else. Turns out that your ultimate outcomes are proportional exactly to the amount of time you spent in each area, it's kind of like a weighted average of all the areas in which you grew up. So there's this really robust linear property in these childhood exposure effects. I think there's up here a couple of questions.

[Questioner:] So I wanted to follow up on Steven's question about the absolute mobility so if you look at the kids who start out in the top decile, are they also in these good areas for the upward mobility, are they also good areas for preventing downward mobility or staying at the top.

[Chetty:] Great question. So two things to note there on downward mobility. So the first fact we generically find is that it matters less where you live if you're rich than if you're poor. So if I were to redraw that map I showed you at the beginning for kids who started at the top of the income distribution the first thing you'd notice is that there's less variation across places. So to take one example take Atlanta vs. San Jose vs. Salt Lake City. So take the kids in the top 1% of the income distribution, kids in Atlanta are doing just as well on average as kids in the other areas so that's fact number one. The second factor is that there's still some variation on to the variations about 40% of 50% as big as for the low income kids. That variation is not strongly and it's definitely not negatively correlated with the variation in upward mobility so if anything it's uncorrelated or slightly positively correlated so as I was saying for example, the more mixed income cities like places like Sacramento they generate better outcomes for low income kids and they generate if anything slightly better outcomes for the high income kids so it doesn't look like here's this place that's really good at bringing low income kids up and we end up somehow leaving the higher income kids behind. You don't see that evidence of that hard tradeoff in the data.

[Questioner:] A woman's perspective -- I'm an implementer rather than a researcher and specifically in public health and we're working on African American infant mortality in Wisconsin is 2 to 3 times that of the rest of the country and our maps look identical to your maps. We're faced with a task right now along with half the states in the country to come up with areas of social determinants of health to address infant mortality and we would be adding incarceration or criminal justice reform into that. The problem is there is so little research available to help us figure out, the evidence for which of these when we choose with very little money. So I'm wondering if you want to comment on that or say something about that topic.

[Chetty:] Sure. So what are the implications of this type of analysis for health. So while I haven't shown you anything here, we're actually working on a study at the moment using data on mortality linked to all these measures I've been showing you, doing a similar spatial analysis where we find I think consistent with what you just said that there's dramatic variation across space in terms of mortality income gradients so we know in the U.S. that poorer people live shorter lives than higher income people. But that turns out also to vary dramatically depending upon where you're living and we're trying to do a similar analysis to understand why that is the map looks correlated with what I'm showing you but it's not actually identical so my guess is some similar factors might matter but there probably are other things up as well like health behaviors and so on which my colleague David Cutler who I'm collaborating with on this has spent quite a bit of time thinking about. So I don't have anything concrete got to tell you at the moment, but these types of datasets that I am showing you here are exactly the sort of datasets that can shed light on the on the issues that you are interested in and I agree they're very important.

[Questioner:] So, thinking about the big map, the first map. we would expect that opportunity structure should be capitalized into housing prices so if you adjust for housing prices or cost of living differentials does this map change much?

[Chetty:] Let me make two points there as well so first of all if you adjust for cost of living this map actually looks relatively similar so if you deflate everyone's income by a local cost of living index of based on the price of commodities and rent and things like that, you get a very similar map so why is that conceptually? So intuitively you would think that if you're earning \$30,000 in New York it's worth a lot less than earning \$30,000 say, here in Madison, right? But what this map is showing you is not just the level of outcomes of the kids, the way to think about it is how well are the kids doing relative to the parents? Most people tend to stay roughly around the area where they grew up so when you make a cost of living adjustment you are pulling down kids incomes when they live in very high cost areas but you're also pulling down their parents incomes and so upward mobility is really a study of how much did you move relative to your parents and that's why it ends up not being that affected by adjustments for cost of living. Now a second point to make there -- you started your question is wouldn't you expect this to be capitalized in house prices especially at a much more local level, doesn't you would think intuitively, an economist would say it would cost more to live in an area where kids are going to do better so it turns out empirically that the correlation between rents in an area and our upward mobility measures is about 0.2 So there is a positive correlation but better that's also telling you that there are lots of places at roughly comparable rent for your kids might actually do much better and that I think is a very interesting question to think about. Basically why aren't people themselves choosing to move to better places where their kids would do better if it doesn't cost them back much more. And the MTO experiment I think brings some of these issues to light. You know why did we have to give people these housing vouchers if their kids are going to do so much better down the road, why weren't they electing to make those moves themselves? One striking thing about the MTO experiment which I didn't spend time on is that they're actually three groups in the MTO experiment. There is the control group, the section 8 group which could use it's vouchers in any area, and the experimental group which I focused on which had to use the voucher in a low poverty area and what we end up finding is that the section 8 group which had an unrestricted voucher which could actually be used to do more things right, it's a strictly better option. That ended up having smaller effects, considerably smaller effects than the experimental voucher. So actually limiting people's choices to move to these lower poverty areas could improve their outcomes. So again that has the feel of, you know, why didn't people choose to make those moves where their kids could have done better and that raises the issue I gave in another lecture I gave, the Ely lecture at the American economic association on behavioral economics and I think there could potentially be behavioral factors at play where you are getting the gains for your kids with some uncertainty 20 years down the road but you've got to pay the fixed costs up front of moving to a very different place, and there's a lot of inertia, there may be a lack of information, lots of things a play that could be leading to these choices but I think that this is really important because it suggests that we might be able to move families to better neighborhoods without having to spend tremendously more in order to achieve those gains.

