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TECHNICAL REPORT

The Effects of Tulsa Remote on Inducing Moves to Tulsa: Estimates and Implications

Upjohn Institute Technical Report No. 25-050

May 2025

Timothy J. Bartik
W.E. Upjohn Institute for Employment Research

ABSTRACT

Tulsa Remote is one of the largest of over 100 remote worker attraction programs in the United States. Since its beginning in 2018, the program has provided generous incentives ($10,000 for moving to Tulsa and staying one year), along with accompanying networking services and incentives to encourage entrepreneurship, to over 3,000 Tulsa Remote member households, selected out of tens of thousands of applicants. Tulsa Remote's purpose is to promote economic development in Tulsa. Tulsa Remote members spend more locally and generate fiscal benefits, as well as start up businesses and help make Tulsa more attractive for high-tech job growth. These benefits depend on the degree to which Tulsa Remote actually induces Tulsa moves. What percentage of approved Tulsa Remote applicants would not have moved to Tulsa "but for" the incentive offer? Using data on applicant households, including both approved and not approved by Tulsa Remote, this report estimates Tulsa Remote's "but for" rate. Using a variety of models, this report estimates a "but for" rate of 58–70 percent, averaged across all member households. The report then enters these "but for" estimates into a regional econometric model that simulates the effects of Tulsa Remote on the per capita incomes of the original Tulsa residents. In the baseline estimates, the benefits to original Tulsa residents, in higher incomes, are over four times the program's financial costs. Simulations also show that benefits go up with the following: a higher "but for" rate, a higher retention rate, higher wages of Tulsa Remote members, higher entrepreneurship effects among Tulsa Remote members, and increased housing supply to accommodate the induced population growth. The program's support activities and other policies accompanying the program (e.g., housing supply policies) are at least as important as the incentive payments and the selection process for membership in Tulsa Remote.

JEL Classification Codes: J61, R23, R11

Key Words: Regional migration, remote workers, local economic development

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EXECUTIVE SUMMARY

This report evaluates the benefits of Tulsa Remote, one of the largest and most generous remote worker attraction programs in the United States. Tulsa Remote induces sufficient in-migration to Tulsa such that benefits for other Tulsa residents significantly exceed the program's costs.

Since 2018, Tulsa Remote has offered a $10,000 payment to remote workers for moving to Tulsa and staying for one year. This incentive payment is selectively offered to applicants, with preference given to high-skilled tech workers. Out of tens of thousands of applicants, Tulsa Remote has so far attracted over 3,000 households to Tulsa. Tulsa Remote encourages these Tulsa Remote "members" to be engaged in the community and to start new businesses.

Using data on the migration choices of Tulsa Remote applicants, the report estimates how approval into the Tulsa Remote program affects the probability of moving to Tulsa, controlling for several other factors that may affect migration. These estimates imply that 58–70 percent of Tulsa Remote members would not have moved to Tulsa "but for" being approved for the Tulsa Remote program.

These induced relocations of remote workers directly create local jobs, but they also bring one or more additional workers, which reduces the direct benefits for other Tulsa residents. However, the induced job and worker relocations also stimulate the creation of indirect jobs, through multiplier and other spillovers, and these do significantly benefit other Tulsa residents. Tulsa Remote members buy local goods and services, creating more demand for Tulsa retail jobs. The high skills of Tulsa Remote members also help encourage other employers to locate or

expand in Tulsa. Entrepreneurship by Tulsa Remote members further increases overall Tulsa jobs.

By inputting the induced job and population migration effects of Tulsa Remote into a regional econometric model, the Bartik Benefit-Cost Model of Business Incentives, this report estimates that indirect multiplier and spillover effects from the Tulsa Remote program yield benefits for other Tulsa residents that are over four times the program's costs.

These benefits are roughly evenly divided among higher labor earnings, greater availability of state and local tax revenue, and increased property values. The labor market improves because indirect job creation increases local employment rates and real wages for other Tulsa residents. Additionally, the total job creation and population in-migration increase state and local tax bases by more than they increase required public services for the higher population; this results in "fiscal benefits," allowing tax rates to be cut or public spending to increase. Finally, the induced in-migration increases property values, providing capital gains for Tulsa property owners.

The distribution of Tulsa Remote benefits is moderately progressive. The share of benefits accruing to the bottom three-fifths of the income distribution exceeds this group's baseline income share, thereby making the net income distribution more equal. However, benefit-cost ratios for the program exceed 3-to-1 for all income groups, so the program's benefits are less targeted toward poorer residents than is true of most social welfare programs.

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INTRODUCTION

Since its beginning in 2018, the Tulsa Remote program has provided thousands of remote workers with a generous incentive for relocating to Tulsa. The program seeks to boost Tulsa's economy. But such a boost depends on the extent to which the program induces workers to move to Tulsa, versus providing incentives to workers who would have moved to Tulsa anyway. This report estimates the program's effects on its immediate goal: inducing workers to move to Tulsa. These estimates are then used to simulate plausible program benefits.

Tulsa Remote is one of the largest and most generous of the over 100 remote worker attraction programs offered around the United States by various communities and states. Eligible applicants for Tulsa Remote are remote workers who have not recently lived in Oklahoma, with preference in selection given to remote workers with high incomes and high-tech skills, and who seem more likely to remain in Tulsa and be engaged in the Tulsa community. From its beginning in late 2018, Tulsa Remote has had over 3,000 selected applicants move to Tulsa, out of tens of thousands of applicants. The Tulsa Remote incentive is generous: $10,000 for those who move to Tulsa and stay for at least one year. Furthermore, the program is much more than the cash incentive. For example, Tulsa Remote provides services to encourage "Remoters" to become involved with community organizations and start up new businesses in Tulsa.

The rationale for Tulsa Remote is its promotion of local economic development: attracting these carefully selected remote workers is expected to boost the local economy. Part of this boost is "demand-side effects": attracting the selected remote workers will enhance the Tulsa economy with new income, increasing demand for local goods and services, and creating local spinoff jobs; for example, in retail sectors such as restaurants, grocery stores, and hardware

stores. But there may also be "supply-side effects." The relocated remote workers may start up new businesses in Tulsa, which the Tulsa Remote program tries to encourage. Tulsa Remote may also help attract new high-tech employers to Tulsa. If these remote workers leave their remote jobs and take a job at a Tulsa-based employer, their skills may increase the productivity of Tulsa employers, which will enhance their competitiveness, encourage job creation, and facilitate local wage increases.

The benefits of Tulsa Remote—increases in current Tulsa residents' real earnings per capita due to job creation and higher labor productivity—depend on whether the program induces in-migration. Tulsa Remote does try to screen out individuals who would have moved to Tulsa anyway, for example, by asking in the application whether they were already planning to move to Tulsa. But obviously, even with such screening, some approved "Tulsa Remoters" will inevitably include persons who would have moved to Tulsa without the program. What percentage of the 3,0000-plus Tulsa Remoters would not have moved to Tulsa "but for" the incentive?

This report's first aim is to estimate this "but for" percentage for Tulsa Remote. To do so, this report matches administrative data from Tulsa Remote with moving data from a commercial data provider, Infutor. This merger of data allows us to compare the moving behavior of "treated individuals"—those approved by Tulsa Remote for the $10,000 moving incentive—with "comparison individuals"—applicants similar to those approved by Tulsa Remote, but who were not approved.

This report's key new empirical finding is the following: the "but for" effect of Tulsa Remote on inducing moves to Tulsa is between 58 percent and 70 percent. That is, 58–70 percent of Tulsa Remote members who moved to Tulsa would not have moved to Tulsa "but for" the

incentive offer. The variation between 58 percent and 70 percent is due to different models, all of which are reasonably plausible. Is 58 percent a high enough "but for" percentage"? In baseball, it would be—a batting average of at least 0.580! For basketball free throw shooting, this number is mediocre—a free throw shooting percentage of only 58 percent!

Whether 58 percent to 70 percent is "large" depends on the benefits per Tulsa Remoter compared to the cost. To address this issue, the report estimates the economic benefits of Tulsa Remote. These economic benefits are the gains in real income for the original Tulsa residents. Benefits for Tulsa Remote members are ignored in these calculations.

The economic benefits are estimated by entering Tulsa Remote into a regional economic model of how "economic development programs," which shock employment, interact with the workings of the local economy to affect local incomes. Two-thirds of this report is devoted to explaining this model and then presenting the analysis. This lengthy exposition is needed because such a benefit-cost analysis of Tulsa Remote has not previously been done. Without such an analysis, the new estimates of a "but for" of 58–70 percent are not as meaningful.

Based on simulating this model, this report concludes that 58 percent is indeed "large," in the following sense: the benefits of Tulsa Remote plausibly are over quadruple the program's costs, including the extra costs due to incentives going to some persons who would have moved to Tulsa anyway. This benefit-cost calculation also counts as costs not just the $10,000 incentive payment per Tulsa Remote member, but the extra $5,000-plus (for a total cost per member of $15,000-plus) that the program pays to run the program selection and to provide Tulsa Remote members with other services to encourage community involvement and entrepreneurship. The analysis also is conservative in that it ignores that a portion of these costs are charitably funded and might not otherwise be devoted to Tulsa, which means that some of these costs might be a

"free lunch" for Tulsa. Furthermore, as already mentioned, these benefit-cost calculations only consider effects on the original Tulsa residents. Presumably the program also benefits Tulsa Remote members—otherwise they would have turned down the incentive offer—but such benefits are not considered in this report's benefit-cost figures.

Another sense in which a "but for" rate of 58 percent is large is compared to business tax incentives. Studies of business tax incentives suggest that unless business tax incentives are very large, the "but for" rate will be far below 58 percent. As I will discuss in more detail later, based on the research literature on business location decisions, a business tax incentive of similar costs to Tulsa Remote—offering a little over $15,000 per job to a business considering locating or expanding in Tulsa—would be expected to "tip" only 6 precent of these location or expansion decisions. Ninety-four percent of these jobs would have been created in Tulsa anyway, meaning that only 6 percent of the jobs would not have been created "but for" a $15,000 per job incentive offer to the business. To put it another way, in order for a business tax incentive to have a "but for" rate of 58 percent, the incentive offer for the business job-creation project would have to be at least $267,000 per job.

Therefore, Tulsa Remote tends to have higher benefit-cost ratios than many business tax incentives, because it is a relatively cheap way to create local jobs. Why is Tulsa Remote lower cost per induced job than business tax incentives? A plausible hypothesis is that remote workers are more "footloose"—that is, more flexible in choosing locations—than is true of the typical business job-creation project.

An overall benefit-cost ratio that exceeds 4-to-1 is quite large. But what about the distribution of these benefits? Do the benefits go to the poor, the middle class, or the rich? Based on the program's estimated "but for," and the simulation model, the benefits are moderately

progressively distributed. In particular, a higher percentage of net benefits go to the bottom three income quintiles—that is, to households whose income is in the bottom 60 percent of the Tulsa income distribution—than their baseline share of overall income. Therefore, the program tends to increase the share of the bottom 60 percent of the income distribution in overall Tulsa income. The economic pie is sliced more evenly. On the other hand, Tulsa's original residents in all income groups gain from the Tulsa Remote program. The benefit-cost ratio for the program exceeds 3-to-1 for each one of the five income quintiles, from low to middle to upper income. These broad benefits may increase the political sustainability of the Tulsa Remote program.

The "but for" rate for Tulsa Remote is lower for groups with prior ties to Tulsa, such as those who have lived previously in Tulsa. However, even for this group with a lower "but for" rate, Tulsa Remote may have benefits that exceed costs if this group has a higher "retention rate"—that is, more of those Tulsa Remote members with strong prior ties may remain in Tulsa 10 years later.

As the report will show, the benefit-cost ratio depends on factors other than the "but for" rate or the retention rate. In particular, Tulsa Remote's benefits depend on whether Tulsa Remote members are paid higher wages, the extent to which the program encourages Tulsa Remoters to start new businesses, the effect of the Tulsa Remote program on business attraction, and local policies to expand housing supply to accommodate population growth. Higher wages for those selected for Tulsa Remote will increase fiscal benefits for state and local governments by boosting the tax base, and will have multiplier effects on local jobs by increasing demand for local goods and services. If the Tulsa Remote program succeeds in recruiting high-tech workers who are more likely to start up businesses, or who are more attractive to high-tech employers, the program will have greater long-run spin-off effects on boosting the Tulsa area's job growth.

Finally, if Tulsa Remote's attraction of remote workers is accommodated by policies that expand the supply of new homes and apartments sufficiently to reduce upward pressure on housing prices, this in turn will reduce upward pressure on overall local prices and nominal wages. These lower upward cost pressures will keep the Tulsa area more attractive for job growth.

In sum, the benefits and costs for Tulsa Remote depend on more than the $10,000 incentive offer and the "but for" that results from this job offer. The program's effects also depend on a broader, integrated local strategy. The services provided by the Tulsa Remote program are at least as important in increasing community impact by boosting retention and entrepreneurship. Finally, because Tulsa Remote is an economic development strategy that by design grows an economy by selectively encouraging population growth, policies to accommodate population growth by added housing supply also are needed to maximize the potential of the Tulsa Remote strategy.

MATCHING THE DATA: PROCEDURES AND RESULTS

To estimate the effects of Tulsa Remote on inducing moves to Tulsa, we merged administrative data from Tulsa Remote—on all applicants, regardless of whether or not they were selected for the incentives offer—with moving data from a private consumer-data company, Infutor. Tulsa Remote obviously knows whether approved applicants move to Tulsa in order to qualify them for the incentive and to involve them in other program activities (e.g., introducing them to various community features). But the program has no easy way to accurately determine whether unsuccessful applicants moved to Tulsa..[1]

[1] For example, in a recent survey that included rejected applicants to Tulsa Remote, the response rate of this rejected group to the survey was only 1.4 percent. (Teodorovicz, Choudhury, and Starr 2024). Not surprisingly, being rejected from Tulsa Remote does not lead people to be eager to answer surveys related to the program.

Infutor is a company that tries to compile consumer data on every person in the United States, and then sells these data to businesses for marketing purposes. These data include information on where the person lives at various times. From past uses of the Infutor data, Upjohn Institute staff have concluded that this Infutor "location information" is not always perfectly accurate and timely. In particular, there are often long lags in whether a person's move to a new location is reflected in the Infutor data, and in some cases Infutor simply loses track of a person or at least their location.

However, under the reasonable assumption that Infutor's imperfect moving information is similarly imperfect for both successful and unsuccessful applicants to Tulsa, these imperfections will not bias the estimates. The missing or incomplete data will add some statistical noise to the estimation. The estimates of the effects of Tulsa Remote on inducing Tulsa moves will become more imprecise. But there will be no bias because there is no reason to think that Infutor does any better or worse at capturing Tulsa moves for successful versus unsuccessful applicants.

To match Tulsa Remote data with Infutor data, our first step is to match on name combined with the city and state of residence at the time the person applied to Tulsa Remote. This name/city/state matching obviously often results in multiple matches (e.g., John Smith in Atlanta, Georgia). To get more unique and more accurate matches, in cases where Tulsa Remote has house number and street name information for the residence at the time of application, we see if we can identify an Infutor observation with the same house number and street name. If so, we assume that this is the best match and use the moving information from Infutor for that observation, throwing out the other initially identified matches. In a few other cases, we find in Infutor a house number and street name that is close to the Tulsa Remote data (e.g., the number is slightly scrambled), and we then treat that Infutor observation as the correct match.

However, in many cases, we do not have house number and street names, or we cannot find that house number and street name in Infutor. In that case, we retain all the possible Infutor matches. However, in these cases we weight the observation by (1/number of matches to that Tulsa Remote observation). In other words, both the descriptive statistics and the empirical estimation treat the matched Infutor observations, both unique and multiple, each as separate observations. But all these statistics use a weight of 1 if the match is unique, and, for example, a weight of one-half if two Infutor observations are matched to the same Tulsa Remote observation. The notion is that the weight of (1/number of Infutor matches) is reflecting that the probability that this match is correct is proportional to this weighting variable.[2]

The final administrative data provided by Tulsa Remote had 56,053 observations. After removing applications without first or last name, without city or state, non-U.S. applications, and applications received after March 1, 2023, we ended up with 41,415 applicants. March 1, 2023, was chosen because our Infutor data only track location data up to December 1, 2023. We wanted to give at least some reasonable time after the application date for the application to be possibly approved and the person to move to Tulsa, in this case at least eight months.

We were able to successfully match 17,725 applicants to at least one record in Infutor, or about a 43 percent match rate. Then, we excluded applicants who a) applied from Oklahoma or b) already bought a house or signed a lease in Tulsa, because these applicants were ineligible under program rules. Finally, we also removed observations with too many Infutor matches. Specifically, we got rid of the about 10 percent of match cases where the number of Infutor matches exceeded 10. The final sample, our "big sample" used in estimations, ended up with

[2] I say "proportional" to the probability that this is the correct match because even if the match is unique, this unique match may be incorrect. Maybe we have someone named Keyser Soze in Tulsa Remote who applied from New York City, and we also have only one person with that same name in New York City in the Infutor data, but maybe it is a different Keyser Soze.

16,692 Tulsa Remote records, or 29,003 Infutor observations corresponding to our 16,692 Tulsa Remote observations. Out of the 16,692 matches, 11,141 are unique matches.

For our estimation, we also used a smaller interview sample. This sample is restricted to individuals who, based on a preliminary review of their application, were invited to an interview. Interview invitations were made when Tulsa Remote staff thought that the person was clearly eligible for the Tulsa Remote program and might be a good candidate. There end up being 11,517 Infutor observations in the "interview sample," corresponding to 6,960 Tulsa Remote observations.

Why use a smaller interview sample? Restricting ourselves to this sample removes some ineligible individuals from consideration, which is a plus. In addition, this restricts us to individuals who could be argued to be more "comparable" on both observed and unobserved characteristics than is true of the full sample. Thus, restricting ourselves to the interview sample may better control for unobservable variables in estimating the effect of the Tulsa Remote incentive on moving behavior.

Why not only use the interview sample? Because the big sample reflects the effects of the Tulsa Remote incentive offer including the effect of being selected to be interviewed. Therefore, the full sample estimate is really answering the question of what is the effect of both being selected to interview and then being offered an incentive to move to Tulsa. The interview sample may better control for unobservable variables, but instead answers the following question: conditional on being selected for an interview, how does the incentive offer affect the probability of moving to Tulsa? In sum, the interview sample better controls for unobservables, but then necessarily answers a somewhat more limited question. On the whole, I think it is better to control for unobservables, but the "big sample" estimates are included for completeness.

To get a better sense of how well Infutor measures moves, as well as for some basic information on Tulsa Remote approval probabilities, Tables 1 and 2 report some cross-tabulations, for the big sample and the interview sample, respectively, of Infutor moving to Tulsa probabilities with either of the following: whether Tulsa Remote approved the applicant for an incentive, which Tulsa Remote administrative data measure as a finalist; or whether the person ended up becoming a member, which is what Tulsa Remote calls someone who is offered an incentive and actually moves to Tulsa.[3]

These tables are a bit complicated. I report both frequencies, which is the number of Infutor/Tulsa Remote observations in a particular cross-tabulation cell, and the "sum of weights" for that cell, where the sum of weights reflects how many Tulsa Remote observations that cell corresponds to. Thus, for the big sample, the total number of observations on which the frequencies are based is 29,003, but the sum of weights, corresponding to the Tulsa Remote observation number, is 16,692. For the interview sample, the total number of observations on which the frequencies are based is 11,517, but the total sum of weights, corresponding to the Tulsa Remote observation numbers, is 6,960. The tables calculate various percentages reflecting the probabilities that the event represented by one cell occurs conditional on some other cell occurring. These probabilities always are based on the various sums of weights and thus can be interpreted as the probability that a particular type of Tulsa Remote applicant has some event that they are selected for or that they select.

Looking first at Table 1, the big sample, we find that 16.8 percent of the Tulsa Remote applicants are approved by the program. That is, they are designated by Tulsa Remote as a

[3] The "treatment" that is chosen by Tulsa Remote is whether they are a finalist in the administrative data, which is what I refer to as Tulsa Remote approving the person for the program. Membership depends on the person's choice of whether or not to move to Tulsa. Technically, you could say that Tulsa Remote "determines" membership, but it is determined based on the person's choice, once they are approved for the incentive and become a finalist.

Table 1 Descriptive Statistics for Tulsa Remote Approval, Infutor Moves to Tulsa, and Tulsa Remote Membership, Big Sample

Panel A: Tulsa Remote Approval versus Infutor Tulsa Moves					
	Rejected		Approved		
	Frequency	Sum of weights	Frequency	Sum of weights	Probability of approval (Approval sum of weights divided by approval plus rejected sum of weights) (%)
No Infutor Tulsa move	24,024	13,630	4,230	2,420	
Infutor Tulsa move	302	258	447	385	
Total	24,326	13,888	4,677	2,804	16.80%
Probability of Infutor Tulsa move (Sum of weights move/sum of weights total) (%)		1.86		13.71	
Difference in probability (probability if approved minus probability if rejected) (%)				11.85	
Raw estimate of induced move percent (difference/approve) (%)				86.5	
Panel B: Tulsa Remote Member versus Infutor Tulsa Move					
	Not member		Member		
	Frequency	Sum of weights	Frequency	Sum of weights	Probability of member (sum of weights member/sum of weights both member and non-member) (%)
No Infutor Tulsa move	26,734	15,250	1,520	800	
Infutor Tulsa move	316	271	433	372	
Total	27,050	15,521	1,953	1,171	7.02
Probability of member being detected by Infutor Tulsa move (sum of weights Infutor move in Panel B/member total sum of weights) (%)				31.7	
Probability of member among those approved (sum of weights member total in Panel B/approved total sum of weights in Panel A) (%)			41.8		
Inferred true probability of moving to Tulsa among those rejected (prob of Infutor moves rejected*ratio of member % among those approved/Infutor move % among those approved) (%)			5.7		

finalist, who will receive the incentive if they choose to move to Tulsa and remain for a year. Of those applicants, according to Infutor move data, 13.71 percent of those approved by Tulsa Remote moved to Tulsa. But among rejected Tulsa Remote applicants, 1.86 percent moved to Tulsa anyway. Therefore, ignoring any other observable or unobservable differences between

Table 2 Descriptive Statistics for Tulsa Remote Approval, Infutor Moves to Tulsa, and Tulsa Remote Membership, Interview Sample

Panel A: Tulsa Remote Approval versus Infutor Tulsa Moves					
	Rejected		Approved		
	Frequency	Sum of weights	Frequency	Sum of weights	Probability of approval (approval sum of weights divided by approval plus rejected sum of weights) (%)
No Infutor Tulsa move	6,715	4,051	4,229	2,419	
Infutor Tulsa move	126	106	447	385	
Total	6,841	4,157	4,676	2,803	40.27
Probability of Infutor Tulsa move (sum of weights move/sum of weights total) (%)		2.55		13.72	
Difference in probability (probability if approved minus probability if rejected) (%)				11.17	
Raw estimate of induced move percent (difference/approve) (%)				81.4	
Panel B: Tulsa Remote Member versus Infutor Tulsa Move					
	Not member		Member		
	Frequency	Sum of weights	Frequency	Sum of weights	Probability of member (sum of weights member/sum of weights both member and non-member
No Infutor Tulsa move	9,425	5,671	1,519	799	
Infutor Tulsa move	140	119	433	372	
Total	9,565	5,790	1,952	1,170	16.81
Probability of member being detected by Infutor Tulsa move (sum of weights Infutor move in Panel B/member total sum of weights) (%)				31.8	
Probability of member among those approved (sum of weights member total in Panel B/approved total sum of weights in Panel A) (%)			41.7		
Inferred true probability of moving to Tulsa among those rejected (prob of Infutor moves rejected*ratio of member % among those approved/Infutor move % among those approved) (%)			7.8		

SOURCE: Author's calculations.

those approved by Tulsa Remote versus those rejected, the inferred "effect" of Tulsa Remote is to increase the probability of moving to Tulsa by 11.85 percent (13.71 − 1.86). Among those moving to Tulsa, and thus eligible for the incentive, an estimated 86.5 percent were induced to

move by the incentive—that is, they would not have moved to Tulsa "but for" the incentive (86.5 = 11.85/13.71).

