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DO INDIVIDUAL DEVELOPMENT ACCOUNTS
PROMOTE HOMEOWNERSHIP?

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P R O C E E D I N G S

MR. GAYER: All right. Hello and welcome to the Brookings Institution. My name is Ted Gayer. I am the co-director of the Economic Studies Program here at Brookings.

Today we have in store for us an interesting discussion of a new study that my colleague, Bill Gale, has written. He's co-author with faculty members at UNC Chapel Hill and Washington University in St. Louis. The paper examines the long-term impact of homeownership of an individual account development program.

The AV guy just -- are we in -- are we on? Excellent.

Okay. The paper examines the long-term impact on homeownership of an individual development account in Tulsa, Oklahoma. The Tulsa IDA was a savings account where withdrawals from housing down payments were awarded matching funds. As you'll soon learn, the authors find an interesting result. While there was a short-term increase in homeownership from this program, by 10 years there's no statistically significant impact on the homeownership rate or the duration of homeownership between those who participated in the program and those who did not.

The author's research question is interesting for a number of reasons. I'll just highlight a few. I'm sure our discussants will highlight more. First, given the problems with wage stagnation, limited saving opportunities for the low income, and constraints on our federal and state budgets, it's important that we look for effective and targeted policies that promote saving. This also relates to the question of whether housing is the best vehicle for people to increase their wealth.

Housing went from being largely viewed as a consumer durable earlier in the 20th century, to later being seen as a wise investment in the '70s and '80s, when it

appreciated about 1 percent a year above inflation. To being viewed as a relatively quick way to garner wealth in the 1990s and early 2000s, when it appreciated about 4 percent a year above inflation.

With housing prices down about 30 percent nationally and about $6 trillion in lost housing wealth, things look very different now. Still, perhaps we shouldn't entirely give up on the role of housing as an inducement for saving, as it does provide a way for people to pre-commit, first for the down payment and later through paying down principle as part of monthly mortgage payments.

The author's research question is also important because, given the number of people substantially underwater in their mortgages, it's important to reevaluate the different means by which policy can promote homeownership. I think a shift from subsidizing mortgage finance to subsidizing incentives to save for a down payment is good policy, but it does lead to the question of how best to provide such incentives.

Finally, for the wonkish out there, the authors' research shows that it's not enough to demonstrate that a policy has an effect or even to demonstrate the magnitude of the effect. One must also question the timing of the effect. A program that provides a time-limited incentive may just change the timing of the desired market outcomes, not the aggregate impacts. So it's important to understand how this works to kind of get into the black box of the policy, and to determine how quickly the market outcomes return to trend.

With that just brief overview I'll turn it over now to David John to moderate the session. And again, welcome, and I hope you enjoy the event.

MR. JOHN: Let's see if this still works. Yes, it does. Thanks, Ted.

The session we've got today is to look at the IDAs, as Ted just

mentioned, and see what their long-term effect is. I mean, those of us who have been involved to some extent in asset development for many years have worked on IDAs. We've looked at them, but this is one of the first studies about the long-term effects.

The way we're going to set up today's program is, first we're going to have remarks by Bill Gale. Bill is the Arjay and Frances Miller chair in federal economic policy here at the Brookings Institution. He's also a co-director of the Tax Policy Center and director of the Retirement Security Project. You have full bios in your packets there, so I'm not going to go through that.

Following his presentation, there will be a response from Greg Mills. Greg is a senior fellow at the Center of Labor, Human Services, and Population at the Urban Institute. And another response by Reid Cramer, who is a director of the Asset Building Program at the New America Foundation.

After their responses, we're going to give Bill a couple minutes to respond to their responses, and then we're going to open it up for questions. So without further ado, Bill?

MR. GALE: Okay. A little musical chairs here of speakers.

Welcome, thank you all for coming here this morning. What's looking like a beautiful day.

This paper that I want to present today is co-authored with five different -- five other people: Michal Grinstein-Weiss at UNC is the lead author on this; Michael Sherraden, of course the founder of the IDA idea, as a co-author; and then Bill Rohe, Mark Schreiner, and Clint Key, who are associated in some different ways with one of the three of us at the beginning. So, it's a group effort. And it reflects, I think, the wide variety of researchers and perspectives that are interested in how IDAs work and what all

that means for raising living standards for low-income households, and public polices, and so on.

So I want to say a little bit by way of background. This looks, to me, like a very educated group, so I'm going to skim through some of the usual getting up to speed stuff. A lot of you are up to your necks in the IDAs and related policies.

So, anyway, the big picture is, there are different approaches to raising the living standards of low-income households. And this has been a question, of course, for decades if not centuries. What's the best way to improve the economic status of low-income households? The sort of traditional approach has been consumption transfers, or income support, if you want to call it that. In the '70s, '80s, '90s, the last decade, the role of work incentives, the EITC, et cetera, has ascended on importance. And then in the last two decades, the role of saving, the role of increasing asset accumulation, wealth accumulation among low-income households has become important.

And there are a number of reasons why, a number of arguments given. Just to touch on them, savings can help with down payments, with lumpy purchases to get people over the hump. If you can't get a down payment to buy a car, then you can't get to your job, then you can't work, and so on and so forth.

They can help in emergencies -- savings can help in emergencies. There's documented situations where people are living very close to the edge and in financial terms, something bad happens -- they lose their job, they get sick, their car breaks down -- and because they can't fix that, they can't pay for that particular incident, a whole series of bad episodes happens after that. So saving, having a nest egg, can provide funds to offset the effects of an emergency. Saving interventions, subsidies, can offset other policies, other federal polices, that are means tested for a variety of reasons.

And then there's the most intriguing, I think, idea here is that the act of saving itself could inculcate good behavior in people. That is, the very act of setting money aside and saving it and watching it accumulate could change people's values and behavior in itself, which is not something the standard economic model would accept. But certainly other models would. So it's a very intriguing idea.

Individual development accounts are kind of the mainstay of this field. IDAs basically are savings accounts where, if you take out the money for a qualified reason, you get a match. And the -- typically they also come with financial education. They also come with varying degrees of case management, of workers actively encouraging people to save.

The IDA concept, as I mentioned, was originally conceived and designed by Michael Sherraden in his 1991 book. It's important to note, I think, that if you're talking about an individual retirement account, that's something that's set up by federal law. Every IRA is subject to the same set of regulations. Every IDA initiative is different. So I forget who, you know -- happy families are all the same, unhappy families are all unhappy in their own way. IDAs are like unhappy families, they're all different.

There's been more than 50,000 IDA actual accounts opened over the last decade. The federal government has ministered a whole variety of these things, but so have state governments, local governments, community centers, some banks have done that. And other countries are pursuing this as well. Not only IDAs, but child development accounts. I'm sure Reid can talk about that more.

The problem is there's not a lot of evidence. It's a very popular idea, people like the notion of accumulating wealth, but there hasn't been much evidence done on -- evidence accumulated on the impact of IDAs. There's one Canadian experiment

that shows significant effects on small business, start-up, and educational attainment. This was a randomized experiment that was done, I think, in 10 Canadian cities. There were very high match rates for education. I think 3-to-1 -- I think -- and the same for small business start-up. So, that experiment did show an impact on those outcomes over several years.