[Questioner:] I'm a little puzzled. What makes a better area and, particularly, I was looking at some of this stuff, are rural areas better than urban areas generally? Maybe it isn't suburbs and cities, maybe it's rural areas best of all looking at some of these outlying rural areas are doing very well in Wisconsin, Dubuque doing well makes no sense to me at all unless it's something about the wide open area. What makes a better area?

[Chetty:] Makes sense to people in Dubuque. So, I tried to get at that by showing you these five factors, these five characteristics of areas that seem to produce better outcomes -- more integrated places, schools, things like that but let me comment on your point about rural vs. urban. You're absolutely right, and you see that in this map that more rural areas on average seem to generate better outcomes and particularly I was actually even finer detail than that, the places that generate particularly bad outcomes are places with very high levels of concentrated poverty. So the large public housing projects I think are the most extreme example that you saw that in the MTO experiment. So these places where everyone around you is very poor very low employment rates, gang violence things like that, they generate much worse outcomes than more suburban or, going more to the extreme, more rural areas. Now one thing to note though is in a place like Dubuque the kids who are very successful, this goes back to a point I made early on, this is where they're growing up, not necessarily where they're living as adults so a lot of those kids growing up in rural Iowa and doing well, a number of them are living in Chicago or somewhere else when we're measuring their income in adulthood so it's not per se I think the economy of Dubuque that is somehow doing well and a powerhouse in terms of generating jobs and higher levels of income, it's I think something about the childhood environment and one way to think about that may be why integration matters and why it might be better in rural areas is the simple idea that if you're in a rural area there's only one school for example typically that that you might be able to go to. It's much easier to self segregate if you're in New York City than if you're in a much more rural area. So my wife for example grew up in a small town in Southern Illinois and the doctors kids and all the other kids in the town went to the same school and that would have looked very different say in a larger city so is that the definitive explanation, I don't know but it would be consistent with some of the evidence that I'm showing you.

[Berger:] I'll ask you a policy question, so I realize that this moves beyond your data, but if you think about wanting to -- so kids do better in more integrated areas and so you want to integrate neighborhoods, and so you can think about something, policies like MTO where you're giving conditioned vouchers, you have to move to a certain area or you could think about subsidizing developers to develop low income areas or subsidizing developers to develop low income units, so have you thought about some of the policies that you think might be most effective or do you have preferences in those areas?

[Chetty:] Yeah, I guess the tricky thing in all of these policies is to think about the equilibrium impacts which is where I feel like I'm not totally confident yet because I don't know what the resorting process will look like, I think the big struggle in issues of trying to integrate communities is people very naturally self segregate again. I think there are couple of narrow policy lessons which are very clear to me so if you have the choice between building public housing or spending that money on giving people vouchers to live in various areas- I think it's pretty clear from the data that you want to do the latter. And in terms of costs actually it's not that the doctors are more expensive than the public housing, and the U.S. is moving in that direction but we still have something like 1,000,000 or 1.5 million families living in large public housing, I think it's clear that you want to go in more decentralized direction if you're going to choose between those two things. On the subject of another aspect where I think policies can be improved as in terms of targeting, so as I mentioned trying to target families with younger kids don't try to move the adults don't try to move the older kids but also tried to give these credits for building in certain areas. So for example, in the U.S. we have a very big tax credit called the low income housing tax credit we spend about six billion dollars a year on it. That credit requires developers to build housing that will be affordable to low income people but it doesn't say where you have to build that and what's been showing empirically is that a lot of the units, it's been very effective in getting developers to build more such units but they tend to be in relatively higher poverty areas so you can imagine changing the

credit, so again you have this conditionality. You have to build an area that is more mixed income and I think one lesson that emerges from this is it's not that you have to force people to build in the most affluent communities where always you're going to have people trying to move away and that's just not feasible. What you can see for example from the MTO experiment it's not like you were moving people into Westchester County or midtown Manhattan are the best, the highest income places in New York. You're making relatively narrow moves and these ended up still having quite a significant effect, so it's conceivable that developers might build in the Bronx instead of Harlem and that can actually have an effect.

[Questioner:] I just have a question, if you have applied this analysis to other countries where culture might be different vs. the capitalistic American cultures to a different society that has different values and if the same of rules are that you're finding here that you have here axioms of how things are in causative factors if that applies in those other countries?

[Chetty:] Yeah, it would be fascinating to understand what this looks like in other countries and do similar spatial analyses see if you find similar patterns and so on. It's basically a data at the moment is a limitation were just starting to get these data. Even in the U.S. four years ago it would have been impossible to show you most of these results. Now I think other countries are starting to, Scandinavian countries are ahead of the U.S. in terms of data availability and the issue there is there's just less geographic variation to study in Denmark than in the U.S. because Denmark's just not that big so as people develop these databases and see the value of having such data my guess is you'll see similar analysis in the UK or maybe even eventually in developing countries like Brazil I think would be very interesting, one could learn a lot from that that type of analysis so at the moment I mean my hunch is that some of these patterns things like segregation in schools you would think that's got to be like a relatively universal thing, the magnitudes might not be the same but it feels like that mechanism, but empirically I don't have anything more to say about that at the moment.

[Berger:] So we could do one more question if anyone has one. Ok so please joins us for our reception right behind the lecture hall.

[Applause, piano music]