But this assumes that Infutor actually measures all moves to Tulsa, rather than simply having some probability of measuring that move. Tulsa Remote has a good idea of whether approved applicants moved to Tulsa. As shown in Panel B of Table 1, 41.8 percent of approved applicants moved to Tulsa, considerably greater than the 13.7 percent figure based on Infutor-detected moves.[4] In other words, Infutor appears to have only a probability of 31.7 percent of detecting an actual move to Tulsa (31.7 = 13.7/41.8).

There is no reason to think that Infutor has a lower or higher probability of detecting moves to Tulsa for rejected applicants, compared to approved applicants. Therefore, the implication is that the true probability of moving to Tulsa among rejected applicants is 5.7 percent (5.7 = 1.86 × 41.8/13.7). This seems intuitively plausible—people already interested in moving to Tulsa for other reasons are more likely to apply to a program such as Tulsa Remote, and a probability of about 6 percent passes a sniff test. Under this assumption, the "but for" percentage is the same even correcting for Infutor's imperfect moving measure. In other words, we would get 86.5 percent for an induced moving probability using the 41.8 percent and 5.7 percent moving probabilities for approved versus rejected applicants (86.5 percent = (41.8 − 5.7)/41.8).

Looking at Table 2, the interview sample, as one would expect among this sample of persons that Tulsa Remote felt were eligible and good candidates for an interview, the probability that an applicant who is interviewed is approved is 40.3 percent. The Infutor move probability

[4] Tulsa Remote refers to "finalists" who move to Tulsa as "members." As mentioned above, "finalist" is a designation that refers to a choice controlled totally by Tulsa Remote among applicants. "Membership," although administratively designated by Tulsa Remote, is due to the finalist's choice to move to Tulsa.

for those approved is basically the same at 13.7 percent, but among rejected applicants in this group, the probability of an Infutor-detected move to Tulsa is higher at 2.6 percent. Therefore, the induced percentage, or "but for" percentage, is somewhat lower at 81.4 percent. When we adjust for Infutor only detecting about 32 percent of actual moves to Tulsa, we get that the rejected applicants among those selected for interview had a 7.8 percent probability of moving to Tulsa anyway, versus a 41.7 percent probability for those approved by the program, which again leads to an inducement percentage of 81.4 percent (81.4 percent = (41.7 – 7.8) / 41.7). The implication is that those selected by Tulsa Remote for an interview are a somewhat more mobile group, or at least somewhat more likely to move to Tulsa anyway, but not by a large amount extra. Or, to put it another way, the comparison group in the interview sample is somewhat more similar to the approved group than is true in the big sample, and it turns out that making the groups more comparable slightly reduces the inducement percentage of the program.

But all of this analysis of the inducement effects of Tulsa Remote makes no attempt to control for observable variables that might be correlated with the likelihood that someone moves to Tulsa, and might also be correlated with whether someone is approved by Tulsa Remote. In the next section, I will briefly describe various control variables that will be used, before going on to use these control variables in estimating the effects of Tulsa Remote on moving behavior.

CONTROL VARIABLES

To come closer to estimating the "true effect" of Tulsa Remote on moving to Tulsa, it is important to control for different variables. The important control variables are those that might be correlated with the probability that someone might move to Tulsa, and might also be correlated with whether Tulsa Remote selects the applicant for the program. If such variables are

omitted from the subsequent statistical analysis, they may cause "omitted variable bias": if the variable is omitted, it may appear that Tulsa Remote approval is having an effect that is really due to the omitted variable having an effect on moving to Tulsa that is falsely attributed to Tulsa Remote.

Various plausible Tulsa Remote control variables are included in the Tulsa Remote administrative data. Table 3 presents some descriptive statistics for these control variables, in both the big sample and the interview sample. These control variables are all categorical variables. The table includes percentages of the total sample in each category based on the sum of the weights in that category, which corresponds to the number of Tulsa Remote applicants in the data.

Table 3 Descriptive Statistics on Control Variables

	Big sample			Interview sample		
	Frequency	Sum of weights	Percent	Frequency	Sum of weights	Percent
Total	29,003	16,692	100.00	11,517	6,960	100.00
RECODE of age						
18–24	455	262	1.6	264	159	2.3
25–34	3,212	2,021	12.1	2,379	1,493	21.5
35–44	2,619	1,591	9.5	1,908	1,187	17.1
45–54	1,212	737	4.4	890	551	7.9
55–88	592	376	2.3	408	279	4.0
missing	20,913	11,705	70.1	5,668	3,291	47.3
Gender						
female	5,613	3,532	21.2	4,358	2,744	39.4
male or non-binary	5,366	3,094	18.5	4,402	2,571	36.9
missing	18,024	10,066	60.3	2,757	1,645	23.6
Race						
white or Caucasian	4,410	2,771	16.6	3,715	2,323	33.4
non-white	5,500	3,267	19.6	4,057	2,450	35.2
missing	19,093	10,654	63.8	3,745	2,187	31.4
Education						
none	353	167	1.0	37	26	0.4
high school	9,766	5,427	32.5	2,287	1,390	20.0
associate	3,076	1,705	10.2	1,300	775	11.1
bachelor	10,283	6,087	36.5	5,042	3,044	43.7
master/doctor	4,591	2,813	16.9	2,739	1,669	24.0
missing	934	493	3.0	112	56	0.8

Table 3 (Continued)

	Big sample			Interview sample		
	Frequency	Sum of weights	Percent	Frequency	Sum of weights	Percent
Industry category						
computers	1,839	1,106	6.6	1,200	732	10.5
health related	2,041	1,261	7.6	869	558	8.0
finance/bank/real estate	2,977	1,755	10.5	1,470	897	12.9
management and PR	5,264	3,006	18.0	2,364	1,400	20.1
retail/wholesale/other goods and services	2,808	1,532	9.2	739	427	6.1
other	7,282	4,254	25.5	3,283	2,017	29.0
missing	6,792	3,778	22.6	1,592	929	13.3
Income						
less than $40k	5,512	3,059	18.3	1,080	630	9.1
$40k–$60k	4,889	2,782	16.7	2,214	1,319	19.0
$60k–$100k	4,574	2,854	17.1	3,395	2,162	31.1
$100K+	2,965	1,781	10.7	2,621	1,589	22.8
missing	11,063	6,216	37.2	2,207	1,260	18.1
Employment category						
business owner/self-employed/small business owner	8,359	4,604	27.6	1,480	845	12.1
full-time remote/can be remote/independent contractor	7,166	4,028	24.1	3,226	1,899	27.3
full time employee or none of these	10,055	6,007	36.0	5,587	3,451	49.6
missing	3,423	2,053	12.3	1,224	765	11.0
total	29,003	16,692	100.0	11,517	6,960	100.0
Know someone in Tulsa						
false	21,666	12,372	74.1	7,849	4,735	68.0
true	7,337	4,320	25.9	3,668	2,225	32.0
Relocation plan						
was considering moving to Tulsa	1,618	1,021	6.1	1,598	1,007	14.5
was planning on moving but not to Tulsa	2,238	1,391	8.3	2,237	1,390	20.0
wasn't planning on moving anywhere	570	385	2.3	569	384	5.5
missing	24,577	13,895	83.2	7,113	4,179	60.0
Region applying from						
Midwest	4,390	2,394	14.3	1,480	857	12.3
Northeast	2,388	1,482	8.9	975	625	9.0
"reference states" (nearby=KS, MO, AR, TX, LA)	6,463	3,619	21.7	2,552	1,515	21.8
South Atlantic states	7,546	4,494	26.9	2,953	1,856	26.7
Western states	8,216	4,703	28.2	3,557	2,107	30.3
Previously lived in Tulsa						
false	27,172	15,627	93.6	10,584	6,391	91.8
true	1,831	1,065	6.4	933	569	8.2
Years at prior address						
less than 1 year	2,490	1,466	8.8	1,710	998	14.3
1–5 years	7,759	4,695	28.1	4,170	2,570	36.9
6–10 years	2,988	1,831	11.0	1,351	841	12.1
more than 10 years	9,121	4,803	28.8	2,456	1,403	20.2
missing	6,645	3,897	23.3	1,830	1,148	16.5

SOURCE: Author's calculations.

As shown in the table, for many variables, a high percentage of the Tulsa Remote observations are missing. This is due both to Tulsa Remote applications not always consistently asking particular questions over time (the program has had three different administrative data systems over time), or to applicants not responding.[5] To deal with this "missing variable" problem in the subsequent empirical analysis, the statistical analysis will also include the "missing category" as a separate category rather than dropping observations that are missing.

Judging from the non-missing observations, a disproportionate percentage of applicants fit the profile of a mobile person who might be interested in a program aimed at attracting highly educated remote workers to Tulsa. The applicants are more highly educated than average, have relatively high incomes, are employed more in more technical and professional industries, and are in the younger age range of 25–44.[6] A relatively high proportion of the applicants (compared to likely U.S. averages) also have some connection to Tulsa, either knowing someone in Tulsa or having previously lived in Tulsa, or having already been interested in moving to Tulsa. This is even more true of the interview sample, which tends to disproportionately select persons who are more educated, have higher income, are in more technical or professional industries, are younger, and have various sorts of connections to Tulsa. All of this makes sense given the goals of the program: to attract persons to Tulsa who will make a strong economic and social contribution to Tulsa, and who will stay in Tulsa long-term.

[5] Because the missing categories correspond in part to different administrative time periods, the control variables implicitly control for some time period fixed effects. Future work with a Tulsa Remote sample that covers more years (the current sample is mainly applicants from 2021 and 2022) would enable an exploration of how the program's impact might vary over time.

[6] The industries are aggregated from a much larger list. See Appendix A for how these industry groups are defined.

Applicants also seem to be disproportionately from nearby states, as one would expect, and less from the Northeast and Midwest. Gender and race variables have a very high percentage of "missing" data, but overall, the applicants evenly represent both males and females, and whites and non-whites.[7]

To reduce bias, it is likely important to control for these variables. Many of these variables seem likely to be correlated with how often a person moves, and whether they move to Tulsa. And it seems plausible that they might be correlated with whether Tulsa Remote approves an application. Therefore, the raw "but for" percentages inferred in the prior section might be biased due to these omitted variables.

REGRESSIONS WITH LINEAR PROBABILITY MODELS TO ESTIMATE AVERAGE EFFECTS OF TULSA REMOTE

Our first approach is to estimate the probability of an Infutor-detected move to Tulsa, as a function of Tulsa Remote approval, with controls for all the other personal characteristics listed above (age, etc.), in a linear probability model. This is done for this study's "big sample" of Tulsa Remote applicants, and this study's "interview sample" of only those applicants who were invited for an interview.

A linear probability model means that the dependent variable, a zero-one variable for whether someone is observed to move to Tulsa in the Infutor data, which we are seeking to explain, is expressed as a linear function of whether or not the person was approved by Tulsa Remote, and a linear function of all these control variables. The linear probability model has the

[7] The non-white percentage might be seen as somewhat surprisingly high, but is hard to interpret due to a high percentage missing, and changes in how data on this are collected over time. The data from surveys in Teodorovicz, Choudhury, and Starr (2024) did show more non-white applicants than some might guess.

advantage that its coefficients are easy to interpret: the coefficient on these variables simply represents the effect of a one-unit change in the right-hand side (RHS) variable on the probability of the person moving to Tulsa, as measured in the Infutor data. Because all our RHS variables are categorical variables, the coefficients simply reflect the estimated effects of being in that category on the probability of moving to Tulsa, relative to the omitted category.

Linear probability models are sometimes criticized because for some combination of values of the right-hand-side (RHS) variables, the resulting predicted probabilities will exceed one or be less than zero. Predicted probabilities outside the zero-one interval do not make any sense. However, as pointed out by many econometricians (for example, see Chen, Martin, and Wooldridge [2023]), the purpose of such regression models is not to predict the probability for each observation in the sample, but rather to predict the average change in the probability for a change in a particular RHS variable, holding the other variables constant. In this case, our main interest is to know how much the probability of a move to Tulsa on average changes if someone is approved for Tulsa Remote, holding other moving determinants constant. For detecting these "average partial effects" of a change in one variable, linear probability models have been shown to often be quite good, compared to more complicated to interpret non-linear alternatives, such as logit or probit (Chen, Martin, and Wooldridge 2023). However, in some cases, linear probability models do not do as well as estimating average effects of a RHS variable. Therefore, it is useful to double-check to see if the linear probability model estimates are robust to more complex models. I do this in the section after this one, and the estimates are robust.

Table 4 shows the linear probability regression model for the big sample with all the categorical control variables. I also show for comparison a model that has no control variables—

Table 4 Linear Probability Model Regressions Predicting Probability of Infutor-Detected Move to Tulsa, Big Sample, without and with Controls

	Without controls		With controls			
	Coefficient	Standard error	Coefficient	Standard error	Probability of test stat of this size if group has true zero effect, missing excluded	Probability including missing category
Approved by Tulsa Remote	0.11855	(0.00628)	0.09086	(0.00738)	<0.001	<0.001
RECODE of age					0.735	0.714
18–24			0.00546	(0.01381)		
25–34			0.00976	(0.00745)		
35–44			0			
45–54			0.00167	(0.00966)		
55–88			0.00928	(0.01409)		
missing			−0.02135	(0.02830)		
Gender					0.038	0.110
female			0.01261	(0.00607)		
male or non-binary			0			
missing			0.00201	(0.01202)		
Race/ethnicity					<0.001	<0.001
white or Caucasian			0.02851	(0.00640)		
non-white			0			
missing			0.02217	(0.01170)		
Education					0.175	0.100
none			−0.00627	(0.00471)		
high school			0			
associate			−0.00108	(0.00451)		
bachelor			0.00570	(0.00330)		
master/doctor			0.00437	(0.00464)		
missing			0.01413	(0.00675)		
Industry category					0.318	<0.001
computers			−0.00453	(0.00765)		
health related			−0.00156	(0.00605)		
finance/bank/real estate			0.00604	(0.00595)		
management and PR			0			
retail/wholesale/other goods and svcs			−0.00433	(0.00502)		
other			0.00481	(0.00465)		
missing			−0.02183	(0.00551)		
Income					<0.001	<0.001
less than $40k			−0.00451	(0.00375)		
$40k–$60k			0			
$60k–$100k			0.01591	(0.00564)		
$100K+			0.01273	(0.00724)		
missing			−0.02783	(0.00670)		
Employment Category					0.013	0.014
business owner/self- employed/small business owner			−0.00991	(0.00354)		
full-time remote/can be remote/independent contractor			−0.01490	(0.00593)		
full time employee			0			
missing			−0.03960	(0.02545)		

Table 4 (Continued)

	Without controls		With controls			
	Coefficient	Standard error	Coefficient	Standard error	Probability of test stat of this size if group has true zero effect, missing excluded	Probability including missing category
Know someone in Tulsa					<0.001	<0.001
false			−0.03306	(0.00417)		
true			0			
Relocation plan					0.539	0.162
was considering moving to Tulsa			0.00824	(0.01131)		
was planning on moving but not to Tulsa			−0.00114	(0.00987)		
wasn't planning on moving anywhere			0			
missing			0.05290	(0.02753)		
Region applying from					0.153	0.233
Midwest			0.00589	(0.00460)		
Northeast			−0.00568	(0.00498)		
"reference states" (nearby=KS, MO, AR, TX, LA)			0			
south Atlantic states			0.00079	(0.00400)		
western states			0.00241	(0.00425)		
Previously lived in Tulsa					<0.001	<0.001
false			−0.06899	(0.01004)		
true			0			
Years at prior address					<0.001	<0.001
less than 1 year			−0.00336	(0.00577)		
1–5 years			0			
6–10 years			−0.00005	(0.00527)		
more than 10 years			−0.01687	(0.00326)		
missing			0.01294	(0.00693)		
Intercept			0.07858	(0.01506)		
Number of observations	29,003					

NOTE: LPM regression is weighted by 1/number of matches to Tulsa Remote data. Standard errors in parentheses. Standard errors are robust. Table shows omitted category in each set of categorical variables as being set to zero.
SOURCE: Author's estimation of model.

that is, the probability of moving to Tulsa is simply predicted by a single RHS variable (and a constant), the probability of being approved by Tulsa Remote.

The linear probability model (LPM) regressions without controls simply replicate the comparison between mean probabilities of moving to Tulsa of those applicants who were improved and rejected by the program. As discussed previously, the probability of those approved by the program of moving to Tulsa was 13.71 percent, versus 1.86 percent for those applicants who were not approved by the program. The difference is 11.85 percent, which is

precisely what is estimated in the LPM regression without controls. The only new information is the standard error of this estimate, which is 0.628 percent, so the estimated effect is highly statistically significant.[8]

The estimated effect on moving to Tulsa of Tulsa Remote approval is reduced when we add controls. Before discussing this reduced effect, note that the control variables often make a difference, and that the estimated effects often seem reasonable. The regression statistics reported include statistics for the probability of different groups of variables being statistically significant. One column shows results for all categorical variables in a category being significant, excluding the "missing" category from consideration. The other column also includes the "missing" category. I emphasize the first column, as it represents whether or not the various non-missing categories make a difference to moving probabilities. If "missing" is significant, the interpretation is not obvious—it could have to do, for example, with changes over time that are unobserved that have nothing to do with that category of variable, but simply happen to be correlated because data availability varies over time.

Focusing on the categorical variables that are statistically significant even when we ignore the missing category—that is, have a probability of less than 0.05 of coefficients of that size occurring by chance even if the true coefficients were all zero—we observe that the following categories have "statistically significant effects": gender, race, income, employment category, know someone in Tulsa, previously lived in Tulsa, years at address applying from. Many of these effects seem reasonable. Among applicants, higher income persons who know someone in Tulsa or have previously lived in Tulsa, and have lived at their current location less

[8] We could, of course, have gotten this same standard error simply by calculating the standard error of the difference between the two sample means in Table 2 for probability of moving to Tulsa for the approved subsample versus the unapproved subsample.

than 10 years, are more likely to move to Tulsa. For example, other things equal, someone who doesn't know anyone in Tulsa is 3.3 percentage points less likely to move to Tulsa than someone who does know someone in Tulsa. Someone who never lived in Tulsa is 6.9 percent less likely to move to Tulsa than someone who did previously live in Tulsa.[9]

But the main variable of interest is the effect of Tulsa Remote approval on the probability of moving to Tulsa. This is reduced from an effect of 11.85 percentage points without controls, to an effect of 9.09 percentage points with controls. Because those approved by Tulsa Remote have an Infutor-detected move probability of 13.71 percent, the implied "inducement percentage"—the percentage of those approved by the program who move to Tulsa and would not have moved to Tulsa "but for" the program—is 66.3 percent (= 9.09 / 13.71).[10] This 66.3 percent "but for" with controls is reduced from the 86.5 percent "but for" that is estimated without controls. The estimated effect of Tulsa Remote is still highly statistically significant (a t-statistic of 12.31 compared to a null-hypothesis of a zero coefficient), but is scaled back by about 20 percentage points (20.2 percentage points = 86.5 percent − 66.3 percent).

Why does this change occur? Presumably because omitting controls causes omitted variable bias. Some variables that are positively (negatively) associated with Tulsa Remote approval are also positively (negatively) associated with the applicant moving to Tulsa even without the program.

In Appendix B, I decompose the omitted variable bias. I conclude that the bias is due to a number of variables, but perhaps the biggest influence is controlling for whether a person either

[9] It is also true that women, whites, and those classified as remote or independent contractors are less likely to move to Tulsa compared to those classified as full-time employees, and those patterns are perhaps harder to readily explain.

[10] As before, adjusting for Infutor having some probability of missing moves would not change this inducement percentage. We would blow up both 9.09 and 13.71 by 1 over the estimated percentage of Tulsa moves detected by Infutor.

knew someone in Tulsa or had previously lived in Tulsa. These variables predict moving to Tulsa anyway, and are positively correlated with Tulsa Remote approval.

A similar LPM regression was run for the interview sample. As mentioned above, this smaller sample has the advantage of better controlling for possible unobservable variables that might affect both the probability of moving to Tulsa and whether someone was approved for Tulsa Remote, and thereby bias the results. Table 5 shows the LPM estimates for the interview sample.

Table 5 Linear Probability Model Regressions Predicting Probability of Infutor-Detected Move to Tulsa, Interview Sample, without and with Controls

	Without controls		With controls		Probability of test stat of this size if group has true zero effect, missing excluded	Probability including missing category
	Coefficient	Standard error	Coefficient	Standard error		
Approved by Tulsa Remote	0.11168	(0.00661)	0.08501	(0.00796)	<0.001	<0.001
RECODE of age					0.711	0.829
18–24			−0.00476	(0.01767)		
25–34			0.00768	(0.00934)		
35–44			0			
45–54			−0.00692	(0.01150)		
55–88			0.00768	(0.01685)		
missing			0.00491	(0.03465)		
Gender					0.009	0.034
female			0.01833	(0.00706)		
male or non-binary			0			
missing			0.00606	(0.01331)		
Race/ethnicity					<0.001	<0.001
white or Caucasian			0.02770	(0.00747)		
non-white			0			
missing			0.00585	(0.01319)		
Education					0.016	0.031
none			−0.02660	(0.00919)		
high school			0			
associate			−0.00943	(0.00890)		
bachelor			0.00276	(0.00764)		
master/doctor			0.00522	(0.00935)		
missing			−0.00111	(0.02523)		
Industry category					0.72	0.003
computers			−0.00421	(0.01188)		
health related			0.00227	(0.01195)		
finance/bank/real estate			0.00433	(0.01017)		
management and PR			0			
retail/wholesale/other goods and svcs			−0.01222	(0.01163)		
other			0.00472	(0.00864)		
missing			−0.04103	(0.01166)		

Table 5 (Continued)

	Without controls		With controls		Probability of test stat of this size if group has true zero effect, missing excluded	Probability including missing category
	Coefficient	Standard error	Coefficient	Standard error		
Income					0.166	<0.001
less than $40k			0.00976	(0.00807)		
$40k–$60k			0			
$60k–$100k			0.01806	(0.00816)		
$100K+			0.01250	(0.00941)		
Missing			−0.06325	(0.01536)		
Employment Category					0.035	0.029
business owner/self-employed/small business owner			−0.02154	(0.00902)		
full-time remote/can be remote/independent contractor			−0.02283	(0.01080)		
full time employee			0			
missing			−0.05220	(0.03015)		
Know someone in Tulsa					<0.001	<0.001
false			−0.03446	(0.00733)		
true			0			
Relocation plan					0.561	0.235
was considering moving to Tulsa			0.00778	(0.01157)		
was planning on moving but not to Tulsa			−0.00159	(0.00996)		
wasn't planning on moving anywhere			0			
missing			0.05627	(0.03288)		
Region applying from					0.019	0.040
Midwest			0.01478	(0.01055)		
Northeast			−0.01957	(0.00991)		
"reference states" (nearby=KS, MO, AR, TX, LA)			0			
South Atlantic states			−0.00098	(0.00830)		
Western states			−0.00033	(0.00842)		
Previously lived in Tulsa					<0.001	<0.001
false			−0.08177	(0.01583)		
true			0			
Years at prior address					<0.001	<0.001
less than 1 year			−0.00797	(0.00833)		
1–5 years			0			
6–10 years			−0.00484	(0.00949)		
more than 10 years			−0.03665	(0.00685)		
missing			0.03315	(0.01829)		
Intercept			0.10071	(0.02132)		
Number of observations	11,517					

NOTE: Weighted by 1/number of matches to Tulsa Remote data.
SOURCE: Author's estimates.