The only other experiment with IDAs -- by which I mean, randomized experiment rather than the demonstration -- the only other randomized experiment took place in Tulsa, Oklahoma, right at the turn of the century for four or five years. There was a 2-to-1 match here for home purchases, a 1-to-1 match for other qualified purposes, which I'll talk about in a second. Overall, you could accumulate $6,750 toward a down payment, counting the match, which is a lot of money relative to the median house value in Tulsa. Certainly is sufficient amount of money to put a down payment down.

So in earlier work examining the effects over the first 4 or 5 years, Greg Mills and I and several other people showed that the IDA did raise the homeownership rate of the treatment group relative to the control group by between 7 and 11 percentage points, depending on if you used all renters or renters in unsubsidized housing or different -- or the whole sample. There was no measured effect on other qualified uses -- retirement, savings, home repair, business start-up, small business. So basically anything that got a 1-to-1 match we could not find an impact on.

But housing purchases -- sorry, homeownership we were able to -- which got the 2-to-1 match, we found a statistically significant, and I would argue economically significant effect 7 to 11 percentage points over the 4 years. And that's in the paper that's listed here in the *Journal of Public Economics* in 2008.

All other IDA research, besides these two experiments I mentioned, is

non-experimental and that is a subject -- that should subject it to concern. The people who sign up for IDAs are not like people who look -- who do not sign up for IDAs. The people who sign up for IDAs are motivated savers, and in the paper that I'm presenting today we have some evidence on that. But it's not -- the IDA sample is not a random cross-section of low-income households, even controlling for observable characteristics. So, I would argue that inference that comes from comparing a group of people who signed up for an IDA and a group of people who were just a random section of the low-income population is not a valid comparison. And there are a number of studies out there, but I don't think we learned anything in particular from those studies. I think we need the randomized control group that an experiment offers.

Okay. So, this is all short-term effects. The effects were measured four years after baseline and people had three years to accumulate the effects -- accumulate savings in the Tulsa IDA. So we're really looking at the impact in that earlier study one year after they stopped contributing.

But remember, the long-term goals are what we are really after here. If you're looking at the original goal of raising the economic status of low-income households, that's inherently a long-term goal. If you just raise somebody's status this year and they fall back the next year, it's nice that you did that this year, but the real goal here is changing the long-term status.

So, there's no evidence to date in the U.S. in particular on long-term effects of IDAs until this paper. And the long-term effects could differ from the short-term effects. They could differ in either way. The logic of, for example, inculcating good behavior -- if IDAs inculcated good behavior of the financial education stuck, if the encouragement from the case management stuck with people, if getting people over the

hump into homeownership, then became a gateway toward further financial gains -- then you'd expect the long-term effects to exceed short-term effects. That is, you'd expect there'd be this impact of IDA, and it would keep expanding over time.

So it's possible that the long-term effects are larger than the short-term effects, but it's also possible that the long-term effects are smaller than the short-term effects. And the logic here, which I'll come back to, is as follows.

The experiment ran for three years. During that time, treatment group members had an incentive to accelerate home purchases into the experimental time period because they got a 2-to-1 match during that time period.

SPEAKER: Who matches, Bill?

MR. GALE: I'll tell you in a second.

So, treatment group members had an incentive to accelerate home purchases in time. Control group members not only didn't have access to the IDA, they didn't have access to a regular homeownership assistance subsidy at the Tulsa Community Center during the experimental period. So they had incentives to postpone home purchases. They had a worse set of options available to them during the experimental period than they did after the experimental period.

So if you think the incentives are important, the timing is important, then that would be an argument why the short-term effects would be larger than the long-term effects. That is, the long-term effects would be less than the short-term effects. So, we've got arguments going both ways and the only way to resolve this is through empirical analysis.

So what we do in this paper is we commission a new survey of the baseline, control group, and treatment group. And the survey takes place 10 years after

random assignment. So we have what I'm calling long-term -- what we call long-term -- 10-year impacts on homeownership and on the amount of time that people owned a home during that period. So we have the 2009 homeownership rate, which gives you one test. And we have the amount of time during the sample period, the 10-year sample period, that people owned homes, which gives you a second test.

I want to just summarize the main results before I go through any more of the analysis. The first is that the effect on the 2009 homeownership rate is very close to zero. It's not statistically significant. So, the 7 to 11 percentage point effect we saw after 4 or 5 years falls to 1 to 3 percentage points and it's no longer statistically significantly different than 0. It's not significantly different from zero; we'll say it that way.

The second is that the gains that the treatment group made through 2003 -- that is, through the first part of the experiment, the first analysis -- go away almost immediately in 2004. And basically what happens is these incentives that I'm talking about kick in and it looks like nobody in the treatment group buys a home in the year after the experiment ends and the match goes away. And the control group just catches up, basically, at that point.

In fact, the control group catches up so fast that there's no impact on the average amount of time that controls or treatment groups owned homes during the 10-year period, even though after 4 years the treatment group was ahead. Okay?

A couple of other results to mention. There was an impact on homeownership rates among households with above median sample income. So, people who were in the top half of the income distribution in the sample -- we're talking about people like 15,000 to 35,000 -- there was an impact of IDAs on homeownership. And we'll talk about that a lot. We tested 24 subgroups. In one of them there was a

significant effect. The issue is, is that a real effect or is that a false positive? You would expect to get one significant effect, even if all of the effects were random, all right? And I'll just tell you at the beginning, we cannot resolve that issue, we can only point it out -- point out that the results are totally consistent with no effects, even though we got one significant effect.

As an aside, the homeownership rates for both treatments and controls grew over the time period -- substantially over the time period. This is evidence on sample selection; this is evidence that the IDA group is not a randomly selected subgroup of the population. The people that signed up for this experiment were very motivated to buy homes and their homeownership rates for both groups grew dramatically.

And then there's an interesting statistical situation -- interesting in a geek-ish sense -- that the sample-wide difference and difference estimate, which one normally uses as an estimate of the impact of an IDA here, is not a reliable indicator for reasons I can talk about. But if we don't have time, it's in the paper.

So, what I want to do briefly is just go through the experiment, mention some preliminary data analysis, explain how we get our results, explain what our results are, and summarize.

How much time do I have, David?

MR. JOHN: About 20 minutes remaining.

MR. GALE: Twenty minutes remaining? Oh, okay. I'm going to tighten that up a little bit. But we'll see. I'm not going to talk for 20 more minutes.

Okay, so the American Dream Demonstration was sponsored in 14 sites around the country in the late 1990s. It was sponsored by a whole consortium of funders. Ford, I think it's fair to say, was the lead. But there were many -- pardon?

MR. JOHN: They were the largest.

MR. GALE: The largest, yeah. But there were many other foundations involved, and I don't mean to slight anybody by not mentioning them.

There was only one experimental site that was in Tulsa. The others were simply demonstrations of IDAs. And the eligibility rules were pretty simple. You had to be employed at the time you signed up, and the income the previous year had to be less than 150 percent of the poverty line, where that's a function of your family status.

Pretty simple design. The treatment group had access to IDAs. They had some required financial education they needed to do, and there was fairly active case management. Workers at CAPTC, which is the Community Action Program --

SPEAKER: Project. Community Action Project.

MR. GALE: Project of Tulsa County, CAPTC, which administered it. They were fairly actively -- basically bugging people, you know. You haven't saved this week, we haven't gotten your contribution this week, don't forget to save, et cetera.

The control group didn't have any of that. And neither group had access to an existing CAPTC home assistance subsidy during the experiment. Of course, when the experiment ended, both groups got access to that back.

The actual account was a regular bank savings account. The treatment group could contribute up to $750 a year for 3 years. After that, they had six months to take the money out. The money, Bruce, was provided by the consortium of foundations who sponsored this. And as I mentioned, the 2-to-1 match for purchasing a home, just to be clear, this is not a subsidy literally for first time home purchase. It's subsidy for purchasing a home. So, someone who already owned a home or someone who had owned a home in the past was still eligible for 2-to-1 subsidy for home purchase. In

reality, it turned out to function much like a first time homebuyer's subsidy, but it was not literally a first time homebuyer's subsidy.

So the timeline -- there were four survey waves. What we focused on in this paper is the first one and the last one. What Greg and I focused on in the 2003 paper was the first one and the third one. Basically, nothing happened between the first and the second one because there was only 18 months apart. But the first and the fourth survey are about 10 years apart, and I'll refer to these generally as 10-year impacts.

So we contacted -- we were able to reach 80 percent of the original baseline group, which is a fairly high number after 10 years. We asked the same questions as earlier years, plus we asked some retrospective housing question surveys about -- to get a whole history of people's housing.

In terms of the sample -- balance in the sample, there's -- the treatment control groups are balanced in pretty much all dimensions. The one thing that's troubling is there's a difference in the baseline homeownership rates, which are higher for the control groups than for the treatment groups. There's not a statistically significant difference, but it affects the arithmetic calculations -- it is the reason why the aggregate difference in different results can be misleading. And we can talk about that if we need to.

There's some attrition from wave 1 to wave 4. As I mentioned, we only have 80 percent of the wave 1 sample. But the attrition is not systematically correlated with anything you would particularly care about. It's not correlated with treatment status; it's not correlated with baseline homeownership status. It's like there's one age group and one -- there's a -- you know, we have like 50 variables in the appendix that we're matching on, and 3 or 4 of them are correlated with attrition which, again, you would

expect some to be correlated. But there's no pattern, it's not like we lose younger people or we lose educated people or we lose high income people. There's nothing like that. And of course, in the regressions we control for anything that's correlated with attrition anyway, so. Which then removes the impact of that.

The sample is an interesting sample; it is not a representative sample of low-income households: 80 percent female, 41 percent African American. What's really interesting is more than 50 percent of this sample has some college experience, and that's much higher than you see in sort of like the PSID or the SIP or some other low income sample. And 84 percent have a bank account, which is also higher than you see in many low income samples.

And we think what's going on here is, if you remember the eligibility criteria, your income last year had to be below 150 percent of the poverty line. So anyone who was sort of a high permanent income person who happened to have a bad year might find the IDA particularly attractive to sign up for. And it turns out there's enormous income mobility in the sample, which I won't talk about here but it's stuff we've actually looked at.

SPEAKER: (inaudible)

MR. GALE: I'll get to -- it's roughly 800. It varies by regression. There's a -- roughly speaking, there's 200 baseline owners, 600 baseline renters. But it will vary by technique that we used.

So we used three different techniques to look at this. One is just a raw difference in difference -- you take the wave 4 value, you subtract out the wave 1 value for homeowners -- for treatment group versus control group.

Second one is regression, standard OLS regression analysis. I should mention, the dependent variable is homeownership, which is a 1-0 variable. So

technically, we should be using probit or logit or something like that. We've done that, the results are the same. It's just much easier to interpret the OLS numbers because the coefficients are the marginal effects, so we just report OLS numbers. On the paper that Greg and I did, we also reported the other results and they're virtually identical.

And we did some propensity scoring analysis, too, which doesn't change any of the results but it's kind of one of the things people check just to make sure the results are okay.

Importantly, we estimate the effects of being exposed to the IDA. That is, we estimate the effects of being in a treatment group. Sometimes people want to know what's the effect of opening an IDA? That's called an intent to treat estimate, and you can get that number by taking any of our numbers and dividing it by .9 because 90 percent of the sample opened -- 90 percent of the treatment group opened an IDA. So, the treatment effect -- the treatment on the treated, the TOT effects, are basically the same as the ITT effects because almost everybody opened an IDA.

And we estimate one-tailed test throughout the paper based on the hypothesis that IDAs ought to be raising the homeownership rate. It turns out nothing is affected by that. If we estimated two-tailed test we would have the same conclusions.

So let me just show you some of the basic results. What this -- this is actually a difference in difference, even though there's only two lines here. There's two zeroes, which you can't see because they're zero. But the control group basically, among renters in the control group -- baseline renters -- the homeownership rate started at 0, obviously, because they're renters, and went up to 42.6 percent at the year 10. And in the treatment group, they started as 0, went up to 45.3 percent.

So first, note the big rise in the control group. When I said that

homeownership rates went up in both groups, there's a big increase in homeownership in the control group. That presumably has nothing to do with the IDA. That presumably has to do with the characteristics of the control group, their motivations for saving, and so on.

The effect of the IDA is the difference between those two, and that is 2.7 percentage points. But the p-value is 24 percent. That is, there's a -- it's not significant at a 90, 95, 80, 85 percent level. It is significant at a 76 percent level, if you want to think of it that way. But generally, it's considered a statistically insignificant difference. And note that even if it were significant at 2.7 percentage points, it's much lower than the 7 to 11 percentage points that we estimate it in the earlier study.

For homeowners at baseline you have a slightly different pattern. I mean, obviously the homeownership rates are 1 at baseline because they're homeowners. The rate falls to 77.4 percent for the control group over 10 years and then falls to 79.1 percent over the 10 years for the treatment group. So there's a 1.7 percentage point increase for the treatment group relative to the control group. But again, it's -- the p-value here is .38 so it's not generally considered a statistically significant impact.

I'm going to skip over the aggregate numbers and just mention that there's a problem with the aggregate numbers because of this initial weighting of homeownership rates at baseline. We can come back to that and talk about it if anybody wants to. But basically there's a flaw built into the aggregate calculation that you solve by looking at owners and renters separately, okay?

Then we turned to sub-sample effects. We did 12 different sample splits. So this would be high-income/low-income, older than median/younger than median, more educated than median/less educated than median, black/white, have a bank

account/don't have a bank account, have health insurance/don't have health insurance, et cetera. We came up with 12 different criteria to split the sample on. So we estimated 24 subgroup estimates.

As I mentioned, one of them came in significantly positive, and that is for households with income above median income. And so there, the impact was almost 10 percentage points on the homeownership rate, which is a fairly sizeable impact. Greg and I got the same thing in our earlier study, so that should be noted.