Before turning to the key coefficient—the effects of Tulsa Remote approval—a brief comment on the control variable effects. Overall, the control variables have a similar pattern as

with the full sample. The one exception is that for the interview sample, applicants from the Northeast are less likely to move to Tulsa.

In these new estimates, including the control variables reduces the Tulsa Remote approval effect on the Infutor-detected probability of moving to Tulsa, from 11.2 percent to 8.5 percent. Both are highly statistically significant and precisely estimated. The implied "but for" is reduced from 81 percent without controls, to 62 percent with controls. That is, the Infutor-detected probability of moving to Tulsa is 13.72 percent. The estimated effect of 8.5 percent is 62 percent of this probability, implying that 38 percent of those who were approved by the program, and who moved to Tulsa, would have moved to Tulsa even without the program.

What control variables drive this difference between the estimates without controls and those with controls? Appendix B explores this in more detail. Although there are influences by many control variables, as with the big sample, the interview sample results suggest that the control variables for whether an applicant knew someone in Tulsa or had previously lived in Tulsa had the biggest effects. These two variables had significant effects on whether someone moved to Tulsa, and were positively correlated with being approved for the program by Tulsa Remote.

LOGIT MODELS OF HOW TULSA REMOTE AFFECTED THE PROBABILITY OF MOVING TO TULSA

To make sure the results are robust to functional form, estimates are also done in a logit formulation. Such a specification attempts to fit the model with a non-linear functional form in the probability of moving to Tulsa. This has the advantage of preventing predictions of

probabilities that exceed one or are less than zero. The cost is that the coefficient estimates do not directly tell us how a given variable affects moving probabilities.[11]

The logit estimates in general are qualitatively similar to the linear probability model estimates. Focusing on the control variables, for both the big sample and the interview sample, an applicant was more likely to move to Tulsa if they knew someone in Tulsa, or had previously lived in Tulsa, or had lived at their current location less than 10 years. These findings are quite robust to the different specifications. Appendix C presents the logit estimates.

To help interpret these estimates, computations were done of the effects of "treatment on the treated." Using the logit estimates, simulations were done of the estimated probability of moving to Tulsa for all those who were approved for the program, due both to that approval and to the particular values of the control variables for that observation. Then we created an imaginary sample of these "treated observations" in an imaginary world where Tulsa Remote did not exist. This means that each individual observation, which was "treated" (approved for the program) in the real world, had the approval variable reset to zero, to reflect this imaginary world. But all of the control variables' values stayed the same. The probability of moving to Tulsa in this imaginary world was then recomputed for each treated observation.

Table 6 shows the results. For comparison, results are also shown comparing this "treated on the treated" logit simulation "but for" with the "but for" from the linear probability models.

As this shows, the implied "but for" from the logit model, compared to the linear probability model with the same sample, is slightly greater, particularly for the interview sample.

[11] However, as Appendix C explains, with a little work the coefficients can be interpreted. The model could also be estimated with a probit model. However, probit estimates are even harder to interpret. In practice, the choice of probit versus logit rarely makes any difference.

Table 6 Simulations of Effects of Tulsa Remote on Probability of Moving to Tulsa, Logit Models of Effects of "Treatment on Treated"

	Probability of Move to Tulsa				
	For those approved	Same persons if unapproved	Difference	Implied "but for" (%)	"But for" from LPM model (%)
Big sample	0.13713	0.04503	0.09209	67.2	66.3
Interview sample	0.13713	0.04148	0.09564	69.7	62.0

NOTE: Simulations based on logit model with estimates reported in Appendix C. Simulations calculate weighted mean probability for those treated with and without treatment variable (approved by Tulsa Remote) turned on. Underlying Tulsa Remote effect in big sample logit model has t-statistic of 11.38, and in interview sample, 9.97.

SOURCE: Author's estimates.

What explains this slightly greater "but for"? The logit model essentially allows for the following likely possibility: the estimated effect of any variable, including Tulsa Remote approval, on the probability of moving will be less for persons whose characteristics would lead us to predict a moving probability that is either close to zero or close to one. Because the moving probability cannot be less than zero or more than one, changes in variables have less of an effect for observations whose characteristics push them closer to the extremes. The overall sample has a probability of moving to Tulsa that averages a little under 14 percent for the "treated group," and a little under 2 percent for the untreated group. The LPM assumes the marginal effect of the Tulsa Remote approval on the probability of a Tulsa move for the treated group and the untreated group will be the same. But the logit model allows for the likelihood that for persons otherwise unlikely to move to Tulsa, the marginal effects of program approval or lack of program approval are likely to be lower on the probability of moving, expressed in percentage points terms.[12]

Logit estimates are also done that allow the effect of Tulsa Remote approval to vary with all of the control variables. That is, in addition to allowing the probability of moving to Tulsa to

[12] More technically, the LPM is a model of the form $p = B_0 + B_x * X$, whereas the logit model is of the form $\ln(p/(1-p)) = C_0 + C_x*X$, where p is the probability of moving to Tulsa, X are predictor variables (including Tulsa Remote approval dummy variable), B_0 and C_0 are the constant terms, and B_x and C_x are vectors of slope coefficients on the X variables. The linear form of the LPM means that the probability changes the same at all values of p, whereas the logit specification means that the change in the probability of moving as X changes will be lower if p is either close to zero or one.

vary with program approval, and the various control variables, the specification includes a complete set of interactions between program approval and the control variables. This allows us to see whether Tulsa Remote approval has different effects on applicants with different characteristics.[13]

The only program-approval/control variable interactions that were statistically significant were those with the two control variables for whether the person knew someone in Tulsa or had previously lived in Tulsa.[14] In both cases, program approval had greater effects on individuals who did *not* already know someone in Tulsa, or who had not previously lived in Tulsa.

To determine the practical significance of these interactions, Table 7 reports simulations of the effects of "treatment on the treated" for various groups in both the big sample and the interview sample. That is, we calculate the predicted percentage who would have still moved to Tulsa if they had not been approved for the program, versus those who actually moved, for persons differentiated by whether they knew someone in Tulsa at the time they applied, or had previously lived in Tulsa. Table 7 shows the results.

As the table shows, being previously involved with Tulsa, either through knowing people or previously living in Tulsa, does predict that the applicant is significantly more likely to move to Tulsa even without the program incentive. The moving probability is even higher for those offered the incentive, but the differential and "but for" goes down a lot.

[13] Why not also do this with an LPM? Because I wanted to make sure that any significant interaction effects were due to true effects, and were not, for example, due solely to the observation having a probability further away from zero.

[14] The statistics for these interactions were as follows: for the big sample, the approval/know someone interaction had a significance level of less than 0.0001, and the approval/previously lived in Tulsa interaction had a significance level of 0.0003. (That is, the probability of the approval/previously lived in Tulsa interaction effect of that absolute size had a probability of occurring by chance if the true effect was zero is 0.0003.) For the interview sample, the respective significance levels for these interactions were 0.0127 and 0.0640, respectively. The corresponding ratios of coefficients to standard errors were 6.18, 2.49, 3.60, and 1.85.

Table 7 Simulations of How Moving to Tulsa Varies with Program Approval and Tulsa Involvement, with "But For" Calculations

	Big sample				Interview sample		
	% of those approved (%)	Approved and moved (%)	Not approved and moved (%)	But for (%)	Approved and moved (%)	Not approved and moved (%)	But for (%)
Panel A: Whether Already Knew People in Tulsa When Applied							
Did not know	61.7	11.09	1.99	82.1	11.09	2.35	78.8
Did know	38.3	17.94	11.63	35.2	17.94	11.31	37.0
Overall average	100.0	13.71	5.68	58.6	13.71	5.78	57.9
Panel B: Whether Had Previously Lived in Tulsa							
Had not	89.7	12.45	3.97	68.1	12.45	4.16	66.6
Had lived	10.3	24.74	20.55	16.9	24.74	19.89	19.6
Overall average same as in Panel A							

NOTE: Of those who previously lived in Tulsa, 86.9 percent also said they knew someone who lived in Tulsa. Of those who knew someone who lived in Tulsa, 23.4 percent had previously lived in Tulsa.
SOURCE: Author's estimates.

For example, as one extreme, consider someone who had previously lived in Tulsa. Most of these people (87 percent) also knew someone in Tulsa. Offering this group an incentive to move to Tulsa yields an Infutor-detected move to Tulsa for around 25 percent of this group. But 20 percent of this group would have had an Infutor-detected move even without being approved by Tulsa Remote; the "but for" for this group is less than 20 percent.

At another extreme, suppose the Tulsa Remote applicant did not know someone in Tulsa at the time they applied. Most of this group also had not previously lived in Tulsa. (Specifically, 98 percent of this group had not previously lived in Tulsa.) If this group with less prior Tulsa involvement is offered an incentive, the probability of an Infutor-detected move to Tulsa is around 11 percent, less than half of what it is for those persons who had previously lived in Tulsa. But only a very low percentage of this group is estimated to have been likely to move to Tulsa without the incentive, at around 2 percent. Therefore, the inducement percentage or "but for" percentage is much higher for this group, at about 80 percent.

In addition, note that the overall "but for" for the entire treatment group is 58.6 percent (big sample) or 57.9 percent (interview sample). At 58 percent or 59 percent, the "but for" in this logit specification—with interactions between all control variables and the Tulsa Remote

approval zero-one variable—is somewhat less than the 67 percent or 70 percent found in the prior logit specification (Table 6). This prior specification was the same except for not including these interaction terms. Why this difference? The most plausible explanation is that this is driven by the interaction terms that are most statistically important. These are the indicators for whether an applicant knew someone in Tulsa or had previously lived in Tulsa. Including such interactions, which makes the specification more flexible, allows the Tulsa Remote approval to have lesser effects for a minority of the approved applicants. This turns out to somewhat reduce the overall effects of the Tulsa Remote program. Tulsa Remote approval is correlated with whether an applicant knew someone in Tulsa or had previously lived in Tulsa, and so excluding interactions of approval with these applicant control variables tends to bias the estimates upward a bit.

MATCHING MODEL OF EFFECTS OF TULSA REMOTE ON MOVING TO TULSA

So far, the estimates have controlled for observed control variables to some degree by including these control variables as predictors, or in some cases by interacting these control variables with whether someone was approved for Tulsa Remote. However, the estimates have not fully controlled for all possible interactions between the various control variables.

One way to control for all possible interactions is matching. In matching, each "treated observation"—in this case, someone approved by Tulsa Remote and therefore designated a finalist—is matched with a comparison group person that was not approved but who is identical on all control variables. That is, the person in the comparison group is in the same age range, same gender, same race, same income range, and so on, for all the control variables. This is an exact matching procedure, which is feasible since all the control variables are here defined as

discrete categorical variables.[15] With such matching, each treated observation is being compared with an observation that controls not only for a possible separate effect for each control variable, but also for any possible combined effect of the control variables, since the matched observation, by construction, has the same combination of control variables as the treated observation.

More specifically, the matched observations approach first restricts ourselves to the interview sample. This interview sample restriction is intended to further control for possible unobservables that lead to being selected for the interview group. A further restriction is to only consider approved or rejected observations (finalist or non-finalist observations in Tulsa administrative data, or alternatively could label as treated or comparison observations) that were already unique matches in the matching of Tulsa Remote administrative data with Infutor data. This avoids complications due to different weights on a treated observation and a matched comparison group observation—with this restriction, all observations have the same weight. As is usual with matching procedures, treatment group observations are dropped if they can't find a match. In the matching literature, this is referred to as being dropped because the treated observations and comparison group lack a common support for that range of X variables. Finally, we do the matching without replacement—that is, each comparison group observation is only included once. If we matched the same comparison group observation to more than one treatment group observation, this complicates the standard error calculations.

So, for example, if some combination of X variables has three treatment group observations and seven comparison group observations, we randomly match the three treatment

[15] With continuous variables, one could also do other matching, such as Mahlanbois matching and propensity score matching, which are various procedures for finding the person in the comparison group with the "nearest" values on the various X variables. In the current data, the only X variable that is potentially continuous is the age variable, and even that variable has the problem of having many missing observations. Therefore, it seemed preferable to do exact matching.

group observations with three of the comparison group observations. If some other combination of *X* variables has seven treatment group observations and three comparison group observations, three of the treatment group observations will be matched to the three comparison group observations, and the other four will not have a match. Finally, if there are three treatment group observations in a given cell representing some particular combination of *X* values, and no matching comparison group observations, those observation are also dropped—they lack a "common support."

An alternative way to control for all combinations of *X* variables is to include all possible combinations as predictor variables in an LPM, or a logit or probit model. However, this still is relying on extrapolation of the results for cases where the treatment group observations do not have an exact match—we are inferring effects of the treatment in part by including information even when there are cells of particular combinations of *X* variables in which either the treatment group or the comparison group lacks any matching observations on the other side. To avoid this extrapolation—and such avoidance is a more conservative approach—matching seems preferable as a way to test the robustness of the estimates of Tulsa Remote's effects.

Table 8 shows the results from this matching exercise.

As shown in Table 8, 1,395 interview sample persons who were approved by Tulsa Remote ("finalists") and were unique matches with the Infutor data were also able to be matched to a comparison group observation based on having exactly the same combination of all the control variables.

Table 8 Effects of Tulsa Remote on Moving to Tulsa, Restricted Matched Sample

Group	Sample size	Proportion moving to Tulsa in Infutor data	Standard error of estimated proportion moving (or difference in proportions for difference calculations)
Approved interview sample with unique Infutor matches and with comparison group exact match on control variables	1,395	0.16201	0.00987
Comparison interview group with unique Infutor matches and exact match to approved interview sample	1,395	0.05520	0.00612
Difference		0.10681	0.01141
But for (%)		65.9	

NOTE: As noted in text, for matching we only use observations in Infutor sample with unique Infutor matches, so no weighting is needed. We randomly find unique match in comparison group, and do matching without replacement. Standard error of difference in means is based on paired t-test. If we ignored pairing and simply calculated standard error of difference as square root of sum of variance of each group's estimates, standard error of difference increases from 0.01141 to 0.01161.
SOURCE: Author's estimates.

Comparing the treatment group sample with the matched comparison group sample, 16.2 percent of the treatment group sample was observed to move to Tulsa, versus 5.5 percent of the comparison group sample. The difference of 10.7 percentage points in the moving to Tulsa probability is highly statistically significant.[16] The implied "but for" is 65.9 percent—that is, about one-third of the treatment group sample would have moved to Tulsa even without being offered the chance to get the moving incentive, but the other two-thirds were induced to move to Tulsa due to the incentive offer.

The key lesson here is that the overall "but for" results are robust to including a complete set of interactions among the control variables, and to also then only including exact matches. With this more "conservative" estimation, the "but for" percentage stays within the same range as suggested by the prior estimates, which are more restrictive in imposing more assumptions on the data.

[16] The t-statistic is 9.36 (= 0.10681/0.01141), where a t-statistic whose absolute value is as high as this or higher has a probability of occurring by chance, when the true difference is zero, or less than 0.0001.

The matching approach also can be used to look at how the effects of Tulsa Remote on moving behavior vary for groups with different prior involvement with Tulsa. More specifically, the matched sample can be broken down to look at matches that vary in a) whether they already knew someone in Tulsa, or b) whether they previously lived in Tulsa. Table 9 shows these breakdowns.

Table 9 Effects of Tulsa Remote on Moving to Tulsa, Stratified by Prior Involvement with Tulsa, Restricted Matched Sample

Group	Sample size	Proportion moving to Tulsa in Infutor data	Standard error of estimated proportion moving (or difference in proportions for difference calculations)	Sample size	Proportion moving to Tulsa in Infutor data	Standard error of estimated proportion moving (or difference in proportions for difference calculations)
Panel A: Comparison of applicants those who did not know anyone in Tulsa with those who did						
	A1: Did not know anyone in Tulsa			A2: Did know someone in Tulsa		
Approved interview sample with unique Infutor matches and with comparison group exact match on control variables	930	0.14409	0.01152	465	0.19785	0.01849
Comparison interview group with unique Infutor matches and exact match to approved interview sample	930	0.02796	0.00541	465	0.11613	0.01487
Difference		0.11613	0.01234		0.08172	0.02328
But for (%)		80.6			41.3	
Panel B: Comparison of applicants who previously did not live in Tulsa versus those who did previously live in Tulsa						
	B1: Did not previously live in Tulsa			B2: Did previously live in Tulsa		
Approved interview sample with unique Infutor matches and with comparison group exact match on control variables	1,329	0.15199	0.00985	66	0.36364	0.05967
Comparison interview group with unique Infutor matches and exact match to approved interview sample	1,329	0.05342	0.00617	66	0.13636	0.04257
Difference		0.09857	0.01132		0.22727	0.07094
But for (%)		64.9			62.5	

NOTE: As noted in text, for matching we only use observations in Infutor sample with unique Infutor matches, so no weighting is needed. We randomly find unique match in comparison group, and do matching without replacement. Standard error of difference in means is based on paired t-test. Unpaired tests with equal sample variances can be calculated from standard errors of each sample, and are slightly larger than paired t-test standard errors.
SOURCE: Author's estimates.

For the groups differentiated by whether the applicant already knew someone in Tulsa, the results are roughly similar to the prior section's results. For those who did *not* know someone

in Tulsa, the estimated "but for," the percentage of those who were approved who would not have moved without the incentive, is 81 percent. This "but for" of 81 percent is almost twice as great as the "but for" of 41 percent for those who previously knew someone in Tulsa. These "but for" estimates are similar to the corresponding "but for" estimates for the interview sample, in Table 7, of 79 percent and 37 percent.

As with the prior analysis, the lower "but for" with the "knew someone in Tulsa" group is that a higher proportion of that group would have been likely to move to Tulsa without the Tulsa Remote program's intervention. This percentage is 12 percent, versus only 3 percent for those who did not know anyone in Tulsa. The applicants who knew someone in Tulsa were more likely to take the incentive offer and move to Tulsa, at 20 percent, versus 12 percent moving to Tulsa for the approved applicants who did not know anyone in Tulsa. But due to the high percentage who would have moved anyway, the "but for" or inducement percentage was far lower for the group that knew someone in Tulsa.

Panel B in Table 9 also looks at matched groups divided by whether the applicant had previously lived in Tulsa. In contrast to Table 7, these estimates do not find big differences in the "but for" based on whether the applicant had already lived in Tulsa. However, the sample size for the "previously lived in Tulsa" matched pairs is so low, at 66 pairs, that the estimates for these pairs are extremely imprecise.

Overall, these estimates are consistent with the other estimates of "but for." The overall "but for" for Tulsa Remote is likely to be somewhere in the range of 58–70 percent, depending on the sample and specification used. Among subgroups, the "but for" is lower for groups that had some prior involvement with Tulsa.

But what do these estimates mean for Tulsa Remote? Is a "but for" of 58–70 percent for Tulsa Remote sufficient for program benefits to exceed costs? And is it feasible and desirable for Tulsa Remote to do more to increase the "but for" for the program; for example, by approving fewer applicants who already know people in Tulsa or had previously lived in Tulsa? The next section begins a consideration of these issues.

OUTLINE OF A MODEL FOR ESTIMATING REMOTE'S BENEFIT-COST RATIO FOR TULSA RESIDENTS VARIES WITH THE "BUT FOR" PERCENTAGE AND OTHER "POLICY-AFFECTED" VARIABLES

To decide whether a Tulsa Remote "but for" of 58 percent yields benefits worth the program's costs, some estimates must be made of likely program benefits. For this report, these benefits are what I have defined in previous studies as "economic development benefits" (Bartik 1991, 2011, 2019a, 2022). These economic development benefits are gains in per capita income, which accrue to the original local residents, due to some intervention that seeks to boost local economic prosperity. In prior studies, I have applied this methodology to look at the benefits of business tax incentives to promote local economic development (Bartik 1991, 2019a, 2022), preschool programs (Bartik 2011, 2025), and college scholarship programs (Bartik et al. 2021). This section of the report outlines this methodology before turning in the following sections to Tulsa Remote benefit estimates.

Local "economic development" benefits—gains in the original resident's per capita incomes—are not the same as a policy's "local economic impact," at least as that "impact" is customarily calculated. Regional economic analysts frequently estimate or at least calculate the "impact" of some policy on state or local employment, aggregate total income or labor income, GDP/value-added, or tax revenue. For example, one can look at any number of studies of the

impact of the proposed Amazon HQ2 (Fuller and Chapman 2018), or the Foxconn project in Wisconsin (Ernst and Young 2017; Baker 2017), or various semiconductor projects such as the Micron Chips factory in New York State (REMI 2022), all of which focus on these aggregate economic impacts.

For studies of Tulsa Remote, those performed so far have focused on these "economic impacts." For example, studies have estimated that Tulsa Remote has generated annual labor income of $157,300 per Tulsa Remote member (Newman, O'Dell, and Fikri 2021) or $181,213 in additional annual labor income per Tulsa Remote member (Tulsa Remote 2024). These impacts are due to both the labor income of Tulsa Remote members, as well as other members of their household, and the "multiplier effects" of the spending of Tulsa Remote households on jobs and labor income in various retail sectors of the Tulsa economy. This added labor income is then calculated to yield, per Tulsa Remote member, perhaps $4,000 in added state and local tax revenue (Newman, O'Dell, and Fikri 2021).

There is nothing wrong with such economic impact estimates—they provide useful information. But they have limitations. Perhaps the key limitation is that gains in a local area's labor income (or overall local GDP, or overall local personal income) do not necessarily correspond to gains in per capita income for the local area's original residents. At the national level, if a policy boosts overall labor income or personal income or GDP, for most policies it is safe to assume that this also boosts per capita values of these same variables. This per capita income boosts leads to higher incomes on average for the original U.S. population. Most national policies do not have strong effects on population. (Obviously immigration policies would be one exception, as might be policies that affect childbearing or infant mortality.)

In contrast, at the state or local level, virtually every local policy will have some effects on population in-migration or out-migration. If a policy boosts local labor income by some percent, but boosts local population by the same percent, then per capita income is unchanged. The original local residents would be no better off even though local labor income has increased.

In the case of Tulsa Remote members, it is obviously true that the policy by its very nature will boost area population as well as employment by attracting remote workers. But the multiplier jobs resulting from Tulsa Remote will also boost local population. The research literature on local labor markets shows clearly that even when local employment is boosted without being tied to in-migration—which is the more usual case of typical economic development programs—in the long run, usually at least 80 percent or more of the new local jobs will end up increasing local population, and only 20 percent will boost local employment rates and hence per capita labor income.