But the problem is, is this a real effect? And this is where you can always generate some significant effects if you run enough regressions. We weren't specifically trying to do that, I can tell you, but we were trying to look at patterns in the data. So we got 24 estimates. Again -- suppose we had 20 estimates. You would expect one of them to be significant at a 5 percent level, even if all the effects were random, okay? That's what the 5 percent level means. There's a 1-in-20 chance that this effect is random, even though it comes in positive. You'd expect 2 of them to be significant at a 10 percent level. Again, even if all the effects were random, that's what the 10 percent level means.

So we actually only get 1 percent -- 1 treatment effect significant at the 10 percent level, which is this group for high-income households. It's not high-income; it's above median in the sample. I want to be clear: we're not talking about what we mean in tax policy when we say high-income households.

So, there's a sort of lingering issue. And we can't resolve it in the paper about whether this is a false positive or not. We can say, you know -- you can look at that effect and say it's significant, and you can -- for high income -- for above median income households. And you can give reasons why you think it makes sense that in --

how that higher-income, you know, moderate-income households have an impact where lower-income households don't. But you could also give reasons why it would make sense for other subgroups. You know, think about the banked versus the unbanked, those with health insurance, those that do not have health insurance, you know, those that have a car, the young versus the old. You can tell lots of plausible stories here. And so, the existence of a plausible story doesn't let you confirm that the estimate is a true positive or not.

All right. Then we turn to retrospective data, which is looking -- we asked people a series of questions about when they -- when did you buy your house, how long did you live there, when did you sell it, what did you do then, when did you buy the next house? So we got basically a time history on everyone's homeownership rate. And we've used that to look at how long people owned homes during the period.

And there were two goals here. One is to see how quickly that effect dissipated between 2003 and 2009. And the other is just to look at the average duration of homeownership over the sample period on the grounds that even if IDA didn't affect the long-term homeownership rate, if it raised the time that people spend owning homes, then that could have positive value, too, given that there's a literature on the positive effects of homeownership on, you know, social capital and stuff like that.

So let me just -- this is the key graph here. If you focus on the bottom two lines, the orange line is renters in the treatment group, the blue dots is renters in the control group. And you can see that in '99 the -- well, in '98 they both started at 0. But in '99 they gradually diverge so that by the end of 5 years there's this difference between the two groups.

Then the experiment ends and in 2004, everything goes away, right?

The homeownership rate for treatment group in 2004 is flat. I don't know if that literally means that nobody bought homes. It could be that some bought and some sold, but you get that little -- that one little flat line there in 2004 and that lets the control group catch up. If you look at the control group homeownership rate, it's just gradually going up year after year after year. This is a motivated group and basically, they catch up in 2004. And then from 2004 to 2008, they're basically the same, okay? So there's not a lasting tale here. It sort of goes away right when the experiment ends.

And then if you look at the mean years of homeownership, for renters in the control group it was 3.1 years over the 10 years. For renters in the treatment group it was 3.2. And then for owners it was 8.4 versus 8.7. None of these effects are statistically different.

And again, with 24 sub-sample effects -- 24 sub-samples, 1 is statistically significant. It's the same one as before, but -- the high-income households -- but that's not surprising if they have a higher homeownership rate at the end of the period. You would expect that if that accumulated over time, they would have more duration of homeownership. So, we still don't know whether this is a false positive or the real thing.

I'm going to skip over internal validity and external validity. And the conclusions I think I stated earlier, but I'll just summarize quickly. We don't get a statistically significant impact on the long-term homeownership rate. We don't get a statistically significant impact on the duration of homeownership. We do get a rapid change in homeownership patters right when the experiment ends in 2003.

We think what's going on here is that the incentives matter. That the incentive -- in particular for the treatment group -- to accelerate made a big difference,

because the gap closed rapidly as soon as the experiment ended.

And so the question is, where do we go from here? We're currently looking at other qualified uses for the funds. We're trying to estimate net worth impacts as well. We've done some preliminary work on the effect on financial attitudes, financial literacy. And aside from the impacts, IDAs also offer an opportunity to try to understand how policy works. There are several different levers in IDAs. There's the education, there's the case management, there's the hard budget incentives. We can't do anything separating those things out in this experiment because this experiment, that was all done as a package.

But if there are other experiments designed -- and I think there are some going on now -- where the financial education part is offered separately, you know, to some treatment groups whereas all treatment groups get the incentives -- the matching incentives. And to the extent that we can separate those, we can learn not only what's good policy but what motivates people to save.

So, let me stop there. And I couldn't ask for two better discussants. Greg was the lead author on the original APP study of the Tulsa experiment, and Reid is Mr. Everything in the world of asset building. So I'm looking forward to their comments.

Thank you.

MR. MILLS: Let's see. Click to exit. Let's see. This here?

SPEAKER: Yes. All right, there you go.

MR. MILLS: Good morning. First of all, I'd like to thank Bill for organizing this event. And to Brookings for hosting it, and also to the foundation sponsors that supported the wave for data collection and made this research possible and that brings us here today.

I also want to congratulate all of the research collaborators, not only those who are here at Brookings but also those from Washington University in St. Louis and also at University of North Carolina, in having very successfully completed this data collection and the analysis. Bill referred to the 81 percent completion rate, I think perhaps minimizing that -- in fact, it was a substantial accomplishment after 10 years to reach and successfully survey that high a percent of this sample.

I want to focus my remarks on three issues. The first is the effects by subgroup on that higher income sub-sample. The second is on what I've referred to kind of as the control group catch-up phenomenon. Bill has detailed that. And then third, on a possible approach for looking at the underlying patterns that led to these aggregate effects on the rate of homeownership and the duration of homeownership over the 10-year period. And with respect to that third issue, I'll be referring to some slides. And throughout, I'll pose some questions that perhaps we can return to during the Q&A.

So first, the effects on the higher income group. And again, as Bill has mentioned, we're talking here about those with above sample median income where the entire group had to meet the 150 percent of poverty level initial income eligibility limit.

So this finding, I think it's important to note, is in contrast and in some stark contrast, actually, to some of the earliest findings that came from the non-experimental correlation analysis at the outset of the American Dream Demonstration. So we're going back now 10+ years before enrollment and recruitment of this experimental sample in Tulsa where this was 1 of 13 sites. And there was early descriptive analysis that came from a small-scale pilot in Tulsa as well as the programs

implemented in the other sites.

And what that analysis showed at the time ---- again, it was a totally non-experimental and correlational -- but indicating that there was no relationship between income and the rate of savings and the rate of use of IDAs among IDA participants.

To me, at least, it points to the importance of instability in the lives of IDA households. And the fact that their capacity to save is importantly a function of their ability to withstand unexpected events that bring unplanned expenditures in their lives. Whether that's health, housing, transportation, maybe even a drop in income or an unexpected expense for extended family members. All of those adverse events can derail the intentions to save of families. And it's important to note as is indicated in the paper, in an IDA program -- even a successful one such as was implemented in Tulsa -- less than half of the IDA participants make a matched withdrawal. That is to say, the other half -- more than half -- use whatever savings they accumulate in their IDA for unmatched, unauthorized withdrawals.