This 80 percent population effect of new jobs in a local economy may seem hard to believe. Surely new firms do not typically hire 80 percent in-migrants, with some exceptions such as headquarters relocations. No, most new firms do not directly hire 80 percent in-migrants. Rather, new jobs are filled by some combination of 1) hiring local residents who are employed, 2) hiring local residents who are not employed, and 3) hiring in-migrants. But any jobs filled by hiring already-employed local residents results in job vacancies. These job vacancies are filled in the same three ways. Thus, new jobs lead to job vacancy chains where various local employers make choices about who to hire. These vacancy chains are only terminated when jobs are filled by the local non-employed or in-migrants. At the end of these job vacancy chains, all new jobs ultimately result in some combination of jobs going to the local non-employed versus in-

migrants.[17] The relative proportion depends on whether employers do more or less hiring of the local non-employed versus in-migrants, which depends in part on the local "non-employment" rate—how many non-employed are readily available and employable. On average, these job vacancy chains result in only 20 percent of new local jobs—even when not explicitly tied to an in-migrant worker—resulting in a boost to local employment rates.

But why focus on per capita income effects rather than effects on aggregate local income? Because ultimately, a true social benefit-cost analysis should care about the well-being of individuals. For "economic well-being," the best available measure of that well-being is the real standard of living for local residents, which is per capita income adjusted for local prices, and ideally also adjusted for various factors that measure the local quality of life. The fact that an area has gained a lot of income in aggregate is relevant only to the extent that by doing so it has increased local residents' standard of living compared to what it otherwise would be. It is these measures of how average well-being is affected by Tulsa Remote that should be compared with the program's costs. Whether or not the aggregate labor income generated by Tulsa Remote exceeds the program's costs does not directly tell us whether the program has net social benefits. These social benefits depend on whether this aggregate labor income is translated into gains in local real income per capita. In a true social benefit-cost analysis, what is relevant is whether the increase in per capita real incomes of Tulsa residents due to the program exceed program costs.

The other, more minor limitation of current Tulsa Remote studies is that they do not allow local wages and prices to change. Local economic impact studies that use input-output

[17] This can also be seen by the mathematical fact that an area's employment is the product of its employment to population ratio, multiplied by the area's population. As a result, a percent boost to area employment must be approximately equal to the sum of the percent boost to the area's employment/population ratio, plus the percent boost to the area's population. This approximation is exact if percentages are defined as the change in the natural logarithm of each variable.

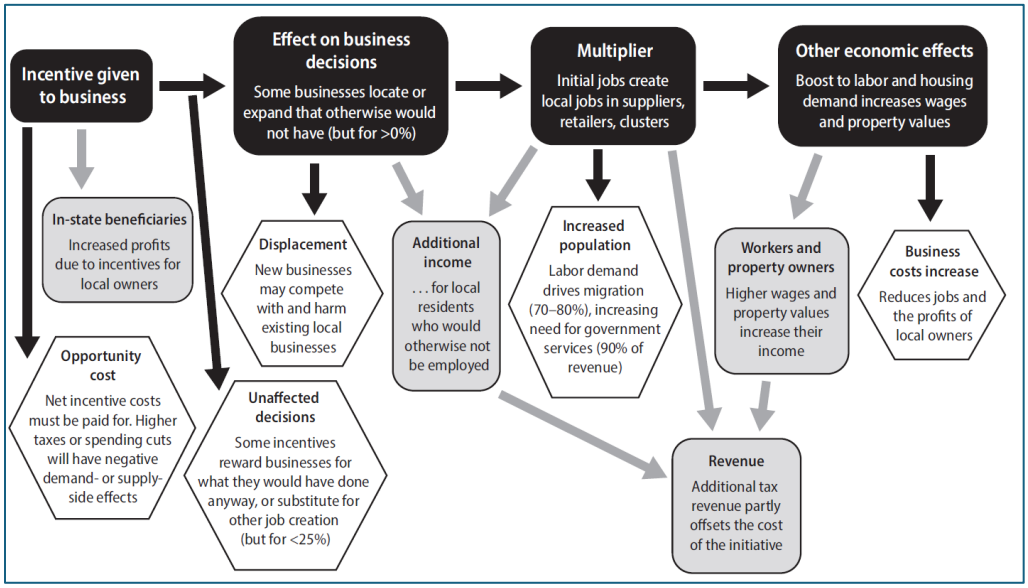
models such as IMPLAN trace how local income of Tulsa Remote members is spent on various locally produced goods and services, assuming local costs do not change. But when we boost jobs and population in a local economy, local costs in general will go up. Local housing prices and land prices will go up. This in turn tends to lead to increases in local nominal wages, at the very least, and perhaps real wages as well. The increase in local wages, along with higher real estate prices, will boost local costs. These cost increases, on the one hand, will tend to somewhat reduce local employment by discouraging employment growth. Studies suggest that such effects might reduce the local multiplier, from that produced by an input-output model, by 20–30 percent.[18] In addition, the increases in real estate prices are a type of "income gain," a capital gain, for any local resident who owns a house or business real estate. But these higher local prices also may reduce the real value of various non-labor incomes that do not vary with local prices. For example, Social Security checks or interest and dividend income may vary with national prices but not with differential changes in local prices. All of these effects should be accounted for.

To estimate the "per capita income effects" of Tulsa Remote, this report uses a model I have developed that has been used previously to evaluate the benefits and costs of business tax incentives (Bartik 2019b; Bartik 2023a; Kansas Legislative Division of Post-Audit 2023). This model is essentially a model of a regional economy, but optimized to consider the impacts of incentive policies to boost employment. The model simulates the effects of such policies on various types of local income per capita.

[18] Thus, if the estimated input-output multiplier is 1.9, the effective multiplier might be 1.5, which is reduced by 0.4. 1.5 divided by 1.9 is 0.789, so the multiplier reduction is about 21 percent (Bartik and Sotherland 2019). Running the benefit-cost model described in Bartik (2023b) frequently yields multiplier reductions, compared to the input-output multiplier, of 30 percent.

The model is more fully summarized in Bartik (2019a, 2023b). Figure 1, reproduced from Bartik (2019a), illustrates the model's interactions. As originally conceived, the model imagines that various incentive deals encourage some firms to locate or expand jobs in a particular local economy. These incentives deals have some probability of "tipping" these location decisions—

Figure 1. A Model of the Local Income Effects of Economic Development Programs



SOURCE: Author's original design describing his model.

there is some "but for" percentage, or probability that the jobs would not have been added to the local economy "but for" the incentive. The estimated number of jobs created—the incented jobs times the "but for" probability—then has some expected multiplier effect on the local economy.

This boost to jobs will have important labor market effects: unemployment rates will go down and labor force participation rates will go up, with those effects diminishing over time as more in-migration occurs. The jobs boost will also put upward pressure on local real wages. The population increases that occur will increase local housing prices and other local real estate prices. Higher real estate prices then lead to a higher overall price level.

The boost to local prices, together with the upward pressure on real wages, will boost local costs of business. These local cost increases will tend to reduce job growth, which will partially offset the initial multiplier effects.

Local real incomes will go up due to increases in local employment rates and local real wages, as well as the capital gains from higher local real estate prices. But some local real incomes will go down because most non-labor income is indexed to national economic conditions, not local economic conditions.

The resulting changes in property values and local incomes will tend to boost state and local tax revenue. But the increase in local population will boost needs for added state and local public spending to keep public service quality constant—for example, as population goes up, school teachers must be hired to keep class sizes the same. There often may still be a "fiscal benefit" equal to increased state and local tax revenue minus public spending increases to keep public services quality constant. But the fiscal benefit is much less than the revenue gain.

Combining these fiscal benefits with the initial costs of the incentive program results in some net budget deficits or surpluses. If there are net budget deficits, this will lead to some combination of tax increases or public spending cuts to restore state and local budget balances. If there are net budget surpluses, there will be some combination of tax reductions and public spending increases. Such budget changes have some direct effects on real incomes per capita of local residents. In addition, these budgetary changes to taxes and public spending in turn have demand-side effects on local jobs. Higher (lower) taxes or lower (higher) public spending will reduce (increase) local demand and hence reduce (increase) local jobs.

In the model, fiscal changes also lead to two types of "supply-side" effects. Type 1: boosts (reductions) to business taxes will reduce (increase) local jobs. Type 2: Decreases

(increases) in local K–12 spending will eventually reduce (increase) future earnings of former K–12 students who remain in the local economy.[19]

All of this is simulated over an 80-year period to ensure we capture long-run effects. In particular, changes in public school spending may affect earnings 30 or 40 years later, when former students are in their prime earnings years. The simulations use empirical parameters estimated in the research literature. For example, the simulations use research-based estimates on how much job growth and population growth affect various local prices and local employment rates, as well as empirical parameters for how sensitive businesses are to business taxes and other local costs, and how much public-school spending affects future earnings.

Applying this economic development benefits model to Tulsa Remote requires a few tweaks. Table 10 outlines the assumptions made to adapt the model.

The model "incented jobs" are those of members and others in their household.[20] The "but for" is derived from earlier in this report.

The retention rate is based on data and assumptions from Tulsa Remote and from Newman, O'Dell, and Fikri (2021). I based my retention numbers in part on conservative projections from Tulsa Remote information on retention as of two years after the move to Tulsa, and based on survey evidence reported in Newman, O'Dell, and Fikri (2021) on whether Tulsa

[19] Does this mean that these two types of "supply-side" effects are the only possible effects? No. For example, some economists would believe that there are supply-side effects of individual income taxes on some households' labor supply and location decisions. In addition, spending other than K–12 spending may have supply-side effects on earnings. This would include other education spending, such as higher education and preschool. It might also include child care spending and public health spending. But the general point is simply that taxes and public spending decisions might have effects on the supply of various productive inputs. Finally, for Tulsa Remote, as will be discussed below, we include supply-side effects from adding college graduates.

[20] Note that the model assumes that the additional household members who are employed add employment as well as population to the Tulsa economy. This is deliberate. It seems like the most reasonable position for two reasons. First, some of these additional household members may also work remotely. Second, research by Howard (2020) as well as other research reviewed by Bartik (2019b) suggests that population shocks result in employment shocks of a similar or greater percentage. See Appendix D for further discussion.

Table 10 Added Assumptions Made in Analyzing Economic Development Benefits of Tulsa Remote

Type of assumption	Assumption	Source/rationale
"But for"	57.9% baseline; also consider 16.9% and 69.7%	Estimates in this report. 57.9% is lowest estimate of overall "but for" from credible model.
Retention of Tulsa Remote members	Baseline is 55% retention at year 5, 33% at year 10, stable thereafter. Also consider high and low retention scenarios.	Scenarios derived based on Newman, O'Dell, and Fikri (2021), and information from Tulsa Remote*
Ratio of Tulsa Remote household workers to members	1.406	Newman, O'Dell and Fikri (NOF) 2021*
Multiplier for member household jobs	1.41	NOF (2021) and Tulsa Remote (TR 2024)*
Labor force participation rate & unemployment effects	0 for member household jobs, Bartik (2023b) for extra multiplier jobs	Member households fill jobs with increased population; multiplier jobs have mix of population versus employment rate effects
Real wage effects	0 for member household jobs, Bartik (2023b) for extra multiplier jobs	Member household jobs increase labor supply as much as demand
Wage premium on Tulsa Remote direct jobs for member households.	0%, but also consider 10% wage premium. Not counted as benefit as in Bartik (2023b), only affects personal income increase that affects state and local revenue effects	Source is Tulsa Remote, which provides ambiguous evidence on whether TR direct jobs have wage premium*
Cost of Tulsa Remote	$15,425 ($10,000 incentive plus various recruiting, activity, and support costs)	Information from Tulsa Remote on costs per adjusted member year..*
Demand effects of Tulsa Remote costs	Demand effects equal to 68.3% of model's public spending effects	In usual business tax incentive model, incentives go to outside-of-state owners, so no local demand effects. But Tulsa Remote incentive package is either local income transfer ($10,000 incentive to members who stay for one year) or spending (the remaining cost to get to $15,425 costs per adjusted member). Latter is assumed to have usual public spending effects on jobs, former is assumed to have half as large an effect, end up with weighted average of 68.3%.*
Supply-side effects of Tulsa Remote	Each college grad induced to move to Tulsa by Tulsa Remote is assumed to create 0.57 jobs in Tulsa in long-run (baseline assumption). 80% of Tulsa Remote member household workers assumed to be college grads. Jobs adjust to LR effect at 9% per year.	Bartik (2018b) for 0.57 effect; college grad percentage of members in this report's database is about 80%. LR adjustment consistent with research lit described in Bartik (2018a)*

NOTE: Sources with asterisks have more discussion in Appendix D.
SOURCE: Author's assumptions, adopted from prior empirical estimates as cited in the table and in Appendix D.

Remote members still expected to be around in five years: see Appendix D for details. I then continued these trends from year 2 to 5 years later and assumed these trends further continued until year 10. The result was a retention rate of 55 percent as of year 5, and 33 percent as of year 10. After year 10, I assume the "effective retention rate" stayed the same. Why this assumption? First, available data suggest relatively little net out-migration from states and metro areas at the likely ages after year 10, when the median Tulsa Remote member would be 45 (Bartik 2009). Second, some Tulsa Remoters who leave will return at a later age. Third, Tulsa Remoters over time will attract other college graduates to the area—including remote workers and their children, who probably will be college graduates and may stay in the area. For these reasons, it seems a reasonably conservative assumption that in the long term, the overall shock to employed college graduates stays at 33 percent of the initial shock.

The multiplier effect entered into the model is derived from prior Tulsa Remote studies.[21] This multiplier effect is the input-output jobs multiplier without considering cost feedbacks. The structure of the model means that feedbacks from higher costs will lower the effective jobs multiplier below the input-output multiplier. This will be illustrated later on.

Because the structure of the program means that the direct jobs have a one-for-one effect on population, I assumed that the "incented jobs," the "direct jobs," those held by Tulsa Remote members and others in their household, had zero effects on local labor force participation rates and unemployment rates—100 percent of these direct jobs were filled by in-migrants, by definition. These direct jobs were also assumed to have zero effect on local real wages, as both

[21] See Newman, O'Dell and Fikri (2021) and Tulsa Remote (2024). The multiplier here of 1.41 is lower than the 1.50 in the former and the 1.58 in the latter in that those prior studies reported multiplier for direct jobs held by Tulsa Remote members, whereas this report is using the multiplier averaged over both member jobs and jobs held by other household members. This report's multiplier should be lower as other household members have lower labor incomes and are not necessarily remote workers. See Appendix D for more details on where the 1.41 comes from.

labor demand and labor supply are equally shocked. The indirect jobs—the jobs created because the multiplier is greater than 1—are allowed to have the "normal effects" on labor force participation rates, unemployment rates, and real wage rates that were already in the model, and which are based on prior empirical research.

In the baseline model, the direct jobs held by Tulsa Remote member households pay no wage premium relative to typical Tulsa jobs that would be created in export-base industries. As explained in Appendix D, there is some possibility of some wage premium, but it is uncertain, so the baseline model does not allow for any wage premium. However, variants of the model do consider up to a 10 percent wage premium. As discussed in Appendix D, some wage premium in Tulsa Remote member household jobs is possible. In addition, remote worker attraction programs such as Tulsa Remote can potentially increase the wage premium by altering procedures for selecting who receives a remote worker incentive offer. Therefore, analyzing the impact of such wage premia is desirable. The wage premia alters the benefit-cost analysis by affecting fiscal benefits. A higher wage premium increases state and local personal income, thereby boosting state and local tax revenue and thus providing fiscal benefits. These fiscal benefits in turn provide some labor market benefits by boosting demand.[22]

The cost of Tulsa Remote clearly exceeds the costs of the $10,000 incentive. Beyond administrative costs of soliciting and evaluating applications, and distributing incentive payments, Tulsa Remote provides access to co-working spaces, a variety of networking activities (5–10 events per month), and some opportunities for participation in some entrepreneurship development activities. The assumed total costs, based on figures from Tulsa Remote, are

[22] The prior version of the model, for business tax incentives, also allowed the wage premium to provide benefits for the original local residents who get the incented jobs. However, in this model, the direct jobs created go 100 percent to in-migrants, so these direct labor market benefits do not exist in the Tulsa Remote version of the model.

$15,425 per member.[23] These additional costs are integral to the Tulsa Remote strategy: the Tulsa Remote strategy goes beyond the goal of attracting Tulsa Remote members, but also seeks to engage members in activities that promote retention and benefit the Tulsa community.

The assumed total costs of $15,425 per member enter the model in several ways. These costs are assumed to be ultimately paid for by some combination of higher state and local taxes and lower public spending. The program currently receives some state funds for Tulsa Remote members who remain in Tulsa, simply from the state income taxes they pay. In addition, even though the Tulsa Remote program is financed by donations from the George Kaiser Family Foundation, these donations have an opportunity cost. These funds could have instead gone to other public services, such as the Educare child care and preschool centers that also have been supported by the Foundation, or the funds could have gone to economic development programs that aid businesses, which would have lowered business taxes. And sometimes local charitable families even directly support lower taxes, as has happened in the city of Kalamazoo, Michigan, for example, with the Foundation for Excellence, which has contributed funds to the city to lower its property tax rate to be more competitive with local suburban tax rates.[24]

One could instead assume that the donations from the George Kaiser Family Foundation would instead not have been devoted to Tulsa programs. Under this assumption, the Tulsa Remote program is to some extent a free lunch for almost all Tulsa residents. However, it seems more conservative to instead assume that the program's opportunity costs are 100 percent financed by lower Tulsa area public spending and higher Tulsa area taxes.

[23] As explained in Appendix D, these are costs per "adjusted member"—that is, adjusted for the number of months first-year members remain in the program. Because 95.5 percent of all Tulsa Remote members remain in the program for at least one year, these costs per adjusted member are quite similar to the costs per original member.

[24] See https://www.kalamazooffe.org/Home#main-content

This report adopts the regular baseline used in this economic development benefit-cost model: 50 percent of the funds come from increased taxes, and 50 percent from reduced public spending. The regular baseline used in this model also distinguishes between business taxes and personal taxes and K–12 education spending versus other public spending, and assumes that the tax increases and spending cuts are distributed between these tax types and spending types according to baseline shares in the particular state, in this case Oklahoma.

Based on these financing assumptions, these various types of funding for Tulsa Remote first enter the model directly as reducing the after-tax real incomes of Tulsa residents, either by hiking taxes or reducing public services or transfers. Second, these higher taxes or lower public spending have demand-side effects on reducing local jobs, either by directly reducing publicly funded jobs or by reducing local residents' spending on locally produced goods and services. The resulting job reductions then indirectly reduce local residents' real incomes by reducing local employment rates, real wage rates, and property values. Third, higher business taxes are assumed to have supply-side effect in reducing job growth, with the elasticities based on prior research. Fourth, lower K–12 education spending is assumed to reduce future earnings of local Tulsa residents who are in the K–12 system when the spending cutback occurs, and who stay in Tulsa over their career. (The lower earnings of out-migrants from Tulsa are not considered in the model—only the income changes of persons remaining in Tulsa are counted.)

In addition, these Tulsa Remote costs lead to various types of quasi-public spending on the Tulsa Remote program. There is the $10,000 that is eventually transferred to Tulsa Remote members who stay for a year. In addition, the remaining $5,425 in spending for Tulsa Remote also directly creates jobs and has some multiplier effects.[25]

[25] This is a modification to the original tax incentive model. In that model, incentives either went to some out of state firm, and left the state and were ignored; or, went to some in-state firm, and thus were counted as income

As mentioned earlier, the model also considers fiscal benefits, for which all these effects go in reverse: the fiscal benefits allow for cuts in taxes and increases in public spending, which both directly boost real incomes and have a variety of positive demand-side and supply-side effects that indirectly boost local real incomes. To anticipate the results reported later in the baseline simulation, fiscal benefits exceed the Tulsa Remote costs, so on net, the "government budget effects" of Tulsa Remote end up increasing the program's benefits.

A final, potentially important effect of Tulsa Remote—and one of the rationales for the program—is potential external economic benefits from having more local college graduates. College graduates are thought to have spillover benefits for other workers in two ways. First, college graduates may be more likely to start new businesses, and if these businesses export their goods and services outside the local economy, these start-ups will boost local job growth and thus boost local income per capita.[26] Second, college graduates who are part of the local workforce may enhance the overall productivity of their workplace. In other words, worker A's productivity may be enhanced if their co-worker B is more productive. As one example, an employer's ability to successfully introduce new technology depends on the skills of all of the employer's workers, not just a few. If college graduates boost overall local productivity, local firms can to be more competitive and gain market share, which will tend to boost their output and job growth. This will have multiplier effects on the local economy and positive effects on local employment rates and real wages.

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gains for the firm's local owners. But for Tulsa Remote, the $5,425 in spending is public spending, not an income transfer, and the $10,000 income transfer to Tulsa Remote members is not counted, as they are not original Tulsa residents.

[26] If the start-ups are in "non-export-base businesses"—businesses that sell to the local market—the economic impact is more complicated. Increased sales and jobs at any such "non-export-base" startups will reduce sales and jobs of competing Tulsa area businesses. But presumably the new start-ups out-competed these existing businesses by being more "productive" in some sense, that is offering some combination of a higher quality good or service at a lower price. This still enhances the local economy, but in a different way that is harder to incorporate in a benefit-cost model without more information on the magnitude of these productivity gains.

The idea of education spillovers is a fundamental idea in urban and regional economies. Influential books have promoted this idea, such as Glaeser (2012), Moretti (2012), and Florida (2002). Furthermore, there is some empirical support for the notion that increases in a local economy's college graduates promote job growth (Glaeser and Saiz 2004; Bartik 2018b) and higher wages (Moretti 2004).

At least implicitly, this idea of education spillovers has also been mentioned in reports discussing the benefits of Tulsa Remote. For example, Tulsa Remote's own 2024 report touted the program's contribution to "laying the foundation for a strong tech and innovation economy," and notes that "more than 18 percent of program alumni . . . have embarked on entrepreneurial ventures." The 2021 report on Tulsa Remote by the Economic Innovation Group (EIG) asks whether Tulsa Remote can "go on to form the base of a new local knowledge economy," and mentions that "36.7 percent [of Tulsa Remoters] have at least thought about starting a business in the near future . . ." (Newman, O'Dell, and Fikri 2021). EIG does note that although "Tulsa Remote theoretically helps bypass the chicken and egg problem of developing a bench of skilled knowledge economy workers capable of spurring spillover economic development in the future, the degree to which it pans out in practice remains to be seen." The recent academic paper by Teodorovicz, Choudhury, and Starr (2024) cites examples of impacts such as a Tulsa Remote member starting a "bakery business," and another member "start[ing] the scaling process of her technology firm." Richard Florida's recent report puts Tulsa Remote in the context of a wide range of activities supported by the George Kaiser Family Foundation, including "an incubator and co-working space, 36 Degrees North, . . . Tulsa Innovation Labs . . . [and a] venture capital firm, Atento Capital" (Florida 2024). These other services can all be seen as potentially having

some synergistic effect with Tulsa Remote to increase the odds that the attracted college graduates will either start up businesses or boost the local high-tech workforce.

However, although all these studies point to the potential for Tulsa Remote, possibly accompanied by other Tulsa programs, to provide spillover benefits from growing and attracting more Tulsa-area high-tech jobs, it is fair to say that so far none of these studies have sought to quantify this potential impact. In an attempt to do so, the current report uses empirical evidence from prior research that estimates that, on average, each college graduate will cause a "supply-side effect" on a local economy of growing the local economy by 0.57 jobs in the long run. This estimate is consistent with estimates in Bartik (2018b) and Glaeser and Saiz (2004). These long-run "supply-side effects"—due to both increased entrepreneurship and higher workplace productivity—should be considered when evaluating the potential benefits of Tulsa Remote and comparing them with known short-run costs. These supply-side job growth effects will increase the income per capita of local Tulsa residents. This report considers both these baseline supply-side effects and also how the benefits of Tulsa Remote would be altered if such supply-side effects did not occur—or, alternatively, if the activities of Tulsa Remote and other Tulsa groups are able to multiply these baseline supply-side effects by up to 3 times. To what degree are such supply-side effects potentially important when evaluating Tulsa Remote?

So, what does this all mean for the benefits of Tulsa Remote? When this economic development benefit-cost model is applied to Tulsa Remote, along with this report's estimated "but for" rates, what are the results? The next section turns to these simulation results.

SIMULATION ESTIMATES OF THE BENEFITS AND COSTS OF TULSA REMOTE FOR THE PER CAPITA INCOMES OF TULSA RESIDENTS

This section of the report uses the "but for" estimates from earlier in the report, and this (adapted) benefit-cost model of economic development benefits, to simulate the likely benefits and costs of Tulsa Remote for the original Tulsa residents, excluding Tulsa Remoters and other in-migrants.

Before presenting these simulation results, I'll provide some context from prior analyses of the benefits and costs of business tax incentives. These business tax incentives on average seem to have a benefit-cost ratio in the range of 1 to 2 (Bartik 2019a). That is, typically the benefits from the increase in the present value of per capita income for the original residents, compared to incentives costs, have a ratio of 1 or 2 to the present value of the incentive costs. At a ratio of 1, the program would be just worth its costs, which local residents also pay to yield net benefits of zero overall. At ratios that are modestly greater than 1, the program passes a benefit-cost test in the sense that it is estimated to have positive net benefits, but a policymaker should wonder whether there is sufficient uncertainty in the model's underlying estimates that there is a significant probability that the program's gross benefits are less than costs.

Business tax incentives can have benefit-cost ratios of less than 1 if they are particularly poorly designed or are in booming places with high employment rates. For example, long-delayed tax incentives are less cost-effective in inducing business location decisions, which lowers the benefit-cost ratios. If a local labor market is already very close to full employment, with very high employment rates, then almost all new jobs will go to in-migrants, which lowers the employment rate effects of the program and hence lowers overall benefits. If the incentives are awarded to non-export-base firms that sell to a local market, any job creation in the incented

firm will mostly be offset by job losses in other local firms, which will drastically reduce overall job creation and hence the incentive benefits.

On the other hand, business incentives can have benefit-cost ratios as great as 3-to-1 or more under more favorable circumstances. Some tax incentives are more cost-effective—for example, if they are more up front. Some "non-tax" incentives—for example, customized services such as customized job training—may be more cost effective per dollar in inducing job creation than handing out cash. Some export-base job creation may have greater job multipliers, with a net multiplier as high as 3, even after adjusting for increases in local costs. Employment rate effects, and hence per capita income effects of incentives, are greater if they are targeted at local labor market that have low employment rates, which means more local residents are likely to be hired for newly created jobs.

For business tax incentives, gross benefits are mostly labor market benefits. Typically, more than two-thirds of the benefits of these programs come from higher local employment rates or higher real wages. Increased income from capital gains on property value appreciation are usually less than a third of the labor market benefits. Fiscal benefits are also important but modest, typically being less than one-third of the labor market benefits. Fiscal benefits rarely are large enough to pay for the tax incentives. Depending on the scenario, fiscal benefits typically might pay for 10–30 percent of the incentive costs. In highly favorable circumstance—a very high multiplier or a highly distressed local labor market—fiscal benefits might offset incentive costs by 50 percent. Business tax incentives typically do not make money for state and local governments. Claims to the contrary typically assume that none of the incented business activity would have located in the state "but for" the incentive program, and focus on increased tax revenue while ignoring the public spending costs required by an increased population.

Finally, for business tax incentives, net benefits are modestly progressively distributed. By that I mean that although net benefits tend to go up with household income, lower income groups get a higher share of net benefits than their baseline income shares. For example, in one simulation, the lowest income quintile gets 16 percent of an incentive program's net benefits, which is triple its baseline income share of 5 percent (Bartik 2019a, p. 43). However, the lowest income quintile gets fewer dollars than the average household, in that they receive less than 20 percent of the net benefits. Therefore, well-designed business tax incentives do tend to make the income distribution somewhat more equal, but not as much as some other programs. For example, increasing K–12 spending appears to provide benefits to the lowest income quintile that exceed one-fifth of their total benefits, as the lowest income quintile has more children in public schools, and it appears that the most disadvantaged children benefit somewhat more than average from improved school quality. And obviously, many welfare programs might distribute over 80 percent of their benefits to the lowest income quintile.

As will be seen, the results for Tulsa Remote are quite different from typical economic development programs. Table 11 presents the overall benefit-cost picture for Tulsa Remote.

Table 11 Overall Benefit-Cost Ratios and Gross Benefits per Tulsa Remote Member, with Baseline Assumptions

	Benefit/cost ratio	Gross benefits per member ($)	Fiscal benefits per member ($)	State/local revenue gains per member ($)	Increased spending needs per member ($)
Over 80 years	4.31	66,462	32,252	472,599	(440,347)
Over 10 years	3.93	60,547	12,969	143,079	(130,110)

NOTE: Benefit cost ratios are ratios of present values at 3 percent real social discount rate. Dollar figures are present value per member, adjusted to current price levels. Parentheses used to indicate negative values. Estimates use baseline assumptions: "but for" of 57.9 percent, multiplier of 1.41, $15,425 Tulsa Remote cost per member, 1.406 ratio of workers in member household to members, middle assumptions on retention, 80 percent of member household workers with BA plus, 0.57 jobs created in LR per college grad (adjustment to long-run at 9 percent per year), wage premia of zero percent on member jobs, Tulsa Remote spending has 68.3 percent of effect of regular public spending.
SOURCE: Author's calculations using his estimates and model.

As shown in the table, Tulsa Remote has a high benefit-cost ratio of 4.31. In other words, gross benefits are 4.31 times costs; after subtracting costs, net benefits are 3.31 times costs. Viewed as an economic development program, Tulsa Remote has a higher benefit-cost ratio than the typical business tax incentive program. Most of these benefits occur in the first 10 years for reasons we will explore more below. These benefit-cost ratios can also be stated as gross benefits per member: 4.31 = $66,462 divided by the program costs of $15,425 per member. Benefits exceed costs by $51,037 per member (= 66,462 − 15,425).

Over an 80-year time horizon, the fiscal benefits of Tulsa Remote are over twice the program's costs: a present value, discounted at a 3 percent real rate per year, of $32,252 per member. This remaining $16,827 (= $32,252 − $15,425) is a budget surplus that can be used for a variety of purposes to benefit Tulsa residents, such as increased education spending and lower tax rates. Even if we restrict ourselves to a 10-year time horizon, the program's net fiscal benefits, at $12,969 per member, cover over four-fifths of program costs ($12,969/$15,425 = 84 percent). This is a far better fiscal return than almost all business tax incentive programs.

This fiscal benefit occurs even though Tulsa Remote does have very large effects, due to increased population, in required added public spending to maintain public service quality. This required public spending offsets over 90 percent of the extraordinarily large present value of state and local revenue gained per member, over either 80 years or 10 years.

Diving more deeply into the results, Table 12 breaks down the income benefits by type of income, and by income quintile. As can be seen, the largest category of benefits is fiscal benefits, followed closely by labor market benefits and property value benefits. All these categories of benefits are large: any one of these categories of benefits exceeds costs of $15,425 per member.

Table 12 Gross Benefits and Costs per Member, by Income Type and Income Quintile

		Income quintile					
		Total	1 (low)	2	3 (middle)	4	5 (high)
	Quintile base income share (%)	100.0	5.1	9.2	13.7	20.0	52.0
Income type	Total net	51,037	5,914	7,085	9,310	6,686	22,041
	Group net as % of total net	100.0	11.6	13.9	18.2	13.1	43.2
	Total gross	66,462	7,645	9,083	11,717	9,616	28,400
	Fiscal benefits	32,252	3,620	4,177	5,034	6,125	13,296
	Labor market benefits	27,700	3,131	4,661	7,200	3,758	8,950
	Property value benefits	26,426	845	1,305	1,764	3,172	19,341
	Real value of non-labor income	(19,385)	(1,006)	(1,981)	(2,908)	(3,764)	(9,726)
	Education effects	6,400	1,480	1,403	1,239	1,154	1,123
	Exported business taxes	(3,046)	(342)	(395)	(475)	(579)	(1,256)
	Local business effects	(3,885)	(82)	(88)	(136)	(251)	(3,328)
	Incentive costs	(15,425)	(1,731)	(1,998)	(2,408)	(2,930)	(6,359)
	Benefit-cost ratio	4.31	4.42	4.55	4.87	3.28	4.47

NOTE: Same baseline simulation as in Table 10. Quintile base income share's derived from CBO data; see Bartik (2022). Most figures are in dollars per Tulsa Remote member, except for figures for quintile income shares and group net as % of total net, which are in percentage terms; and benefit-cost ratio, which is ratio of gross benefits to costs for each group. Parentheses used to indicate negative values. For more details on model, see Bartik (2022).
SOURCE: Author's estimates.

However, compared to a typical business tax incentive, Tulsa Remote tends to have relatively large fiscal benefits and property value benefits, relative to labor market benefits. For business tax incentives, labor market benefits tend to exceed fiscal benefits and property value benefits combined.[27]

Why this pattern of high labor market benefits compared to costs but relatively more fiscal benefits and property value benefits compared to labor market benefits? On the first point, labor market benefits, Tulsa Remote actually fares quite well relative to business tax incentives

[27] More minor roles are played by other types of income effects: losses due to declining real value of non-labor income, losses of local business profits due to higher costs, some reduction in the exporting of business taxes to non-local owners, and some gains of higher earnings due to higher K–12 education spending. The declining real value of non-labor income is due to the higher local prices brought about by higher housing prices. Losses of local business profits are due to higher costs for locally-owned businesses that are export-base and cannot pass on their cost increases, whereas non-export-base businesses in a competitive market are assumed to be able to fully pass on their cost increases with higher local prices for their output. Some proportion of state and local business tax revenue is exported to out-of-state business owners, who are not part of the model; with fiscal benefits greater than incentive costs, this budget surplus will lead to tax reductions, part of which reduce this tax exporting. This budget surplus also leads to higher education spending, which boosts future earnings of children in school at the time of the education spending increases. See Bartik (2018a) for more discussion of these income types.

in cost per job created, even if we just focus on the jobs that increase the local employment rate. Consider the following analysis: the program costs $15,425 per member. We have estimated an inducement rate of at least 57.9 percent, so the cost per induced member household is $15,425 divided by 0.579, or $26,641. But of course, the jobs in member households are not the local jobs that increase the employment rate and real wages—those jobs are the multiplier jobs. The assumed multiplier on the direct jobs in member households is 1.41, and the number of jobs in member households is assumed to average 1.406 per household. Multiplying 0.41 times 1.406, the "indirect jobs" per member household are 0.576. Dividing $26,641 by 0.576 gives a cost per indirect job created via multiplier effects of a little less than $50,0000 (more precisely, $46,252). But after five years, the model assumes only 55 percent of member households remain, so the cost per multiplier job would almost double to a little over $80,000 (more precisely, $84,095). After 10 years, the percentage of member households still remaining in Tulsa declines to 33 percent, and the cost per multiplier job would go up again by two-thirds, to around $140,000 per indirect job created ($46,252/0.33 = $140,158).[28]

A cost of $140,000 per indirect job created seems extraordinarily high. But, first of all, job creation is very valuable. Consider that if jobs on average increase the employment rate by 20 percent, and jobs on average pay $60,000, then the expected increase in earnings per capita simply from the employment rate increase would be $12,000 per year. If these employment rate benefits persisted forever, at a 3 percent discount rate they would have a present value of $400,000. In the real world, employment rate effects are quite persistent, lasting at least 15 or 20

[28] In addition, as will be seen later on, these multiplier figures of 1.41 needs to be adjusted downwards due to cost feedbacks, but adjusted upwards due to supply-side job creation effects. As shown later, the effective long-run multiplier is 1.19, not 1.41. This requires that the cost figure be adjusted up by 0.41/0.19 = 2.16, to around $300,000 per long-run job created that boosts the local employment rate. (140,158 × 2.16 = $302,741. This $300,000 effect is still less than the $420,000 in labor market benefits cited in the text.

years, but they may not last forever. But there are also real wage increases that are labor market benefits that should be considered. (As well, there are the other benefits, such as fiscal benefits and property value benefits.) Overall, I estimate that in a typical local labor market, the present value of labor market benefits in creating one more job add up, in today's dollars, to $420,000—$339,000 due to a higher employment rate, and $81,000 due to higher real wages.[29] This number might be considerably less in a high employment rate local labor market, and more in a low employment rate local labor market.

Furthermore, business tax incentives also have a very high cost per job created. Updating some prior figures to today's dollars, I get that the typical business tax incentive package would have a cost per job created of $359,000.[30] Why so high? Because the "but for" for business incentive packages is typically lower than 25 percent, and sometimes much lower (Bartik 2018c). In other words, for typical business incentives, at least 75 percent of the time the same project would have gone on anyway, or similar job creation would have occurred via some other firm. Even if business incentives are distributed up front, which tips more location decisions than more delayed incentives, their "but for" is modest compared to the dollars expended.

For example, suppose the $15,425 Tulsa Remote incentive is instead reimagined as a per job incentive to attract some firm to Tulsa. The tax incentive model estimates the effects of such incentives on location probabilities by extrapolating from numerous research studies on how sensitive business location decisions are to state and local business taxes and business incentives. The model estimates that such a one-time $15,425 per job incentive would tip 6.0 percent of location decisions.[31] To put it another way, the model estimates that for a one-time business tax

[29] This updates the figures in Bartik (2022) from 2020 dollars to 2024 dollars.
[30] This updates figures in Bartik (2022) from 2020 to 2024 prices.
[31] To be more comparable to Tulsa Remote, I assume the per job incentive is paid with a one-year lag after the jobs are created.

incentive, getting to the 57.9 percent "but for" rate of Tulsa Remote would take an incentive of $267,000 per job.[32]

Why, for the same dollars, can Tulsa Remote achieve a much higher "but for" rate than typical business incentives? Or, to put it another way, why is it over 17 times as costly ($267,000 divided by $15,425 = 17.3) to tip business location decision via tax incentives as it is to tip remote worker location decisions via a remote worker attraction program such as Tulsa Remote? The basic intuition here is that remote workers, almost by definition, are far more "footloose" than the typical firm making a plant location decision. Remote workers can work anywhere with a good internet connection. Even modest variation in "incentives"—cash payments, services, amenities, whatever makes a place desirable—can have large effects on remote worker location decisions. In contrast, a firm producing some goods or services typically faces numerous constraints related to distance from market, distance from suppliers, availability of skilled labor, availability of land, agglomeration economy benefits from nearby firms, etc. Therefore, it is much more costly to get a business firm to alter its location decision compared to an average remote worker.

Let's now turn to the second question: Why are property value benefits and fiscal benefits so large compared to labor market benefits? For property value benefits, a big reason is that the direct jobs in Tulsa Remote member households that are induced by the program have a one-for-one effect on population, but no effect on the local employment rate. These population increases are what drive up property values. If these jobs had the same effects on employment rates versus

[32] This up-front incentive would have a total cost of less than the $359,000 cost figure previously cited in the text for typical incentive packages. $267,000 divided by a but for of 0.579 yields a cost per direct job created of $462,000. If the effective multiplier after cost offsets is 1.6, the cost per job created including multiplier jobs would be $462,000/1.6 = $288,000. The cost is less than the previously cited $359,000 figure for "typical incentives" in that typical incentives are spread out over many years: 15-year job creation credits, 20-year property tax abatements, etc. This reduces their cost effectiveness considerably.

population that would be expected for normal job creation programs, which just increase labor demand without at the same time directly inducing increased labor supply, the resulting ratio of property value benefits to labor market benefits would be 18 percent, compared to the 95 percent ratio in Table 12 (95 percent = 26,427 ÷ 27,705 in Table 12).[33]

As for fiscal benefits, they loom large in part because Tulsa Remote generally has large effects on both employment and population, and balanced increases in both tend to increase fiscal benefits, but not labor market benefits. This occurs in part because population increases will drive up both property values and property tax revenues.

Turning to Table 12's income distribution effects, the overall net benefits are modestly progressively distributed. For example, the bottom quintile gets 11.6 percent of net benefits, over twice its baseline share of income of 5.1 percent. The greater than baseline share of net benefits is also true for the lower middle-quintile and middle-income quintile as well. For example, the middle-income quintile's share of net benefits of Tulsa Remote is 18.2 percent, versus its baseline income share of 13.7 percent. In contrast, the top two income quintiles' share of net benefits from Tulsa Remote are less than their baseline income shares. For example, the top income quintile receives 43.2 percent of the program's net benefits, below its baseline income share of 52.0 percent.

Thus, the program on net tends to redistribute income to lower income groups versus upper income groups, while also providing benefits for all groups. If one looks at the benefit-cost ratio for each group—the ratio of gross benefits to the incentive costs they face—the groups all have benefit-cost ratios that are greater than 3-to-1, but none exceed 5-to-1.

[33] This scenario is not a realistic scenario, so is not further discussed, as the very essence of Tulsa Remote is to shock both labor demand and labor supply. But this scenario shoots the overall benefit-cost ratio up to 13.32, as it extraordinarily increases labor market benefits.

On the other hand, it is also the case that the top income quintile gets almost 4 times the benefit for the bottom income quintile (22,041 over 5,914 equals 3.73). The Tulsa Remote program is an economic development program that broadly boosts the entire Tulsa economy, and as such it tends to have broad benefits, which to some extent will be shaped by the baseline distribution of income and property. "Economic development programs," even when they increase the progressivity of the income distribution, are never as progressive as an income transfer program or public service program that is 100 percent targeted on the lowest income quintile. But their broad benefits may make them more politically and economically sustainable.

The modestly progressive effects of Tulsa Remote are driven by how its income boosts are distributed across types of income. The model uses prior estimates that gains in employment rates and wages due to job growth will be moderately progressively distributed.

In addition, the model progressivity is driven in part by the fact that fiscal benefits exceed incentive costs. The model uses data on any particular state's tax system to estimate the effects of fiscal benefits and incentive costs on the distribution of income.[34] The model also uses information on how the benefits of public spending is distributed by quintile to generate the incidence of fiscal benefits and incentive costs by quintile (Prante 2013; see Bartik 2018a). As fiscal benefits exceed incentive costs, on net the Tulsa Remote program generates a budget surplus. This is assumed in the model to be half devoted to tax cuts, and half to spending increase. As the state and local tax system in Oklahoma is, as in most states, "regressive"—tax rates on lower income groups are a higher percentage of income than for higher income groups—cuts in tax rates have a progressive effect on the income distribution. As the benefits of most state and local public spending is modestly progressively distributed by income quintile—

[34] As described in Bartik (2023b), these state-specific tax distribution data come from the Institute on Taxation and Economic Policy (2018).

lower income quintiles receive higher benefits as a percent of income than their baseline income shares—the spending increases also have a progressive income distribution.

In addition, the model treats changes in K–12 education spending separately. Because the net fiscal benefits of Tulsa Remote exceed its program costs, the program enables increases in K–12 spending. These increases are estimated to increase future earnings of children who experience these public-school spending increases, and such future earnings are included in the model for children who remain in Oklahoma. These increases are assumed, based on prior research, to be strongly progressively distributed, although not as progressively distributed as for programs exclusively targeted on the poor. But based on the research evidence, the lowest income quintile has more children per household on average, and K–12 spending increases have somewhat greater effects for children from lower income groups. Hence, for example, the model has "education benefits" that are almost a third greater in dollar terms for the lowest income quintile than for the highest income quintile (1.32 = 1,480/1,123).

These progressive effects of labor market benefits, fiscal benefits, and education effects exceed the regressive effects of paying for the Tulsa Remote incentive and other program costs, and the regressive effects of the property value increases. The property value increases, in particular, disproportionately accrue to the top two income quintiles. The top income quintile, for example, gets 73.2 percent of the property value increases, greater than its baseline income share of 52.0 percent (0.732 = 19,341/26,426).

This discussion of different types of "income effects" of Tulsa Remote could be misread as assuming that these effects are separable. However, as this is a model of a regional economy, with many interacting pieces, in general changes that affect labor market benefits will affect other benefit types, such as fiscal benefits and property value benefits, and vice versa. This report

will now turn to examining these interactions, by considering various thought experiments that directly shock one of these income types. It is useful to look at some of these interactions, for two reasons: first, to better understand the model and what it is saying. Second, because some of these thought experiments correspond to some real-world policy choices, or at least suggest possible policy options to consider.

Table 13 compares the baseline scenario from Table 12 with the following scenarios:

- Eliminating local labor market benefits by assuming that job growth has zero effects on employment rates or real wages.
- Fiscal benefits eliminated by sending them all to someone in the Cayman Islands.
- Fiscal benefits devoted to worthless public spending eliminates the benefits of budget surpluses from cutting taxes or providing valuable education and other public services, but still keeps the demand-side job creation effects of public spending.
- Fiscal benefits (and other benefits) boosted by assuming that the Tulsa Remote direct jobs in member households pay a 10 percent wage premium, rather than the baseline zero wage premium.
- Property value increases are eliminated by assuming that housing supply responds sufficiently to increased population so as to eliminate housing price increases.

Table 13 Benefits and Costs per Member, Different Scenarios

	Baseline	No labor market benefits	Fiscal benefits thrown away completely	Fiscal benefits: public spending worthless, but affects demand	10% wage premia for member jobs	Housing prices don't respond to pop growth
Total Gross	66,462	32,784	12,743	27,617	104,388	124,405
Fiscal benefits	32,252	27,986	0	0	55,114	45,425
Labor market benefits	27,700	-	17,062	24,668	35,424	71,560
Property value benefits	26,426	26,678	24,931	26,006	27,508	0
Real value of non-labor income	(19,385)	(19,610)	(18,386)	(19,220)	(20,084)	(0)
Education effects	6,400	3,804	(10,015)	-	17,669	13,192
Exported business taxes	(3,046)	(2,274)	2,793	-	(7,186)	(5,431)
Local business effects	(3,885)	(3,800)	(3,641)	(3,838)	(4,058)	(341)
Incentive costs	(15,425)	(15,425)	(15,425)	(15,425)	(15,425)	(15,425)
Benefit-cost ratio	4.31	2.13	0.83	1.79	6.77	8.07

NOTE: Baseline simulation identical to Table 12. Each other simulation differs in making one change. Negative numbers in parentheses. All figures are in present value dollar terms, except for benefit-cost ratio. All these estimates are totals over all income groups.
SOURCE: Author's estimates of different simulation models.

Local Labor Market Scenario

This scenario not only directly reduces labor market benefits but also ends up reducing fiscal benefits and hence future education effects on future generations' earnings. These fiscal benefit effects and education effects occur because the elimination of labor market earnings increases reduces the government revenue effects of Tulsa Remote. The end result is to cut the benefit-cost ratio in half, from 4.31 to 2.13.

The local labor market scenario is not a realistic policy option, but it does suggest the implications of circumstances or policies that might alter job creation effects on employment rates or real wages. For example, if the Tulsa labor market was at full employment, the benefit-cost picture would tend in the direction of this scenario. On the other side, if Tulsa improved the size and effectiveness of local job training and other job access policies (e.g., provided better child care or transportation access to jobs), then more jobs might go to the local unemployed than currently assumed, and the benefit cost ratio would likely increase over the baseline.