So perhaps not surprisingly, then, that those who benefit most from these programs are those with higher incomes and who can sustain that habit of savings to the point that they could actually make use of the funds for their matched purposes.

A couple of questions I'd like to pose with respect to this. The paper that Bill and I had previously done looked at the pattern of savings over the calendar year. I think it would be of some interest to look at a pattern of savings again, and whether for the high-income group, indeed as we've found previously, these contributions to savings were largely during the tax season reflecting income tax refunds, EITC refunds in particular, and informing us as to whether, in fact, these programs work most effectively in conjunction with tax assistance, as we know was in place in Tulsa.

The other questions, was the effect on the homeownership rate and on the duration of homeownership for the high-income group a result of patterns among the baseline renters, or among the baseline owners? Was it a matter of acquisition of assets for this group or retention of the homes that some of them had at baseline?

Another matter, just totally factual, is what is the median income with respect to the percent of the poverty level? It's of some interest to know that with respect to the targeting of programs to those that are most likely to benefit.

So, the second question. To what extent are -- or, I should say, here again we're looking at the control group catch-up. And by the way, it's a lesson of experience in this field of experimental research. And I'm sure someone like James Carville could say this much more eloquently than I, but basically, it's the control group. And it's always the control group.

SPEAKER: (inaudible)

MR. MILLS: Well, thank you for saying that. I didn't want to say that from the podium here.

All too often in experimental research, the focus is on the treatment group. Quite obviously, they are the ones that are receiving the intervention that you're trying to test. But of course, experimental research differs from non-experimental research, most importantly in having a randomly assigned control group. And as certainly the case in this study as in many, one tends to lose sight of what that counter-factual experience is of the control group and how is it affecting your estimation of the program effects?

So, the paper cites -- so I want to focus a little bit on this issue of the control group catch-up. The paper notes a low rate of control group crossover during the

four years of the demonstration period. Twenty-one of the control group cases indicated that they actually received an IDA, even though by the rules of the program they were not to within that period. And another 27 entered CAPTC's down payment assistance program. Again, something that they, strictly speaking, were prohibited from entering.

I didn't see anything in the paper about the extent to which after the demonstration period. So during the years 5 through 10, control group cases then availed themselves of the opportunity to enter either an IDA program or some other form of home buyer financial assistance.

I think our interpretation of the control group catch-up and of the effects estimated of the program would be quite different, depending upon whether or not the control cases were accessing IDAs during that post-demonstration period. And it is, of course, an empirical question.

Why do I presume that there may have, indeed, been some rate of participation in IDAs for the control group cases? I went back just yesterday to look at some of the history with respect to the emergence and the development of the IDA field during these years, and certainly CAPTC -- those of you who know this organization -- and by the way, another congratulations should go to Steven Dow, who is the executive director of that program and importantly involved in the implementation of this research.

CAPTC played an important role in the adoption of IDAs nationwide. And during this period, the state of Oklahoma adopted a statewide IDA program that they called the Family Savings Initiative using TANF funds. And CAPTC was the administrative entity administering that program statewide. And so, following the completion of the demonstration period, I think there would have been every incentive for CAPTC to enroll control cases into that program. So as well, CAPTC became a grantee

under the Assets for Independence Act. So it was operating IDA programs and control cases, certainly, could have entered those programs after the four-year period. And I think we certainly want to know something about that.

I want to turn, then, in the remaining minutes to suggested approach for looking at the patterns that underlie the effects that Bill referred to. Effects that I think we could say are aggregate effects or stock effects, if you will. And I want to distinguish the stock effects from flows. The two outcomes were the homeownership rate at the 10th year, and then the duration of homeownership over the entire 10-year period.

Ted mentioned kind of getting into the black box, and in a sense I want to take a step and maybe suggest ways of doing that. It might be helpful also in looking at the effects for that higher-income group to do this analysis of the flows.

Okay. So, I'll refer to these -- to some slides that -- and my apologies for -- I guess you don't have these in front of you. Hopefully you're close enough to a screen.

So, as we said, if we look at the homeownership effects, they are a function of year-to-year transitions, the flows. And so any homeownership assistance program, whether it's an IDA program or others, would have its effects by altering the transitions with respect to housing tenure. Renter versus homeowner status: That is to say, increasing the probability that if you're a renter now, you'll be a homeowner next year; or if you're a homeowner now, you will remain a homeowner next year.

What I did was to look at the data -- and what I'm providing here are totally illustrative parameter values that seem to track the trends that Bill has indicated reasonably well. Sorry, again, for these numbers being so small. But basically, one can think of a 2-by-2 transition matrix indicating the probability that a renter in year -- in T

minus 1 will be either a renter or a homeowner the following year. Those probabilities, I take illustratively as .95 and .05, which is to say if you are a renter now 95 percent chance you'll be a renter next year, 5 percent chance you'll be a homeowner next year.

Then for homeowners this year, actually the parameters that seemed to reasonably track the data are just reversed. That is to say if you're a homeowner now, there's a 5 percent chance you'll become a renter next year, 95 percent chance you'll remain a homeowner.

Those you can -- would be what I call the pure control transition matrix for those that have no financial assistance available to them. For treatment cases, the probabilities are somewhat different. In particular, as somewhat higher probability if you are a renter this year, that you will become a homeowner the following year. Instead of 5 percent, 7 percent. And then if you were a renter this year, a somewhat higher percent -- I'm sorry, if you're a homeowner this year a somewhat higher percent you will remain a homeowner next year, 95 percent versus -- or 96 percent versus previously 95.

So one can think of these -- the regime that was in place for the Tulsa demonstration as one in which the treatment cases had access to IDAs but no other homebuyer financial assistance through years 1 through 4. Control cases had access to neither IDAs nor other homebuyer financial assistance. So you can consider them under that pure control scenario during the first four years. For years 5 through 10, both groups were actually under similar regimes. Both groups had access to homebuyer financial assistance, including IDAs.

So the hypothesis I kind of spin out in a simulation, as you'll see, is that the effect of the IDAs on homeownership may be approximately equivalent to that of other homebuyer financial assistance. So you can think of the treatment group as being

subject to a stable rate of transition probabilities for all 10 years. The control cases being subject to a pure control transition matrix in the first four years, and then shifting in the next six years to the very same transition probabilities that the treatment group faces throughout.

So this is what you have if you actually do this scenario, this simulation, as I call it. And again, I should emphasize I have not estimated these parameters. They're just simply illustrative. But they do seem to attract the data reasonably well.

You can see the dark line is the treatment group trend that ends at the value that Bill had shown on page 23 of his slides, where the treatment cases reached by the end of the 10th year a homeownership rate of in the mid 40s. A pure control scenario would -- the lower dotted line -- would have led to a homeownership rate in the low 30s. In fact, the control group as indicated -- although they followed that pure control scenario for the first 4 years, thereafter we're following the transition probabilities of the treatment cases leading to a homeownership rate slightly in excess of 40 percent with a roughly 3 percentage point difference, which was what Bill had indicated as present but not statistically significant.

And then similarly, I did the same kind of simulation for the baseline homeowners showing their trend in homeownership, which, again, tracks very closely the trend that was found in the data. Clearly, all homeowners at baseline are at 100 percent at year 0, the baseline year. And then trending downward, the treatment cases reaching a level in the mid to upper 70s. The control cases under that kind of hybrid transition where they were under pure control probabilities in the first four years. And then the treatment probabilities in the remaining six reached a level only slightly below the treatment cases. It was, again, 2 percentage points lower. So approximately what was

found in the data.

I guess the reason for going through this is to suggest that there is some interest in knowing in the field whether IDAs and other asset building programs act through encouragement of asset acquisition, or is it through the retention of assets that are previously owned? And to the extent it's one or the other, I think it's important to know what these flows look like in magnitude. And I think it's possible with this data to actually do that kind of hazard rate analysis and would be informative.

Thank you.

MR. CRAMER: Thank you. It's nice to be here this morning. Thanks for having me. Very interesting presentation and comments so far. And let me see if I can add to the discussion a bit.

You know, in the real world, we have problems and we usually have limited resources to address them. In an ideal world, perhaps we also might still have problems, but we can identify solutions and then would be able to assess the effectiveness of those solutions. So we know this is easier said than done. We know the real world is complicated, but it turns out the ideal world -- or at least striving for the ideal world -- is also messy. It's hard to design a meaningful experiment. It's hard to implement that intervention faithfully. And then, we usually get one shot to implement and observe what we find. We're stuck at that one moment in time.

This is a very important and laudable study, really striving for this elusive idea. And of course, that comes with this randomized experimental design where we have the control and treatment group. Very important to be able to make these observations. And then, the added value of being able to go back in time after all these years have passed to examine what the long-term impacts have been. Really an

important feature of this paper.

In an ideal world, we'd do this a lot more often. Really, this is how we can learn and make some progress in how we think about the world. And in fact many of our current policies, such as those focused on retirement savings, haven't really been scrutinized to this same extent through this mechanism. I think we'd benefit from that. There's a lot of money at stake, a lot of tax expenditures in play, and I think we'd be benefited from scrutinizing a lot of our policies this way.

As an aside, I'd note that we often spend a little bit more time focusing on policies and mechanisms that go to those lower down the economic ladder. And I think that's unfortunate. But that's really not the issue here in this paper. The authors of this study -- very diligent group, very committed to employing their methods fairly, and transparently. Bill's co-authors that had been noted at the Center for Social Development at Washington University and at UNC, really a great group committed to strong research, careful analysis, presenting the results accurately, and then exploring what the ramifications are.

And I think, you know, we're fortunate that this is a high-quality study that's been performed that looks at participation at a moment in time in this program in Tulsa, and then looks at its impact on long-term homeownership rates. These kinds of interventions, I think is -- what the authors and Greg can attest -- they're complex, they're difficult to run, and to study. But it is really an achievement to be able to pull them off.

Yet, analytically we do know that even when striving for the ideal when we have this experimental design, there is an importance in having some caution in how we look at the results. I do think very significant findings that come out of this that are important to emphasize -- the program did emphasize homeownership. It had a more

generous match, a 2-to-1, as opposed to the 1-to-1 for other allowable uses. So the program was focused on homeownership. And really, some remarkable findings. I mean, the 90 percent take-up rate for people offered the IDA, I think is very significant, very high when compared to other kinds of interventions. Contributions were significant. I think $1,800 over the course of the period -- the contributions that people made into these accounts.

And then, homeownership rates that increased substantially for the treatment group, and then subsequently for the control group as well. So, part of the story here is that both groups did extremely well over this period with respect to homeownership rates.

And after the 10 years, there was no significant difference between -- among the homeownership rates between the control and the treatment group. Of course, this is a relevant finding because we want to know if this intervention should be a basis for policy at a larger scale. We want to know whether, you know, this intervention is worth it in terms of effort and resources when there are alternatives, and there are obviously going to be resource constraints in this arena.

I think there are a few features of the overall context that I think are relevant that I do want to emphasize here and raise. They're all actually raised in the paper and given some attention in the external validity section. But I -- so I think the authors have recognized them. I just do want to highlight a few of them.

You know, one is the particularities of the Tulsa housing market at this moment in time. I mean, obviously prices of -- housing prices were low relative to a national market. I think this impacted barriers for homeownership that both groups would have experienced.

The second feature is the growing prevalence of access to credit that would enable people to become homeowners. This was even true for lower-income households that could access a variety of likely mortgage products. This meant that down payments might have been less of an obstacle than they might have been in the past to becoming a homeowner.

And the period after the treatment ended was probably also an unprecedented period where this access to housing credit was available. And this just, you know, would have been the case in Tulsa. It would have been the case all over the country, in many markets. But I think it was significant and I think unprecedented if we look back into the past.

I also think that it's unlikely that we'll experience this kind of access to credit in the future. And it's going to make it harder for a similar situated group, the control group, to kind of replicate the experience observed in the study.

And then the third feature to emphasize is the impact of the services that all the people in the study would have had access to eventually. You know, when I first read this study, my first thought was to think about the community organization that ran the intervention. Steven Dow has been mentioned, who runs CAPTC. They're a very effective organization. They've run a lot of different kinds of programs, offer a lot of services. I think it's important to recognize that not all community groups and service organizations are the same, and there's a lot of uneven capacity out there. And I think this is a very significant variable.

And for this reason, I've often thought that the comparison between those in the study and the general population was interesting and relevant as a means of distinguishing the impact of both the intervention and those that are getting access to the

other services of the organization. This paper actually does that. It shows that those in the study did very well relative to the general population, a similarly situated group. But the paper also notes that there's probably differences in this population in terms of their motivation. And that's something that Bill emphasized.

We do want to know what an intermediary can do with limited resources. If an organization can move its clients toward responsible homeownership in more cost-effective ways with limited resources, then it might be required for an IDA program. Maybe this is where they should be spending their attention and time.

So this is a very important issue for practitioners and policy types to consider. And it may support thinking about IDAs a bit differently. And maybe how they interact with other support systems and other objectives.

For instance, it might support lowering the match rate that is available in a program such as this. It might support thinking about longer periods of savings, that people would maybe be working with an organization for a longer period of time before they became homeowners. And maybe also an expanded menu of eligible uses beyond homeownership.

So in my mind, it's very relevant that, you know, we think of some of these possibilities. I also want to emphasize that I thought that the finding that the intervention accelerated the timing of purchase among the treatment group to be interesting. Even if the control group caught up, the IDA helped participants become homeowners faster. They raised the down payment, they got into the home buying pipeline sooner, and there's all sorts of reasons why this is important. I mean, homes often offer access to a bundle of services, neighborhood services, schools. It creates other opportunities for forced savings through paying down the mortgage; it creates

opportunities for other kinds of opportunities.

So I think this is a distinct issue from the question of duration, but -- you know, which was how long they were homeowners. So I think it is still relevant.

We've also seen a range of material that's not in the paper, but is around how people who have become homeowners recently have been affected by the economic downturn, and the foreclosure crisis. And basically, as I'm reading a lot of the different data, if you were -- well, certainly, if you were in an IDA program and became a homeowner, you have a lower rate of foreclosure and default than general population.