Fiscal Benefit Scenarios

The first scenario (Cayman Islands) is completely unrealistic, but it does show that eliminating all fiscal benefits has dramatic effects on the program's benefits—the benefit-cost ratio is reduced to 0.83; that is, the program benefits are less than program costs, at 83 percent of costs. Financing the program causes education losses, and the lack of lower taxes or increased spending reduces future job creation and labor market benefits. The other fiscal scenario—spending on worthless public services—restores the demand-side effects of fiscal benefits, showing that they are important in explaining how fiscal benefits affect the overall benefits of Tulsa Remote. But, as shown, overall benefits are considerably reduced so that the program's benefit-cost ratio is cut by more than half (reduced from 4.31 to 1.79). Although this second

scenario is also extreme, it does illustrate that the importance of the ways in which governments use the program's fiscal benefits.

Ten Percent Wage Premia Scenario

As discussed in Appendix D, there is some uncertainty about whether or not Tulsa Remote workers have higher compensation than is the case for a typical job that is attracted by incentives. The baseline scenario makes the conservative assumption that there is no wage premium. What if there was some wage premium? The wage premium scenario explores benefits if those selected for Tulsa Remote had compensation that exceeded typical jobs attracted by incentives by 10 percent.

With this 10 percent wage premia, the benefit-cost ratio for the program significantly increases, from 4.31 in the baseline to 6.77. Most of this occurs due to increases in fiscal benefits, which increase because the induced Tulsa Remote members have higher incomes, which boosts state and local tax bases. These higher fiscal benefits in turn have positive effects on education benefits because the higher tax base enables Tulsa to spend more on public schools, which boosts future earnings of Tulsa residents. Labor market benefits also go up to some extent: some of the fiscal benefits result in higher spending on local goods and services, which boosts local jobs.

Property Value Expansive Supply Scenario

This scenario makes an extreme assumption: the population increase is accommodated with zero increases in housing prices. However, local policymakers, by their actions on zoning and housing codes, could move the real world partway in this direction, by allowing housing

supply to expand more readily than assumed in the baseline scenario.[35] This scenario has extremely favorable effects on the overall benefit-cost ratio, increasing it from the baseline 4.31 to 8.07. Although property value benefits are lost, this is more than offset by many favorable effects: overall prices go up less, which reduces some adverse effects of higher costs on business job creation, which in turn increases labor market benefits. In addition, the scenario eliminates the reduction in the real value of non-labor income that Tulsa residents receive from sources outside of Tulsa, and which is not linked to local prices.

The analysis above focuses on how Tulsa Remote affects the original Tulsa residents in terms of various income types' "present value." But as a summary measure, present values may not be sufficient for many policymakers and readers who want to know when different income effects occurred? The next section turns to considering how Tulsa Remote's effects evolve over time.

DYNAMIC EFFECTS OF TULSA REMOTE

Beyond present values, the model yields estimates for each year after Tulsa Remote members move to Tulsa, up to 80 years. These estimates include income flows, employment changes, population changes, etc. One important reason to look at these year-by-year effects is that policymakers may value the short-term more than is implied by using a 3 percent discount rate to discount future income flows. In addition, long-term effects could be argued to be more uncertain, which perhaps suggests they should be discounted more heavily. Finally, looking at

[35] The baseline scenario assumes a housing price elasticity of 0.300; that is, a 1 percent increase in Tulsa's population will increase housing prices by 0.3 percent. One could imagine that policy might cut this in half. The scenario cuts this elasticity to zero.

these dynamics gives further insight into why Tulsa Remote has particular effects, which might suggest ways of augmenting benefits or reducing costs.

Table 14, and the accompanying Figures 2a and 2b, show benefits by year for gross benefits and the three largest benefit categories: fiscal benefits, labor market benefits, and property value benefits.

The table and figures reveal that most of the overall benefits of Tulsa Remote, under these baseline assumptions, occur in the first five years. After that, there are some years of negative gross benefits, even before considering incentive costs. This is followed by a number of years of fairly steady gross benefits.

Among these three benefit types, property value benefits occur almost immediately but then are soon somewhat offset by some negative effects on property values. Labor market benefits also occur immediately but persist for six years before turning slightly negative for a period, before then turning positive again. In contrast, fiscal benefits, with a slight lag, are consistently positive, with the greatest effects in the first few years.

What is going on in the model? Using the model's assumptions, Tulsa Remote directly shocks both employment and population by inducing 57.9 percent of member households to move to Tulsa. But each year, under the model's retention assumptions, some of these induced households move out before this stabilizes at 33 percent retention at year 10. This positive population shock immediately puts upward pressure on housing prices. But the following negative population shock as these initial households leave Tulsa will reduce property values in subsequent years, although not below their original value. Capital gains in property values are followed by lesser capital losses. These property value increases are assumed to take place immediately, which is consistent with the results for owner-occupied housing in Bartik (1991).

Table 14 Benefits per Member, by Year

Year	Fiscal	Labor Market	Property	Total Gross Benefits
1	0	9,231	49,730	57,780
2	2,810	12,565	(3,719)	12,813
3	3,238	7,963	(6,634)	3,017
4	2,348	4,739	(3,062)	2,713
5	1,770	2,302	(3,131)	(193)
6	1,312	477	(3,096)	(2,288)
7	950	(817)	(2,968)	(3,686)
8	673	(1,680)	(2,782)	(4,534)
9	468	(2,214)	(2,566)	(4,970)
10	319	(2,504)	(2,340)	(5,115)
11	213	(2,287)	(300)	(2,951)
12	243	(1,830)	(109)	(2,293)
13	315	(1,360)	13	(1,658)
14	392	(957)	88	(1,134)
15	461	(639)	133	(732)
16	518	(399)	159	(435)
17	564	(225)	171	(225)
18	600	(102)	176	(80)
19	629	(18)	175	17
20	653	39	172	83
21	672	76	167	125
22	689	99	161	152
23	703	112	155	168
24	716	116	150	177
25	728	117	146	185
26	740	113	141	187
27	751	108	138	191
28	762	102	135	195
29	773	94	132	198
30	784	88	131	208
31	795	83	130	221
32	808	79	129	237
33	820	76	128	256
34	833	74	128	280
35	847	74	129	309
36	861	74	130	340
37	876	75	131	375
38	892	76	130	408
39	907	80	131	449
40	924	85	132	493
41	941	90	132	536
42	958	96	134	587
43	976	101	135	633
44	994	105	136	681
45	1,012	111	138	734
46	1,030	119	138	789
47	1,050	130	139	848
48	1,070	146	139	917
49	1,091	166	140	992
50	1,114	187	140	1,070
51	1,137	209	141	1,151
52	1,160	233	142	1,234
53	1,184	252	142	1,309

Table 14 (Continued)

Year	Fiscal	Labor Market	Property	Total Gross Benefits
54	1,207	271	145	1,387
55	1,230	286	147	1,461
56	1,253	300	149	1,533
57	1,276	310	149	1,598
58	1,298	315	150	1,655
59	1,319	317	150	1,705
60	1,339	317	151	1,754
61	1,359	313	149	1,791
62	1,378	308	149	1,827
63	1,397	303	148	1,860
64	1,416	296	147	1,890
65	1,434	291	148	1,923
66	1,452	289	147	1,958
67	1,471	288	147	1,994
68	1,491	286	149	2,029
69	1,510	282	150	2,063
70	1,529	278	152	2,096
71	1,548	273	154	2,127
72	1,567	267	155	2,158
73	1,586	260	156	2,186
74	1,605	254	157	2,215
75	1,624	247	158	2,243
76	1,644	240	159	2,270
77	1,663	233	160	2,299
78	1,683	225	160	2,323
79	1,702	218	162	2,350
80	1,722	212	163	2,379

NOTE: All estimates are dollar benefits in a given year, per Tulsa Remote member who arrives in Tulsa in year 1. Negative numbers in parentheses.
SOURCE: Author's estimates of baseline model, simulations by year.

Figure 2a. Gross Benefit per Member by Year, First 20 Years

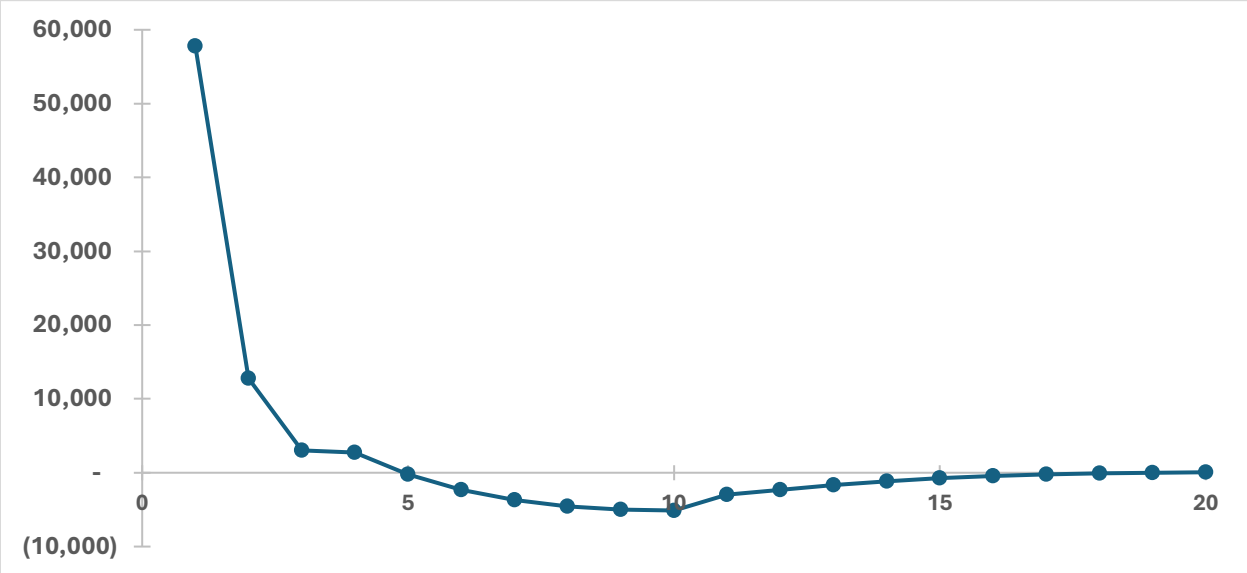
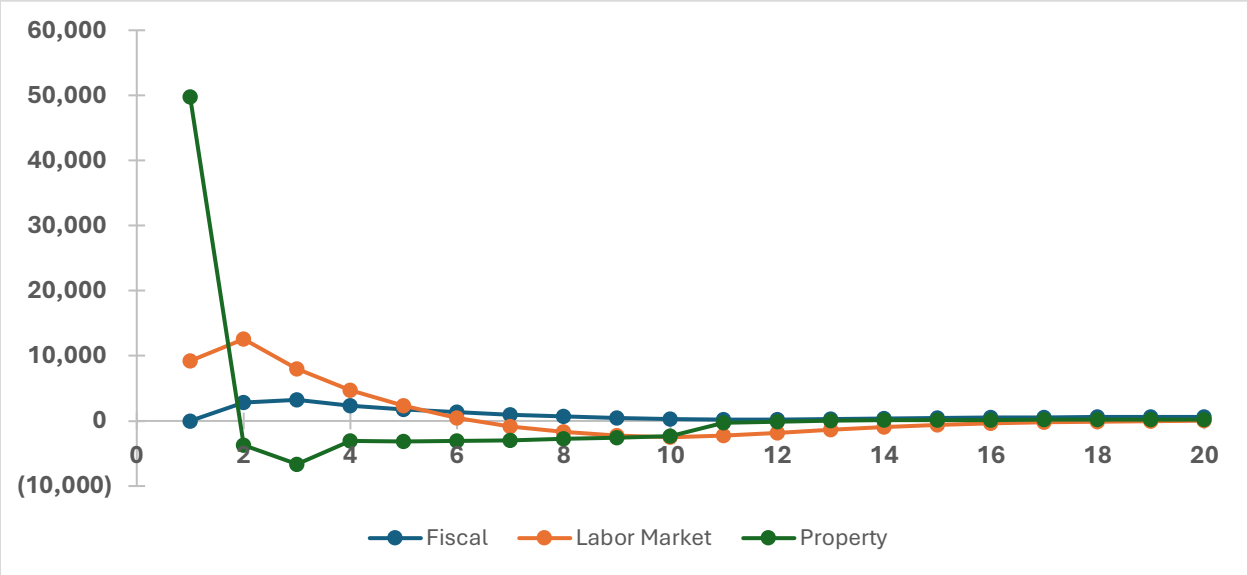


Figure 2b. Benefits per Member by Year by Type: Fiscal, Labor Market, Property, First 20 Years



NOTES ON FIGURES 2a and 2b: All figures are annual dollar benefits per Tulsa Remote member, where year 1 is year member arrived in Tulsa. Negative numbers in parentheses. Horizontal axis numbers the year.
SOURCE: Table 14 numbers, reflecting baseline estimates and simulations by author.

The labor market effects are also due to an initial positive shock to employment, followed by lesser negative shocks to employment, but these employment shocks are due to the indirect jobs in the local retail sector. These labor market effects are a bit more delayed and spread out for several reasons. First, the model assumes that multiplier jobs in the retail sector respond with some lag to the initial shocks to direct jobs in member households. Second, the model relies on previous estimates that a shock to employment will initially have higher effects on employment rates, followed by an abrupt decline to a lower level, which will then slowly depreciate over time.

Fiscal effects take place with a lag but then are more persistent over time. The model assumes that any fiscal benefits in a given year are not used to cut taxes or increase public spending until the following year. Fiscal benefits depend in part on the overall increase in the level of employment, as well as what that does to the level of personal income and hence government revenue, relative to the public spending needs implied by the overall increase in the

level of population. Because of this dependence on the change (since baseline) in the levels of employment—and hence personal income—versus levels of population—and hence spending needs—fiscal benefits tend to remain positive over time, although at a slightly lower level than in the first several years.

Labor market benefits, fiscal benefits, and gross benefits tend to turn slightly more positive as we look at periods beyond 10 years after the initial arrival of Tulsa Remote members. As will be explored further below, this is due to two long-term supply-side effects of Tulsa Remote. First, adding college graduates will tend to increase long-run job growth, an effect that is assumed to take place gradually and cumulatively over time. Second, the fiscal benefits of Tulsa Remote in excess of program costs will increase K–12 spending, which has positive effects on earnings of the affected children, but only with long lags, based on the time it takes those affected children to grow up and reach their peak earnings years. These long-run earnings effects due to more education have indirect effects on boosting fiscal benefits and labor market benefits and property value benefits, but also have their own direct effects on gross benefits, which will be seen later on in this section.

Here's another point that helps distinguish a "benefits" model from models that look simply at the aggregate increase in labor income: at the peak (in year 2), this economic development benefits model estimates an annual labor income effect of $12,565 per member. This is far less than the $181,213 or $157,300 per member estimates of the aggregate impact of Tulsa Remote on total labor income in Tulsa (Tulsa Remote 2024; Newman, O'Dell, and Fikri 2021). The latter estimates are probably reasonable measures of impacts on total labor income of Tulsa, but they do not focus on the change in per capita income of the original Tulsa residents. Furthermore, this model's estimates allow for the "but for" for Tulsa Remote inducing in-

migration to be 57.9 percent rather than the 100 percent implicitly assumed in the prior aggregate impact estimates. Finally, these model estimates allow for the multiplier to be reduced by cost feedbacks, as will be shown next. Once we allow for all of these model effects, the effect on per capita labor incomes is reduced by more than 90 percent compared to estimated effects on aggregate labor income ($12,565 versus $181,213).[36]

Table 15 and Figure 3a show the effects on total jobs of Tulsa Remote over time. The total job effect is stated as a job effect per member. This total job effect includes the direct jobs in member households of those induced to move to Tulsa by Tulsa Remote, who then remain through that year. The total job effect also includes the indirect jobs due to multiplier effects of these direct jobs on Tulsa's local retail jobs. Indirect jobs also include the supply-side jobs created because each college graduate is assumed to over time gradually increase cumulative job growth in Tulsa, either by starting up their own businesses or by attracting more high-tech job growth. The indirect jobs are also shown in Table 15 and Figure 3a because they deserve special emphasis, as it is only these jobs that directly boost employment rates and real wages of Tulsa residents. But the total jobs are also important because they boost fiscal benefits, which have direct benefits and also lead to demand-side and supply-side boosts to Tulsa's economy, which benefits Tulsa residents. Finally, the third column in Table 15 and Figure 3b shows the implied net multiplier. This is the ratio of total jobs to direct jobs. This net multiplier in part depends on the 1.41 input-output multiplier, which is what is assumed in the model. But this multiplier is reduced by increased costs, which discourages Tulsa job growth.

[36] Of note: in the internal model workings, the peak effect on aggregate labor income in year 1 is $76,185 per member, which, divided by 0.579, yields $131,580 per member of an increase in aggregate labor compensation if the "but for" rate was 100 percent. This is not far below the prior EIG and Tulsa Remote estimates. The models roughly agree on aggregate annual labor income impacts, but the current model does not count most of this aggregate impact as a benefit.

Table 15 How Total Jobs per Member, and Indirect Jobs/Member, Vary over Time (Year 1 = Year Member Arrives in Tulsa)

Year	Total jobs/member	Indirect jobs/member	Implied multiplier
1	0.959	0.163	1.21
2	0.922	0.249	1.37
3	0.759	0.205	1.37
4	0.674	0.169	1.33
5	0.597	0.137	1.30
6	0.528	0.109	1.26
7	0.468	0.084	1.22
8	0.415	0.064	1.18
9	0.369	0.048	1.15
10	0.329	0.035	1.12
11	0.326	0.030	1.10
12	0.328	0.030	1.10
13	0.332	0.032	1.11
14	0.335	0.035	1.11
15	0.339	0.037	1.12
16	0.342	0.039	1.13
17	0.345	0.040	1.13
18	0.347	0.042	1.14
19	0.349	0.043	1.14
20	0.351	0.043	1.14
21	0.352	0.044	1.14
22	0.353	0.045	1.14
23	0.354	0.045	1.15
24	0.355	0.045	1.15
25	0.356	0.046	1.15
26	0.357	0.046	1.15
27	0.357	0.046	1.15
28	0.358	0.046	1.15
29	0.358	0.047	1.15
30	0.359	0.047	1.15
31	0.360	0.047	1.15
32	0.360	0.047	1.15
33	0.360	0.047	1.15
34	0.361	0.048	1.15
35	0.361	0.048	1.15
36	0.362	0.048	1.15
37	0.362	0.049	1.15
38	0.363	0.049	1.16
39	0.363	0.049	1.16
40	0.364	0.050	1.16
41	0.364	0.050	1.16
42	0.365	0.050	1.16
43	0.365	0.051	1.16
44	0.366	0.051	1.16
45	0.366	0.051	1.16
46	0.367	0.052	1.16
47	0.367	0.052	1.17
48	0.368	0.053	1.17
49	0.368	0.053	1.17
50	0.369	0.054	1.17
51	0.369	0.054	1.17
52	0.370	0.055	1.17

Table 15 (Continued)

Year	Total jobs/member	Indirect jobs/member	Implied multiplier
53	0.370	0.055	1.17
54	0.371	0.056	1.18
55	0.371	0.056	1.18
56	0.372	0.056	1.18
57	0.372	0.057	1.18
58	0.372	0.057	1.18
59	0.373	0.057	1.18
60	0.373	0.058	1.18
61	0.373	0.058	1.18
62	0.374	0.058	1.18
63	0.374	0.058	1.19
64	0.374	0.059	1.19
65	0.374	0.059	1.19
66	0.374	0.059	1.19
67	0.375	0.059	1.19
68	0.375	0.059	1.19
69	0.375	0.060	1.19
70	0.375	0.060	1.19
71	0.375	0.060	1.19
72	0.375	0.060	1.19
73	0.375	0.060	1.19
74	0.376	0.060	1.19
75	0.376	0.060	1.19
76	0.376	0.060	1.19
77	0.376	0.061	1.19
78	0.376	0.061	1.19
79	0.376	0.061	1.19
80	0.376	0.061	1.19

NOTE: First two columns are jobs estimated to be created by Tulsa Remote of various types, per Tulsa Remote member, as of various years after Tulsa Remote member arrives in year 1. Implied multiplier is ratio of Total jobs to Direct jobs, which is equal to (Total jobs per member divided by (Total jobs per member minus indirect jobs per member)).
SOURCE: Author's estimates and simulation of baseline model.

Figure 3a. How Total Jobs and Indirect Jobs per Member Evolve over Time

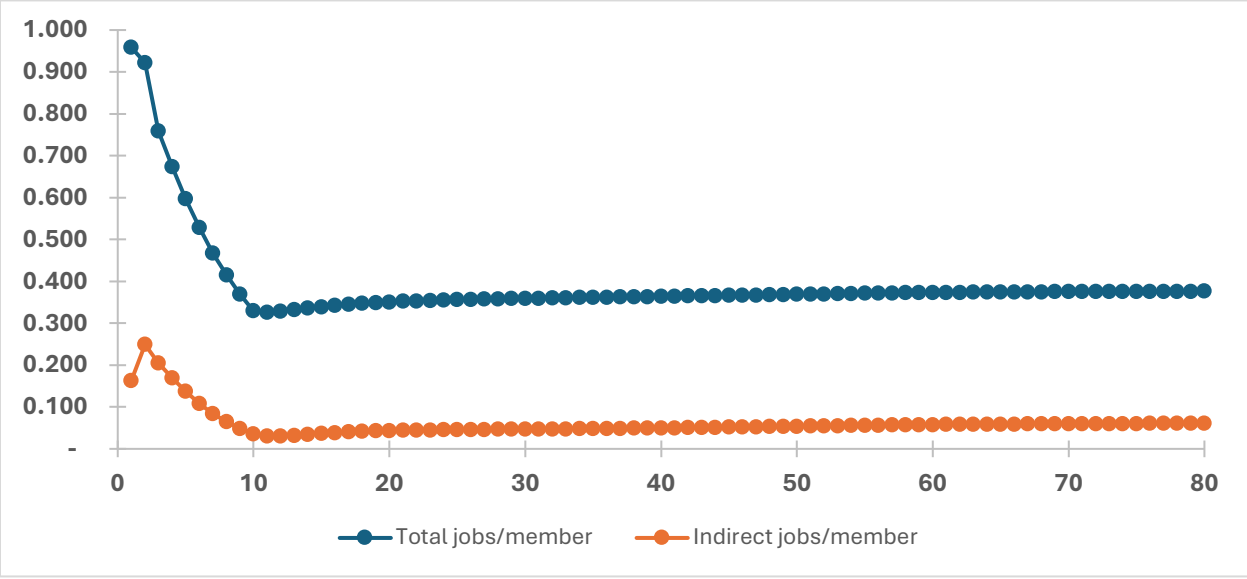
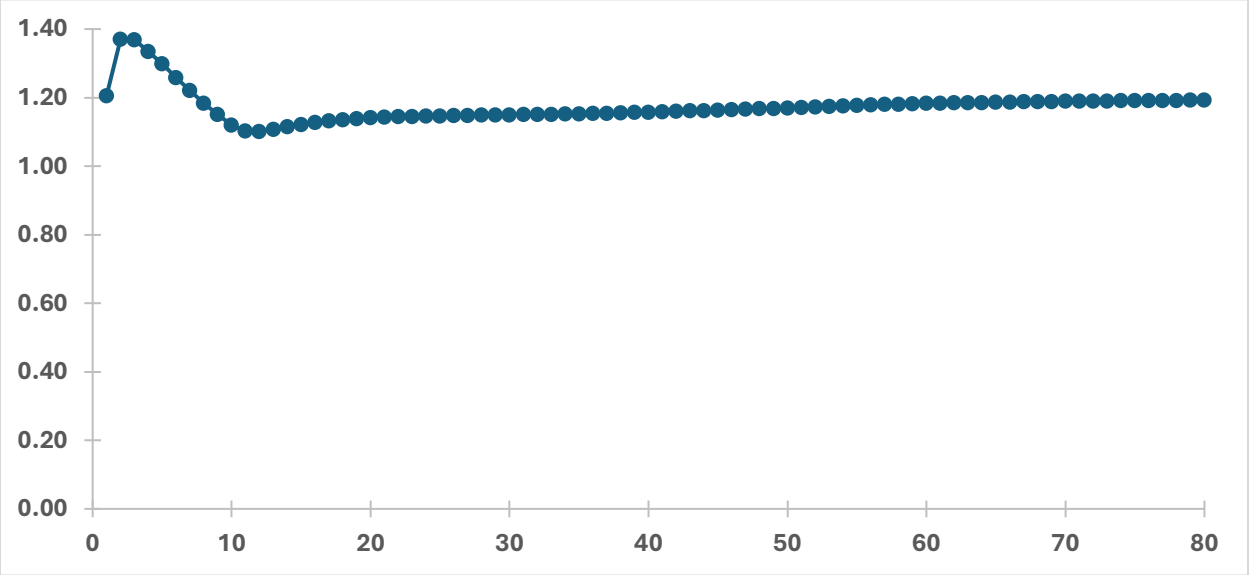


Figure 3b. How Multiplier Evolves over Time



NOTES ON FIGURES 3a and 3b: Horizonal axis shows year, where year 1 is year that member arrives in Tulsa.
SOURCE: Table 15, derived from author's baseline estimates and simulation.