This is also seeming to hold true if you came through being a homeowner through housing counseling, maybe exposure to Self Help, which is an organization in North Carolina that has been working with lower-income homebuyers. So, it may affect the control group as well, but I think that families in general that became homeowners through some kind of mediated experience that involved a community group, they seem to weather the economic downturn better. It may be because they received better mortgage products, but I think that we've seen that this is a relevant finding that I think is very instructive. And we have to recognize that homeownership carries a lot of risks. The gains can be lost. And we need to also learn from the unfolding experience that's kind of happening across the country with respect to ownership.

So for me, you know, this assessment along with that experience does invite a reconsideration of how we view homeownership as an economic security and mobility strategy. So I think we're already seeing changes in the housing sector. I think the future of home buying remains uncertain at this moment in time. But we're likely to see it becoming more of an extended process requiring larger down payments, and then

the importance of savings subsequently will increase over time. And this might mean that there's real value in working with an organization and an intermediary like CAPTC that was able to run this program and maybe do such a good job for both the control and the treatment group.

I do think that there is this strong possibility that the conditions that allowed the control group to catch up have shifted. And I am assuming that this group purchased with lower down payments than the treatment group and perhaps with lower initial equity. I don't know that, that could be something that we looked at further. But I do know that in the future, I think down payments will matter more.

I do, you know, want us to use this study to think a little bit broadly about how we consider the IDA and how we expand its kind of implications beyond homeownership. The study is looking explicitly at homeownership outcomes. But I think we should explore the connections between savings and other kind of desirable socioeconomic outcomes. There's a growing body of evidence that's connecting savings to access, to postsecondary education, to economic mobility. The Pew's Economic Mobility Project is chronicling much of this. And I think that there are links between savings and economic security that do need further exploration. But I think there's a sound basis for thinking about the links between the two. And they're harder to observe than strictly the dichotomy between whether you're a homeowner or a renter.

We do know these families have diverse savings needs that extend beyond homeownership. And given what we've learned about the risks of housing and being an owner, we should be revisiting how we think about homeownership in the days ahead. I think it makes sense to expand the policy objectives that can help families move forward in their lives. Certainly recommend not elevating homeownership as an objective for all

families but, you know, thinking about where it makes sense for some families.

So I'd like to see greater policy supports for savings for other purposes. So, expand it into the retirement area, into the education arena. And I think many families would benefit from having access to savings that they could access on an emergency basis, small pools that could help them get through tough times.

Thank you. I look forward to the discussion.

MR. GALE: I'll just be real quick. First, with thanks to both Greg and Reid for really helpful comments. I should have added my appreciation for Steven Dow in my own talk. He did a great job, CAPTC does a great job. Nothing in the presentation should be taken as a criticism of either him or his organization, which has been a leader in all this.

I also want to heartily endorse something Reid said, which was, we need more experiments on all sorts of saving, not just those that affect low-income households. We spend about $200 billion on tax expenditures for saving. If we took a mere 1 percent of that and ran $2 billion a year worth of experiments -- which, of course, is way off the scale of any experiments we ever run -- but just for 1 percent of that we could learn an enormous amount about what works and what doesn't work in terms of saving behavior.

Greg mentioned that we should estimate sort of hazard functions. That is, the probability of moving in and out of renters and homeownership status. And seeing whether it's an acquisition or a retention issue. That's a good idea, we should do that. I think the answer is going to be, it's a little bit of both, knowing -- well, you saw what the difference was for renters and what it was for homeowners. But we could do that and we should.

I think Reid kind of not explicitly, but implicitly suggested we look at

home equity and home values. And we'd like to do that. There's some missing data there that we need to either impute or otherwise figure out what to do with.

I do want to mention one other issue, which is this issue of what happened to the control group after the experiment ended? It's correct that it could have been that some weird stuff happened to them after the experiment ended. And there's no way to fix that. You know, the experiment only ran once. You know, history played out after that. So there's an issue there.

But it's not -- Greg didn't say this but I wanted to be clear that it's not a problem for the experiment that crossovers might have used home subsidies or IDAs after the experiment ended, right? The "experiment" is the effect of giving people the IDAs for four years. And now we're looking at the long-term effects of giving people IDAs for four years. What happened after that may be of interest for external validity purposes, but it's -- there's nothing wrong, there's nothing -- the experiment isn't undermined if the control group then goes out and gets housing subsidies after the experiment is over. In fact, in some ways it's telling because if there are already these subsidies out there, then the marginal value of layering the IDA on top of that may not be that big. And that is one interpretation of what our results are showing.

But I just want to be clear that the fact that the control group -- the control group wasn't banned for life from using housing subsidies. They were just banned for those four years while the treatment group got the IDA. And that's what we're actually measuring the effect of.

Okay, thanks.

MR. JOHN: Now it's your turn. Oh, good. We'll start with you, please.

MS. CHRISTOPHER: My name is Jan Christopher. I'm from Delaware

State University. I'm a professor.

I'm actually from Tulsa, Oklahoma. And I haven't lived there for about 20 years, but I know the culture. And I know for a fact that people took on second jobs to front load those IDAs so they could get those homes. And so I'm sort of curious as to why or if your survey asks were they working more than one job?

And then the other question I had was, some of these houses that have popped up in Tulsa, they're placing old houses next to new houses. And I'm wondering whether or not do you have any geographic information of whether or not the IDA were funding new houses, older houses, what type of housing were they getting credit for to be able to purchase the home? That would be very interesting to me, because you have a lot of neighborhoods with brand new houses next to very, very old, dilapidated houses.

And then the other question I had was, were any of these people able to build homes even though they were low income? Were they able to build brand new homes as opposed to buying older homes? And I was wondering: were any of those questions asked in your survey?

MR. JOHN: All right. We're going to take a couple questions. Please wait for the microphone and then we'll give the panel a chance to respond. Yes, ma'am.

MS. RADEMACHER: Hi, thanks. Ida Rademacher at CFED. Thank you so much for the study and the great presentations all across the board today.

I wanted to get back, Bill, to the question of the validity of the 10-year findings. And it think that the validity makes total sense in the way that you've described it, in the report does. But the generalizability of 10-year findings to saying that IDAs didn't have a discernable difference is just something I think we really do need to explore. I mean, we do know, as Greg said, it's all about the control group. We know that this was

a highly motivated group, even at the get-go. And that they had, as you note in the paper, a real incentive not to purchase in those first four years because they couldn't have access to the kinds of programs.

So I think that without knowing the uptake of services -- especially IDAs but other services as well -- but without knowing what percentage of the control group did that in the six-year time period following the end of the treatment, I think that we really can't -- I don't know that it's appropriate. And it might even be misleading to come to a conclusion that says that the impact of IDAs disappears after 10 years.

So I just think that that's going to be a rich place for discussion over time, and a very important thing to look at. And if you do have any sense of uptake of IDAs or if the program does, we'd really like to know that. That would be great, thanks.

MR. JOHN: All right, great.

MR. GALE: Sure. Two great comments.