On the other hand, this multiplier is increased in the long run by the supply-side job creation effects of more college graduates.

As Table 15 and Figures 3a and 3b show, Tulsa Remote results in an initial boost to total jobs and indirect jobs, which is followed by a decline, which after 11 years is partially reversed and leads to a slight increase from year 11 on in jobs. What is going on? Tulsa Remote initially has direct effects on jobs due to inducing some Tulsa Remote member households to move to Tulsa. This in turn leads to some indirect multiplier effects on retail jobs. These effects erode over time in the first 10 years for two reasons. First, some Tulsa Remote members who were induced to relocate to Tulsa do not stay, which reduces direct jobs. Second, the indirect jobs effect declines an additional amount because the multiplier declines over time, as rising costs discourage some job growth. The increase after 11 years occurs because these negative effects on the multiplier are offset by the positive supply-side effect of the added Tulsa Remote college

graduates. Another factor is that the model assumes that the effective retention of Tulsa Remote members stabilizes at a retention rate of 33 percent after 10 years.

An implication of this is that anything that boosts retention, or boosts supply-side effects of Tulsa Remote members, will tend to boost these long-run job effects. This implication will be explicitly shown later.

Next, we turn to how government revenue and government spending needs evolve over time, with the difference being the annual fiscal benefit. Table 16 and Figure 4 show the evolution of per member annual state and local government revenue, and needed state and local public spending.

As the table and figure show, annual revenue generated and spending needs both peak in the first few years, then gradually decline for the next 10 years before slowly increasing.[37] The pattern for the first decade or so reflects Tulsa Remote member households being initially attracted to Tulsa, and only some percentage (33 percent) remaining after 10 years.

After the first dozen years or so, revenue generated and spending needs both gradually increase. This pattern reflects that the Tulsa Remote members remaining in Tulsa stabilize after 10 years, and over time jobs are generated from past fiscal surpluses, and from jobs created by the added college graduates.

[37] Note that no effects on revenue generated or spending needs occur until year 2. This reflects that the model assumes that these effects on revenue and public spending needs are not recognized by the government until the next year, when they give rise to changes in taxes and public spending.

Table 16 State/Local Fiscal Effects per Member (Year 1 = Member Arrives in Tulsa)

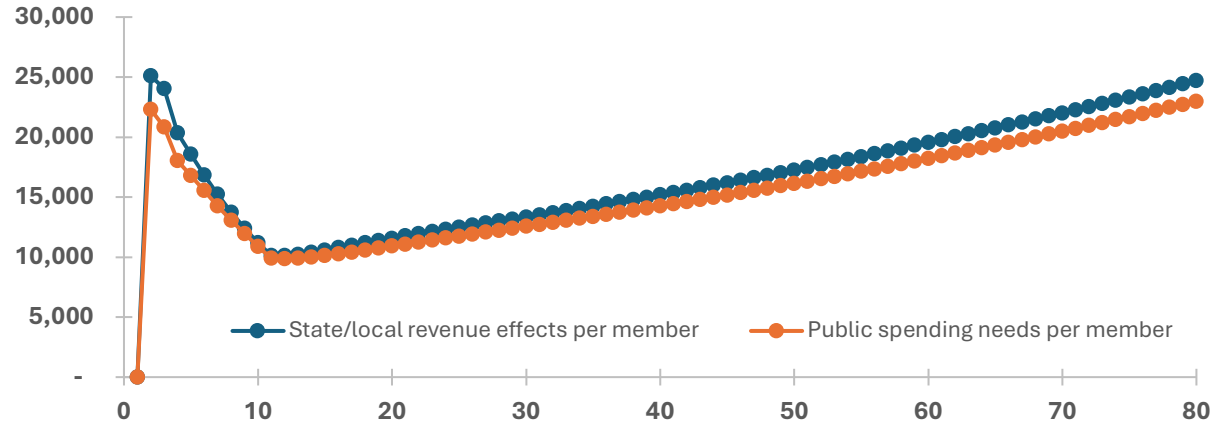
Year	State/local revenue effects per member	Public spending needs per member
1	-	-
2	25,095	22,285
3	24,043	20,805
4	20,354	18,006
5	18,555	16,785
6	16,834	15,522
7	15,214	14,264
8	13,727	13,054
9	12,384	11,916
10	11,185	10,866
11	10,122	9,908
12	10,100	9,857
13	10,205	9,891
14	10,371	9,979
15	10,563	10,102
16	10,764	10,246
17	10,967	10,403
18	11,167	10,567
19	11,363	10,734
20	11,555	10,902
21	11,743	11,070
22	11,926	11,238
23	12,107	11,404
24	12,285	11,569
25	12,462	11,733
26	12,637	11,897
27	12,811	12,060
28	12,985	12,223
29	13,159	12,386
30	13,332	12,548
31	13,508	12,712
32	13,684	12,877
33	13,862	13,042
34	14,042	13,209
35	14,224	13,377
36	14,408	13,547
37	14,594	13,718
38	14,783	13,891
39	14,973	14,066
40	15,166	14,243
41	15,362	14,421
42	15,559	14,601
43	15,759	14,783
44	15,961	14,967
45	16,165	15,153
46	16,372	15,342
47	16,582	15,532
48	16,794	15,724
49	17,010	15,918
50	17,228	16,114
51	17,449	16,312
52	17,672	16,512

Table 16 (Continued)

Year	State/local revenue effects per member	Public spending needs per member
53	17,898	16,714
54	18,124	16,917
55	18,354	17,124
56	18,586	17,333
57	18,820	17,544
58	19,056	17,758
59	19,293	17,974
60	19,531	18,191
61	19,771	18,411
62	20,011	18,632
63	20,252	18,855
64	20,494	19,079
65	20,738	19,305
66	20,985	19,532
67	21,233	19,762
68	21,484	19,993
69	21,736	20,227
70	21,992	20,463
71	22,251	20,703
72	22,512	20,945
73	22,776	21,190
74	23,042	21,437
75	23,311	21,687
76	23,583	21,939
77	23,857	22,194
78	24,134	22,452
79	24,414	22,712
80	24,696	22,974

NOTES: Figures are annual dollar revenue or spending needs generated by Tulsa Remote, per member. Year one is year Tulsa Remote member arrives in Tulsa.
SOURCE: Author's estimates of baseline model, and author's simulation.

Figure 4. How Revenue and Spending Effects per Member Evolve over Time



NOTES: Vertical axis are in annual dollars per member. Horizontal axis shows years, where year 1 is year Tulsa Remote member arrives in Tulsa.
SOURCE: Table 16, which is based on author's estimates and simulation.

Of note is that the annual state and local revenue generated per Tulsa Remote member, which peaks at $25,095, far exceeds prior estimates of $4,000 per member (EIG) or $5,000 per member (Tulsa Remote).[38] This is despite the fact that this model assumes only 57.9 percent of Tulsa Remote members were induced to relocate by the program—any tax revenue associated with the 42.1 percent of members who would have moved anyway would not be due to the program and would not be in this model's estimates. Why is the revenue gain so much larger in this model? Although a full answer would require knowing all the details of the prior estimates, the most likely hypothesis is that the current model estimates include many more sources of revenue boosts. The current model allows for property tax revenue to go up due to higher property values, higher population, and higher per capita incomes. Revenue boosts in sales and income taxes due to higher real wages, caused by the added indirect jobs, are also included. The model also allows for revenue boosts from increases in non-labor income as the population grows. The model assumes that intergovernmental revenue from the federal government increases with local population. Finally, the model uses state-specific estimates of how elastic different types of government revenue are to increases in state personal income (Bruce, Fox, and Tuttle 2006). In some cases, boosts to personal income have greater effects on government revenue than are implied by a revenue source's average share of personal income. In the case of Oklahoma, this is true of personal income tax revenue.

These revenue and spending effects lead to fiscal benefits (or costs). These fiscal benefits, together with incentive costs, result in budget effects.[39] As already mentioned, these budget

[38] The Tulsa Remote figure is not directly stated, but can be derived by dividing the $11.2 million in added annual state and local revenue by the 2,252 new and retained Tulsa Remote members (= $4,973).

[39] As mentioned in a prior footnote, the budget effects that alter taxes and spending for Tulsa residents are net of changes in exported business tax revenue. That is, part of the sum of fiscal benefits plus incentive costs are devoted to business tax changes, and some of these changes affect businesses owned by out of state owners. If fiscal benefits exceed incentive costs, as is true in this model, this leads to business tax cuts, some of which do not accrue to Tulsa residents.

effects are assumed to be divided half and half between tax changes and spending changes. In addition to direct and immediate effects on the taxes and spending experienced by Tulsa residents, the model assumes that changes in K–12 education spending affect future earnings of children in school. These effects are counted to the extent to which these children remain in Oklahoma as adults.

Turning to these budget and education effects, we look at how Tulsa Remote's effects on state and local budgets lead, with some considerable lag, to education-related effects on local earnings. Table 17 and Figure 5 show these effects.

As the table and figure show, Tulsa Remote must be paid for initially due to the budgetary costs of the program. The model assumes that a portion of this comes from lower K–12 education spending than would otherwise occur. This temporarily reduces the future adult earnings of some children who were in K–12 during years when this lower education spending occurred. Again, it is important to remind the reader that this "lower education spending" is an opportunity cost of the funds. For example, to the degree to which Tulsa Remote is paid for initially by charitable contributions, this "opportunity cost" is that instead a portion of these funds could have paid for tutoring, after-school and summer school programs, etc. in local public schools. The reduction in earnings is relative to that counterfactual world, not relative to current earnings.

However, after the program is paid for, Tulsa Remote begins generating budget surpluses. These budget surpluses are assumed in part to increase K–12 school spending. With roughly a 30-year lag, this increased K–12 school spending eventually causes net increases in earnings of Tulsa residents who grew up going to the area's schools. These education-related earnings

Table 17 How Net Local Budget Costs per Member and Earnings Effects due to Education Spending per Member Evolve over Time

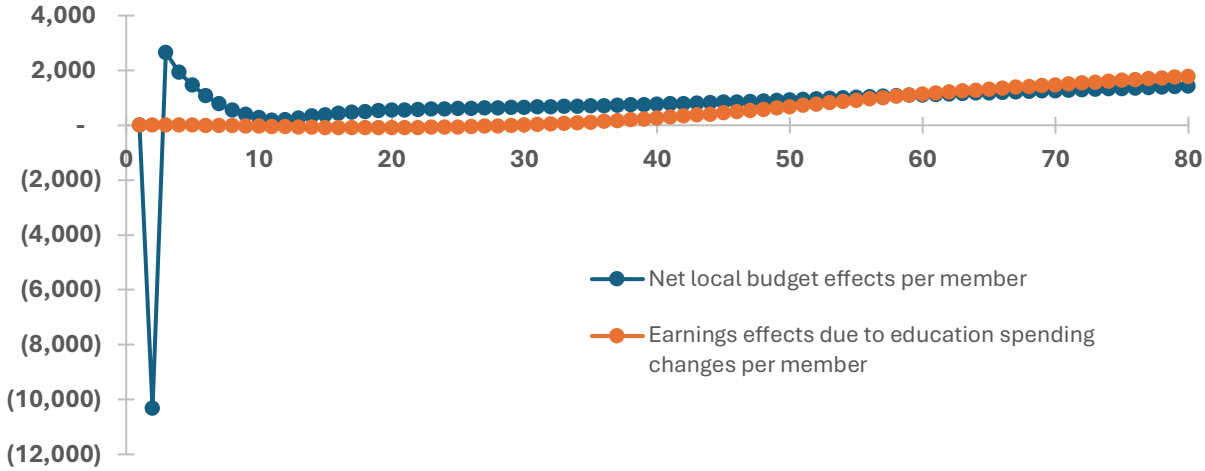
Year	Net local budget effects per member	Earnings effects due to education spending changes per member
1	-	-
2	(10,331)	-
3	2,652	(2)
4	1,923	(5)
5	1,450	(10)
6	1,075	(15)
7	778	(21)
8	551	(29)
9	383	(38)
10	261	(47)
11	175	(56)
12	199	(65)
13	258	(74)
14	321	(83)
15	377	(91)
16	424	(96)
17	462	(101)
18	492	(104)
19	515	(105)
20	534	(103)
21	550	(100)
22	564	(94)
23	576	(89)
24	587	(81)
25	596	(71)
26	606	(62)
27	615	(50)
28	624	(37)
29	633	(24)
30	642	(7)
31	651	11
32	661	30
33	672	51
34	682	75
35	694	101
36	705	128
37	718	157
38	730	186
39	743	219
40	756	252
41	770	286
42	784	323
43	799	359
44	814	397
45	828	437
46	844	477
47	860	520
48	876	565
49	894	612
50	912	660
51	931	709

Table 17 (Continued)

Year	Net local budget effects per member	Earnings effects due to education spending changes per member
52	950	759
53	970	806
54	989	854
55	1,008	902
56	1,026	950
57	1,045	997
58	1,063	1,041
59	1,080	1,083
60	1,097	1,125
61	1,113	1,163
62	1,129	1,200
63	1,144	1,236
64	1,159	1,269
65	1,174	1,303
66	1,189	1,337
67	1,205	1,370
68	1,221	1,402
69	1,236	1,434
70	1,252	1,466
71	1,268	1,498
72	1,283	1,530
73	1,299	1,560
74	1,314	1,592
75	1,330	1,622
76	1,346	1,653
77	1,362	1,684
78	1,378	1,713
79	1,394	1,744
80	1,410	1,774

NOTES: Figures are budget costs or education spending effects on earnings, in annual dollar amounts per Tulsa Remote member. Negative numbers in parentheses. Year one is year member arrives in Tulsa.
SOURCE: Author's estimate and simulation.

Figure 5. How Net Budget Costs per Member and Education Spending Effects on Earnings per Member Evolve over Time



NOTES: Vertical axis shows annual dollar budget effects and education earnings effects per Tulsa Remote members. Horizontal axis shows years, where year 1 is year member arrived in Tulsa.
SOURCE: Table 17, based on author's estimates and simulation.

benefits continue to increase throughout the 80-year forecast period, and eventually exceed in annual dollar magnitude the budget surpluses.

The discussion above about how Tulsa Remote affects the present value of local benefits and costs, and has dynamic effects on annual incomes, assumes the baseline estimates of "but for" and "retention." The estimates so far also assume that Tulsa Remote has just the "normal" effects of college graduates on generating local job growth. What happens if these assumptions are altered? The next section considers this issue.

ALTERNATIVE SCENARIOS FOR TULSA REMOTE'S "BUT FOR," RETENTION RATES, AND SUPPLY-SIDE JOB GROWTH EFFECTS

The above estimates assume the following:

- a Tulsa Remote "but for" of 57.9 percent;
- a retention rate such that 33 percent of those induced Tulsa Remote members are still there at year 10 (implying that 57.9 percent × 33 percent = 19 percent of Tulsa Remote member are both induced and still in Tulsa at year 10; and
- a job creation rate from Tulsa Remote's college graduates of 0.57 added long-run jobs per college graduate.

These assumptions are now altered. First, we consider altering the "but for" rate and the "retention rate." We consider a low "but for" rate of 16.9 percent and a high "but for" rate of 69.7 percent. The low "but for" rate is the rate estimated above for Tulsa Remote applicants who had previously lived in Tulsa. The high "but for" is the highest overall "but for" rate estimated above for the overall Tulsa Remote applicant group.

The report combines these alternative "but for" rates with alternative scenarios for retention of Tulsa Remote members. In addition to the baseline "middle retention rate" used above, the report considers a low retention rate scenario and a high retention rate scenario. These

low retention rate and high retention rate scenarios are derived from data from Tulsa Remote and Newman, O'Dell, and Fikri (2021). Under the baseline scenario (middle retention), the retention rate is 55 percent at year 5 and 33 percent at year 10. Under the low retention rate scenario, retention is 49 percent at year 5 and 27 percent at year 10. Under the high retention rate scenario, retention is 61 percent at year 5 and 51 percent at year 10. Under all three scenarios, retention is assumed to stabilize at the 10-year rate after year 10. Appendix D discusses the derivation of these specific estimates.

Table 18 shows the various combinations of these scenarios. There are three "but for" rates and three retention rates considered, making nine scenarios.

Table 18 How Benefit-Cost Ratio Varies with "But For" and Member Retention

		"But for"		
		16.90%	57.90%	69.70%
Member retention	Low	0.26	3.56	4.60
	Middle	0.46	4.31	5.53
	High	1.08	6.68	8.47

NOTES: Figures are benefit-cost ratios for models that assume various "but for" rates, and member retention rates.
SOURCE: Author's estimates and simulation of various models.

As Table 18 shows, even if the "but for" is low for some group, such as applicants who previously lived in Tulsa, the program can have a benefit-cost ratio exceeding one if the retention rate is high, as it might be for this group. For example, the benefit-cost ratio is 1.08 if retention is high, even with a 16.9 percent "but for" rate. The benefit-cost ratio goes up considerably if the "but for" rate and the retention rate are pushed upward. There is clearly some synergy between the two: pushing up the "but for" and the retention rates simultaneously has greater effects than pushing them up separately. At the extreme, if the "but for" rate is 69.7 percent, and the retention rate is high, the benefit-cost ratio for Tulsa Remote increases from the 4.31 baseline to 8.47.

Alternative scenarios can also be considered that have different supply-side effects of Tulsa Remote's college graduates on local job growth. The baseline estimate that each added college graduate in the local economy will generate, in the long run, 0.57 more local jobs, is based on estimates that simply look at how a higher percentage of college graduates in a local economy affect local job growth. Yet Tulsa Remote, and other Tulsa programs sponsored by the George Kaiser Family Foundation, clearly are attempting to significantly increase the impact of Tulsa Remote members on the local economy by encouraging networking and entrepreneurship. Optimistically, what if such efforts can triple these supply-side effects? Pessimistically, what if we assumed that Tulsa Remote had no supply-side job growth effects?

Table 19 combines these various supply-side scenarios with three different "but for"/retention rate scenarios: a low "but for" and low retention rate scenario; the baseline scenario; a high "but for" and high retention rate scenario. Tulsa Remote's supply-side effects depend both on how many college graduates it attracts and retains, and the assumed supply-side effects per college graduate.

Table 19 How Benefit-Cost Ratio Varies with College Grad Supply Effect, Different "But For"

	No effect	Baseline	Triple baseline
16.9% but for, middle retention	0.41	0.46	0.57
57.9% but for, middle retention	3.70	4.31	5.52
69.7% but for, middle retention	4.65	5.53	7.28

NOTES: Figures are benefit-cost ratios for models that assume various "but for" rates, and member retention rates.
SOURCE: Author's estimates and simulation of various models.

As suggested, there is considerable synergy between whether Tulsa Remote attracts and retains college graduates and these possible supply-side effects. If the "but for" is low and retention is only middling (first row of numbers), the magnitude of the program's supply-side effects per college graduate make little difference. In contrast, if the program is highly successful in attracting college graduates (the third row of numbers), what the program does to encourage

entrepreneurship among its members might make a huge difference. For example, if the program can triple the baseline supply-side effect per college graduate—from 0.57 jobs per college graduate to 1.71 jobs per college graduate—the benefit-cost ratio for the program increases from 5.53 to 7.28. That is, the program's gross benefits increase by 1.75 times the entire program's costs (1.75 = 7.28 – 5.53).

But what do the various scenarios and estimates for Tulsa Remote imply for policy purposes? What does it suggest for how such remote worker attraction programs should be operated in order to maximize their net benefits? The next section considers this issue.

IMPLICATION OF THIS REPORT'S FINDINGS FOR MANAGING REMOTE WORKER ATTRACTION PROGRAMS

As this report shows, remote worker attraction programs have a much higher benefit-cost ratio if they increase both their "but for" and their retention rates. A simple-minded bit of policy advice for managing such programs is to maximize both their "but for" and their retention rates. Easier said than done! Obviously, if Tulsa Remote had mind-reading telepaths on staff—and if applicants in fact knew in advance definitively whether they intended to move to Tulsa and remain there—than Tulsa Remote could achieve a "but for" of 100 percent with 100 percent retention. But this is a fantasy world—in general, it is hard to determine whether a person would have moved to Tulsa without the program and whether they are likely to remain. Tulsa Remote does try to screen out persons already planning to move to Tulsa; for example, by asking applicants this question, but obviously it is difficult to determine what applicants would do without the program.

A critic could argue that Tulsa Remote could increase its "but for" rate by not approving applicants who had previously lived in Tulsa. But even if such a rule was politically feasible, it

might not be desirable. As shown above, even if the "but for" rate for this group was low, the benefit-cost ratio can still be greater than 1 if the retention rate is higher. Finally, Tulsa Remote's benefits increase with the average wages of member households, which should also be considered. Therefore, a more limited conclusion is that if Tulsa Remote has information suggesting a candidate might be more likely to move to Tulsa anyway, the program should try to make sure that that candidate is more likely to remain in Tulsa.

One implication of these results is that the program's various accompanying support activities are important in at least two ways. First, if these networking activities and community engagement activities increase the proportion of Tulsa Remote members who remain long-term in Tulsa, these activities significantly increase the benefit-cost ratio of the program. Second, if these support activities increase the probability that Tulsa Remote members start up new businesses, this also can significantly increase the program's benefit-cost ratio.

In other words, one lesson from Tulsa Remote, both for the program going forward and for other remote worker attraction programs, is that just offering an incentive for workers to move to your city or state is not enough. Tulsa Remote's broader strategy—which includes using various support activities to encourage retention and entrepreneurship—is at least as important as the $10,000 incentive.