On the working issue, the labor supply in Tulsa -- a person who works here named Ben Harris just completed a study as part of his doctoral dissertation that looked at the labor supply effects of the individual development counts and didn't find any net impact on the average between treatment and control. And I can -- talk to me afterwards, I can get you that study.

MS. CHRISTOPHER: Did you ask if they have second jobs, though, in your survey at all?

MR. GALE: Well, there was -- did you participate, how many hours, how much earnings. I don't know if they asked specifically whether the earnings came from one job or two jobs.

MS. CHRISTOPHER: That's very important.

MR. GALE: Yeah, okay.

MS. CHRISTOPHER: Because I know for a fact that people did take on second jobs.

MR. GALE: Good. Well, there -- okay.

And on the using geographic information, we have not linked up to the kind of GPS kind of data sets that let you -- you know, we know where these people live, basically. So we could get exact geographic information and use it in the way that you've talked about. And that would be an interesting follow-up. I hadn't thought about doing that.

In terms of the generalizability, Ida, everything you said I think is right. I mean, we -- the benefit of doing experiment is you get a very clean answer to a very specific question. The specific question is, what is the effect of this four-year IDA program implemented in Tulsa, you know, starting in 1999? So anything that happens in Tulsa or, you know, that could affect the generalizability of the results. And that's just an inherent issue with experiments, and the right way to deal with that is to accumulate more evidence, more experience.

And so I wouldn't conclude from this that IDAs have no effects. I would conclude from this that this IDA had no effect on homeownership after 10 years. Right? And so, you know, now you have to think about how comfortable you are -- anyone is -- inferring, you know, broader generalizations from that. But you're absolutely right. This is a specific experiment; it doesn't answer the universal question about all ideas.

MS. RADEMACHER: But it's that broader piece for me that's important about what percentage of the control group actually did get an IDA within this 10-year period. It's not a cross -- it's a post-experiment crossover, for sure, but I think that we

have this desire to know that. I think the field would want to know that.

MR. GALE: Right. They didn't get this IDA. They didn't get the 2-to-1 match that the Ford Foundation sponsored. A few of them did, Greg mentioned that. And we control for that in the analysis, it doesn't affect much of anything if you --

MS. RADEMACHER: And the six years following.

MR. GALE: But in the six years -- they -- no, a few of them got the Ford Foundation, if you'll call it that, IDA during the four years. We control for that and take that out.

That IDA did not exist after four years, so they may have gone somewhere else and gotten some other IDA, right? So that's why I'm saying we can't conclusively say, you know, that IDAs have no effect. What we can say, this four-year experiment -- the impact of it does not show up in the long-term results.

MR. MILLS: Okay. I think the only point I would make is -- I think it is -- there would be data from CAPTC that I think would enable one to look at the question. I don't know that the data were actually from this survey. I don't know whether at wave 4 the question was asked, let's say of control cases, had they received within the years 5 though 10 -- had they participated in a down payment assistance program, let's say, either an IDA or some other.

I suppose it's certainly -- I would think it would be possible to get that information, though, from CAPTC and then to match with the sample and determine that.

MR. JOHN: All right. One more set of questions, please. One here. All right, yes.

SPEAKER: 2003 was a period of historically low interest rates for housing. And so, just as your IDA experiment was ending, people who were in the

control group all of a sudden had what appears to be much lower housing cost opportunities to them that maybe might have exceeded in terms of savings the value of the down payment that people had with the IDA. How is that going to affect your analysis?

MR. JOHN: All right, we've got one down here?

SPEAKER: This is following up on a comment that Reid had. So I think -- I presume that someone who bought a house using the IDA is better off than someone who bought a house not using the IDA. And if there's any measures of kind of how much equity they have in their house and the propensity to foreclose?

And I guess kind of a slightly unfair question related to that -- and I don't know what happened to the Tulsa housing market -- is somebody who bought a house without an IDA better off than someone who remained a renter in that market? Because it seems like you're looking at peak years. And again, that had no effect with what you're finding. But I'm wondering, was there appreciation or was it better off to remain a renter even with the subsidy?

MR. JOHN: One more, over here. And this, unfortunately, will be the last question.

SPEAKER: Could you say something about the control group? How was it selected?

MR. JOHN: All right. Bill?

MR. GALE: Sure. Thanks again. Good questions.

With response to Ted's question about the Tulsa housing market, you can correct me if I'm wrong but my impression is that the Tulsa housing market is like the land that time forgot. (Laughter) It doesn't go up, it doesn't crash, it just kind of bops

along.

MR. MILLS: It is.

MR. GALE: Fairly steady rate.

The question in the back about low interest rates in 2003, that's correct. It falls into this category of the external validity of the experiment. But note that it's also to a treatment group members that hadn't bought houses. They also had access to low interest rates in that time period. So, these are things that cannot be resolved within the formal confines of the experiment. But they're good things to note, basically.

And then in terms of the control group, how it was selected. There were recruiting methods to bring in people into the experiment. Greg can probably talk about that better than I can. And then each month, a cohort was brought in and they were randomized -- well, first they were surveyed, then they were randomized into a treatment group and a control group. And it was a purely random procedure. Originally the assignment ratio was five treatments to six controls, because they were worried about attrition of controls. But then after a while, they changed it to one-to-one. And Greg, maybe you want to add more about how they were selected?

MR. MILLS: It was done over about a 14-month period. We were targeting on 1,100. We indeed recruited that many. And there was the prescreening, as Bill has indicated, by the CAPTC staff to determine that someone met the eligibility -- income eligibility standard.

And then once that was determined, the APP survey staff interviewed them by phone to get the baseline survey data, wave one data. And at that point they were randomly assigned. And then if a treatment group, they were -- they kind of entered the program and were offered the opportunity to open an IDA. And 90 percent did. And then

the control cases were subject to the control group regime, which was, with very few exceptions, faithfully implemented by the CAPTC staff.

MR. CRAMER: I'm not an expert in the Tulsa housing market or the history of Oklahoma, but we do know that this is probably a stable housing market that didn't experience some of the impacts of the housing bubble.

That said, foreclosures are up nationwide. I assume they're up in Tulsa as well with some of the fluctuation in the employment opportunities. And I think this is one of the unfolding tragedies that's going on across the country, is the impact that the recession has had on families and their balance sheets and there's been a lot of wealth loss and declining asset ownership. And I think that's something that we need to use to look critically at how we design interventions and policies that might help people move forward.

Because homeownership, I think for a number of people, it is a way to move up. And I think for others it actually carries a lot more risk. And I assume that it's impacted the people in this study as well. And I think that that's something that we should inform how we think about policy in the future.

MR. JOHN: And the debate will go on. If you'll join me in thanking our speakers today. (Applause)

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CERTIFICATE OF NOTARY PUBLIC

I, Carleton J. Anderson, III do hereby certify that the forgoing electronic file when originally transmitted was reduced to text at my direction; that said transcript is a true record of the proceedings therein referenced; that I am neither counsel for, related to, nor employed by any of the parties to the action in which these proceedings were taken; and, furthermore, that I am neither a relative or employee of any attorney or counsel employed by the parties hereto, nor financially or otherwise interested in the outcome of this action.

/s/Carleton J. Anderson, III

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