Finally, another important supporting policy is making an effort to make housing supply more elastic. Tulsa Remote and other remote worker attraction policies are economic growth policies that by their very nature will increase population. Such pro-growth policies should be properly accompanied by housing supply policies that will accommodate this growth. If adequate housing supply does not appear, the resulting increases in local prices and costs will tend to choke off much of the benefits of promoting growth via remote worker attraction.

CONCLUSION

The key empirical finding of this report is that Tulsa Remote is likely to have a "but for" rate of between 58 percent to 70 percent, or allowing for some statistical uncertainty, between 50 percent and 80 percent. This finding is limited in that it is not from a randomized control trial (RCT). But deferring judgment until we have a RCT is a mistake. We may never have such a RCT. Furthermore, this conclusion of a 50–80 percent "but for" seems robust to alternative model specifications. The data have been sufficiently tortured in this report that it seems unlikely that further efforts will dramatically lower—or raise—these "but for" estimates.

In addition, this paper's simulations, using an economic development model of benefits and costs that rests on prior research on various empirical parameters governing how a regional economy behaves, suggests that a "but for" of 50–80 percent is sufficient for Tulsa Remote to have a benefit-cost ratio of at least 3-to-1.[40] This benefit-cost ratio is considerably greater than that of typical economic development tax incentives. A 3-to-1 ratio is only comparable to more unusual business incentive programs that use more effective incentives, or target high-multiplier firms, or target highly distressed places.

The results also suggest some of the key factors that determine whether Tulsa Remote or other remote worker attraction programs will have high benefit-cost ratios. These include increasing the wage rate of Tulsa Remote members, increasing the retention rate, increasing job creation activities of program members, and accommodating population growth via expanded housing supply.

[40] If the retention rate is lowered from 57.9 percent to 50 percent, the benefit-cost ratio is reduced from 4.31 to 3.52.

Some uncertainties in this analysis could be addressed through additional empirical work. What is the long-run retention rate of Tulsa Remote, now that the program has been going on for more than five years? How many local jobs have been created through Tulsa Remote members engaging in entrepreneurial activities? From empirical studies or conversations with employers, to what degree has the added college graduate supply from Tulsa Remote led to more high-tech job growth?

Successful local economic development can provide valuable benefits for local residents in increased incomes. Whether local economic development policies are worth it depends on how these benefits compare to costs. Conventional economic development business tax incentives often are high cost and may not always have a high benefit-cost ratio, or even net positive benefits. State and local policymakers should be willing to broaden their economic development policies to consider alternative policies, which may be more cost-effective. Based on this report, this broadening to alternative policies should include well-run remote worker attraction programs, as exemplified by Tulsa Remote.

APPENDIX A: INDUSTRY DEFINITIONS

The empirical analysis of Tulsa Remote data condenses the hundreds of industries named by Tulsa Remote applicants into six classifications. The goal of these six classifications was to place industries in some meaningful groups, with a significant percentage of applicants falling into each group.

Category 1 Computers: computer and network security, computer games, computer hardware, computer networking, computer software, e-learning, info services, info technology and services, technology, telecommunications, and wireless.

Category 2 Health-related: alternative medicine, biotech, health, health/wellness, hospital and health care, medical devices, medical practice, mental health care, and veterinary.

Category 3 Banking, insurance, and real estate: accounting, banking, capital markets, commercial real estate, finance, financial services, insurance, investment banking, investment planning, real estate, and venture capital.

Category 4 Industry, management, and PR: automotive, aviation, business supplies and equipment, chemicals, civil engineering, consumer electronics, consumer goods, consumer services, defense and space, design, electrical/electronic manufacturing, executive office, food and beverages, food production, furniture, govt. relations, human resources, import and export, industrial automation, logistics and supply chain, luxury goods and jewelry, machinery, management consulting, market research, marketing and advertising, marketing/pr, mechanical or industrial engineering, media production, motion pictures and film, nanotech, oil and energy, online media, outsourcing, package and freight delivery, packaging and containers, paper and forest products, pharma, printing, professional services, professional training and coaching,

publishing, renewables and environment, research, semiconductors, sporting goods, staffing and recruiting, and writing and editing.

Category 5 Retail, wholesale and other consumer goods and services: apparel and fashion, arts and crafts, cosmetics, entertainment, events services, fine art, gambling and casinos, hospitality, individual and family services, legal services, leisure and, leisure travel and tourism, libraries, museums and institutions, music, performing arts, photography, recreational facilities and services, restaurants, retail, supermarkets, warehousing, and wholesale.

All other industries.

APPENDIX B: OMITTED VARIABLE BIAS ANALYSIS

For the linear probability models, this appendix calculates "why" in a mechanical sense, the coefficient showing the effect of Tulsa Remote approval on the probability of moving to Tulsa changes as we go from a specification with only this variable on the right-hand side to a specification with a full set of control variables on the side. This is done by doing a full set of auxiliary regressions of the control variables on Tulsa Remote approval. These results, along with the control variable coefficients in the original full regression, are used to do a standard decomposition of omitted variable bias.

This standard omitted variable bias decomposition, applied to this specification, is as follows. Suppose that we want to measure the effect of Tulsa Remote approval on moving to Tulsa controlling for a vector of X control variables:

(B1) $Y = B_0 + B_aA + B_xX$

Here, Y is a dummy for whether the person moved to Tulsa. B_0 is the constant term, A is whether Tulsa Remote approved the applicant for the program, B_a is the coefficient on that approval, X is the vector of control variables, and B_x is the set of coefficients on those variables.

How will the coefficient B_a in Equation (B1) compare with what we get from a specification without the control variables:

(B2) $Y = C_0 + C_aA$

Here, C_0 is the constant term in this simple specification, and C_a is the coefficient on Tulsa Remote approval.

A regression of Y on a vector of variables estimates the best linear predictor, in terms of minimizing sum of squared residuals. If we write this best linear predictor of Y conditional on A

as $E(Y|A)$, C_a is the coefficient by applying that linear prediction operator to Equation (B2).

Applying that same linear predictor to Equation (B1),

(B3) $$E(Y|A) = B_0 + B_aA + (B_x \times E(X|A)$$

$$= B_0 + B_aA + B_x*D_0 + B_x*D_aA$$

Here, D_0 and D_a are from an auxiliary regression of X on A.

In other words, C_a in the simple specification will equal $B_a + B_x*D_a$, where B_x and D_a are both vectors of coefficients. By looking at how X affects moving in the full equation (B_x), and looking at how A affects X (D_a) in the set of auxiliary equations (one for each X variable), and multiplying these pairs of estimated coefficients together, we can exactly decompose why the C_a coefficient and B_a coefficient are different.

Tables B1 and B2 show this omitted variable decomposition for the "big sample" and the "interview sample," respectively.

As can be seen from the table, a wide variety of variables play a role in explaining why the coefficient on Tulsa Remote approval changes when controls are added. Some of the differences are explained by various "missing variable" dummies, which are hard to interpret.

The most consistent finding that makes sense is that some part of the difference is due to two variables: the applicant knows someone in Tulsa; the applicant previously lived in Tulsa. In the full sample, the sum of these two variables' contribution to omitted variable bias is a little under 30 percent (0.00818 out of 0.02769). In the interview sample, these two variables' contribution to omitted variable bias sums to a little under 25 percent of the total (0.00654 out of 0.02667).

For both of these variables, the coefficients reveal the following: on average, Tulsa Remote is more likely to approve an applicant who knows someone in Tulsa or who previously

lived in Tulsa, and such persons are also more likely to move to Tulsa. Therefore, the restricted regression that drops all controls is positively biased by dropping such control variables.

Table B1 Calculations of Components of Omitted Variable Bias, Big Sample

	Regressions of RHS variable on "Approved by Tulsa Remote" variable		Coefficient in original full regression	Product of coefficients	Category to sum	Sum
	Coefficient	Standard Error				
Age_group==18–24	0.00171	(0.00244)	0.00546	0.00001		
Age_group==25–34	0.10652	(0.00737)	0.00976	0.00104		
Age_group==45–54	0.01980	(0.00435)	0.00167	0.00003		
Age_group==55–88	0.01065	(0.00324)	0.00928	0.00010		
Age_group==missing	−0.21314	(0.00917)	−0.02135	0.00455	age sum	0.00573
Female==female	0.26005	(0.00893)	0.01261	0.00328		
Female==missing	−0.59706	(0.00630)	0.00201	−0.00120	gender sum	0.00208
Race==white or Caucasian	0.30541	(0.00878)	0.02851	0.00871		
Race==missing	−0.49109	(0.00793)	0.02217	−0.01089	race sum	−0.00218
Educ==none	−0.01117	(0.00088)	−0.00627	0.00007		
Educ==associate	−0.03875	(0.00488)	−0.00108	0.00004		
Educ==bachelor	0.14551	(0.00926)	0.00570	0.00083		
Educ==master/doctor	0.17722	(0.00837)	0.00437	0.00078		
Educ==missing	−0.02735	(0.00188)	0.01413	−0.00039	educ sum	0.00133
Ind_cat==Computers	0.09096	(0.00621)	−0.00453	−0.00041		
Ind_cat==Health Related	−0.00078	(0.00489)	−0.00156	0.00000		
Ind_cat==Finance/bank/ real estate	0.00608	(0.00580)	0.00604	0.00004		
Ind_cat==Retail/ wholesale/other goods and services	−0.05330	(0.00426)	−0.00433	0.00023		
Ind_cat==Other	0.06875	(0.00858)	0.00481	0.00033		
Ind_cat==missing	−0.16616	(0.00585)	−0.02183	0.00363	ind sum	0.00382
Income==less than $40k	−0.21040	(0.00344)	−0.00451	0.00095		
Income==$60k–$100k	0.24671	(0.00870)	0.01591	0.00393		
Income==$100K+	0.23096	(0.00807)	0.01273	0.00294		
Income==missing	−0.21697	(0.00761)	−0.02783	0.00604	inc sum	0.01385
Empl_cat==business owner/Self-employed/ small business owner	−0.21020	(0.00612)	−0.00991	0.00208		
Empl_cat==Full-time remote/can be remote/ independent contractor	0.02030	(0.00814)	−0.01490	−0.00030		
Empl_cat==missing	0.09564	(0.00735)	−0.03960	−0.00379	empcat sum	−0.00201
KnowTulsa==false	−0.14887	(0.00893)	−0.03306	0.00492	Know someone/ previously lived in Tulsa	0.00818
Reloc_plan==was considering moving to Tulsa	0.04522	(0.00544)	0.00824	0.00037		
Reloc_plan==was planning on moving but not to Tulsa	0.04043	(0.00584)	−0.00114	−0.00005		
Reloc_plan==missing	−0.09179	(0.00787)	0.05290	−0.00486	relocplan	−0.00453

Table B1 (Continued)

	Regressions of RHS variable on "Approved by Tulsa Remote" variable		Coefficient in original full regression	Product of coefficients	Category to sum	Sum
	Coefficient	Standard Error				
Region==Midwest	−0.03436	(0.00601)	0.00589	−0.00020		
Region==Northeast	0.00731	(0.00553)	−0.00568	−0.00004		
Region==South Atlantic states	−0.00983	(0.00824)	0.00079	−0.00001		
Region==western states	0.04714	(0.00866)	0.00241	0.00011	regionsum	−0.00014
LivedTulsa==false	−0.04719	(0.00544)	−0.06899	0.00326		
Address_yrs==less than 1 year	0.03546	(0.00588)	−0.00336	−0.00012	address sum	0.00155
Address_yrs==6–10 years	0.01433	(0.00602)	−0.00005	0.00000		
Address_yrs==more than 10 years	−0.10923	(0.00753)	−0.01687	0.00184		
Address_yrs==missing	−0.01313	(0.00787)	0.01294	−0.00017		
			Sum of products	0.02769		0.02769
			Coeff on "approved" without controls	0.11855		0.11855
			Coeff on "approved" with controls	0.09086		0.09086
			difference	0.02769		0.02769

NOTES: First two columns of numbers show, in each row, estimates of regressing that RHS variable on "approved by Tulsa Remote" variable, with coefficient and standard error. The coefficient in original full regression is in next column. The product of the two coefficients is in next column. Sums of these products are reported in the far right column, for groups of variables that fit together logically. Each group of categorical variables arbitrarily omits one category, the one that is omitted from the full regression, that is with coefficient set equal to zero, to avoid perfect multicollinearity.
SOURCE: Author's estimates and calculations.

Table B2 Calculations of Components of Omitted Variable Bias, Interview Sample

	Regressions of RHS variable on "Approved by Tulsa Remote" variable		Coefficient in original full regression	Product of coefficients	Category to sum	Sum
	Coefficient	Standard error				
Age_group==18–24	−0.00958	(0.00317)	−0.00476	0.00005		
Age_group==25–34	−0.00853	(0.00906)	0.00768	−0.00007		
Age_group==45–54	−0.03100	(0.00574)	−0.00692	0.00021		
Age_group==55–88	−0.01455	(0.00425)	0.00768	−0.00011		
Age_group==missing	0.08578	(0.01099)	0.00491	0.00042	age sum	0.00050
Female==female	0.05609	(0.01083)	0.01833	0.00103		
Female==missing	−0.21772	(0.00838)	0.00606	−0.00132	gender sum	−0.00029
Race==white or Caucasian	0.14482	(0.01051)	0.02770	0.00401		
Race==missing	−0.14143	(0.00983)	0.00585	−0.00083	race sum	0.00318
Educ==none	−0.00506	(0.00115)	−0.02660	0.00013		
Educ==associate	−0.06936	(0.00645)	−0.00943	0.00065		
Educ==bachelor	0.08069	(0.01095)	0.00276	0.00022		
Educ==master/doctor	0.12773	(0.00963)	0.00522	0.00067		
Educ==missing	−0.00212	(0.00183)	−0.00111	0.00000	educ sum	0.00168
Ind_cat==computers	0.06164	(0.00708)	−0.00421	−0.00026		
Ind_cat==health related	−0.00879	(0.00595)	0.00227	−0.00002		
Ind_cat==finance/bank/ real estate	−0.03181	(0.00720)	0.00433	−0.00014		
Ind_cat==retail/wholesale/ other goods & services	−0.02327	(0.00506)	−0.01222	0.00028		
Ind_cat==other	0.03745	(0.01010)	0.00472	0.00018		
Ind_cat==missing	−0.07594	(0.00707)	−0.04103	0.00312	ind sum	0.00316
Income==less than $40k	−0.13781	(0.00510)	0.00976	−0.00135		
Income==$60k–$100k	0.10949	(0.01037)	0.01806	0.00198		
Income==$100K+	0.11831	(0.00949)	0.01250	0.00148		
Income==missing	0.01825	(0.00849)	−0.06325	−0.00115	inc sum	0.00096
Empl_cat==business owner/self-employed/small business owner	−0.03423	(0.00689)	−0.02154	0.00074		
Empl_cat==full-time remote/can be remote/independent contractor	−0.02436	(0.00977)	−0.02283	0.00056		
Empl_cat==missing	0.15465	(0.00754)	−0.05220	−0.00807	empcat sum	−0.00678
KnowTulsa==false	−0.10508	(0.01040)	−0.03446	0.00362	Know someone/ previously lived in Tulsa	0.00654
Reloc_plan==was considering moving to Tulsa	−0.07678	(0.00740)	0.00778	−0.00060		
Reloc_plan==was planning on moving but not to Tulsa	−0.13846	(0.00819)	−0.00159	0.00022		
Reloc_plan==missing	0.26043	(0.01015)	0.05627	0.01465	relocplan	0.01428
Region==Midwest	−0.01382	(0.00711)	0.01478	−0.00020		
region==Northeast	0.00854	(0.00646)	−0.01957	−0.00017		
region==South Atlantic states	−0.00924	(0.00975)	−0.00098	0.00001		

Table B2 (Continued)

	Regressions of RHS variable on "Approved by Tulsa Remote" variable		Coefficient in original full regression	Product of coefficients	Category to sum	Sum
	Coefficient	Standard error				
Region==western states	0.03013	(0.01017)	−0.00033	−0.00001	regionsum	−0.00037
LivedTulsa==false	−0.03575	(0.00623)	−0.08177	0.00292		
Address_yrs==less than 1 year	−0.04356	(0.00744)	−0.00797	0.00035	address sum	0.00381
Address_yrs==6–10 years	0.00138	(0.00721)	−0.00484	−0.00001		
Address_yrs==more than 10 years	−0.00778	(0.00874)	−0.03665	0.00029		
Address_yrs==missing	0.09597	(0.00855)	0.03315	0.00318		
			Sum of products	0.02667		0.02667
			Coeff on "approved" without controls	0.11168		0.11168
			Coeff on "approved" with controls	0.08501		0.08501
			difference	0.02667		0.02667

NOTES: First two columns of numbers show, in each row, estimates of regressing that RHS variable on "approved by Tulsa Remote" variable, with coefficient and standard error. The coefficient in original full regression is in next column. The product of the two coefficients is in next column. Sums of these products are reported in the far right column, for groups of variables that fit together logically. Each group of categorical variables omits one category, the category that is dropped from the original full regressions (or set equal to zero), to avoid perfect multicollinearity.
SOURCE: Author's estimates and calculations.

APPENDIX C: LOGIT ESTIMATES

This appendix reports the basic logit estimates for the "big sample" and the "interview sample." Overall, these estimates are similar to the linear probability model estimates.

Tables C1 and C2 report these coefficient estimates, standard errors, and the probabilities of the t-statistics for different groups of categorical variables. As with the linear probability models, the probabilities of a given set of categorical variables are reported both for the categories including the missing category and dropping the missing category.

Table C1 Logit Estimates of Determinants of Probability of Moving to Tulsa, Big Sample

Variable	Coefficient	Standard error	Probability for that group of variables being significant, missing excluded	Missing included
Approved by Tulsa Remote	1.2977	(0.1140)	<0.001	<0.001
RECODE of age			0.5341	0.6752
18–24	0.1163	(0.3199)		
25–34	0.2428	(0.1432)		
35–44	0	0.0000		
45–54	0.1021	(0.1933)		
55–88	0.2325	(0.2341)		
missing	0.09	(0.3625)		
Female			0.0645	0.0338
female	0.1861	(0.1006)		
male or non-binary	0	0.0000		
missing	−0.2226	(0.1833)		
Race			0.0007	0.0025
white or Caucasian	0.3793	(0.1113)		
non-white	0	0.0000		
missing	0.1273	(0.1935)		
Education			0.0794	0.0915
none	−0.7372	(0.7229)		
high school	0	0.0000		
associate	0.1429	(0.1662)		
bachelor	0.2898	(0.1261)		
master/doctor	0.255	(0.1423)		
missing	0.7033	(0.3171)		

Table C1 (Continued)

Variable	Coefficient	Standard error	Probability for that group of variables being significant, missing excluded	Missing included
Ind_cat			0.2869	<0.0001
computers	−0.0665	(0.1623)		
health related	−0.0311	(0.1661)		
finance/bank/real estate	0.1646	(0.1422)		
management and PR	0			
retail/wholesale/other goods and services	−0.244	0.0000 (0.1995)		
other	0.1236	(0.1158)		
missing	−0.8502	(0.1974)		
Income			0.0023	<0.0001
less than $40k	−0.3715	(0.1912)		
$40k–$60k	0	0.0000		
$60k–$100k	0.3105	(0.1304)		
$100K+	0.1777	(0.1446)		
missing	−0.7116	(0.1910)		
Empl_cat			0.2173	0.0415
business owner/self-employed/small business owner	−0.2742	(0.1572)		
full-time remote/can be remote/independent contractor	−0.1603	(0.1871)		
full time employee	0	0.0000		
missing	−0.5948	(0.2470)		
KnowTulsa			<0.0001	<0.0001
false	−0.8108	(0.0957)		
true	0	0.0000		
Reloc_plan			0.7404	0.0847
was considering moving to Tulsa	0.2037	(0.3020)		
was planning on moving but not to Tulsa	0.0898	(0.2995)		
wasn't planning on moving anywhere	0	0.0000		
missing	0.9592	(0.4226)		
Region			0.219	0.219
Midwest	0.1911	(0.1423)		
Northeast	−0.2106	(0.1848)		
reference states	0	0.0000		
South Atlantic states	−0.0319	(0.1213)		
Western states	0.0951	(0.1133)		
LivedTulsa			<0.0001	<0.0001
false	−0.8684	(0.1223)		
true	0	0.0000		
Address_yrs			<0.0001	<0.0001
less than 1 year	−0.092	(0.1509)		
1–5 years	0	0.0000		
6–10 years	−0.0399	(0.1353)		
more than 10 years	−0.8005	(0.1375)		
missing	0.1796	(0.2744)		
Intercept	−3.1440	(0.3527)		
Number of observations	29,003			

Table C2 Logit Estimates of Determinants of Probability of Moving to Tulsa, Interview Sample

Variable	Coefficient	Standard error	Probability for that group of variables being significant, missing excluded	Missing included
Approved by Tulsa Remote	1.3655	(0.1370)	<0.0001	<0.0001
RECODE of age			0.6178	0.7534
18–24	−0.1499	(0.3855)		
25–34	0.1749	(0.1509)		
35–44	0	0.0000		
45–54	−0.0716	(0.2131)		
55–88	0.1705	(0.2660)		
missing	0.0562	(0.3918)		
Female			0.0151	0.0329
female	0.2541	(0.1045)		
male or non-binary	0	0.0000		
missing	−0.0534	(0.1955)		
Race			0.0009	0.0027
white or Caucasian	0.3924	(0.1180)		
non-white	0	0.0000		
missing	0.0406	(0.2107)		
Educ1			0.7215	0.8219
high school or less	0	0.0000		
associate	−0.1025	(0.2087)		
bachelor	0.0841	(0.1519)		
master/doctor	0.0923	(0.1673)		
missing	−0.2563	(0.7573)		
Ind_cat			0.6028	0.013
computers	−0.0528	(0.1720)		
health related	0.0438	(0.1868)		
finance/bank/real estate	0.0686	(0.1633)		
management and PR	0	0.0000		
retail/wholesale/ other goods and services	−0.3838	(0.2628)		
other	0.0705	(0.1299)		
missing	−0.7583	(0.2352)		
Income			0.1353	<0.0001
less than $40k	−0.1198	(0.3123)		
$40k–$60k	0	0.0000		
$60k–$100k	0.3159	(0.1486)		
$100K+	0.2196	(0.1606)		
missing	−0.8447	(0.2440)		
Empl_cat			0.3156	0.0501
business owner/self-employed/small business owner	−0.3123	(0.2063)		
full-time remote/can be remote/independent contractor	−0.1935	(0.2142)		
full time employee	0	0.0000		
missing	−0.6268	(0.2700)		
KnowTulsa			<0.0001	<0.0001
false	−0.5315	(0.1073)		
true	0	0.0000		

Table C2 (Continued)

Variable	Coefficient	Standard error	Probability for that group of variables being significant, missing excluded	Missing included
Reloc_plan			0.4006	0.0922
was considering moving to Tulsa	0.2702	(0.2973)		
was planning on moving but not to Tulsa	0.0286	(0.2947)		
wasn't planning on moving anywhere	0	0.0000		
missing	1.0083	(0.4754)		
Region			0.0524	0.0524
Midwest	0.2568	(0.1643)		
Northeast	−0.4255	(0.2158)		
reference states	0	0.0000		
South Atlantic states	−0.0451	(0.1377)		
Western states	0.016	(0.1302)		
LivedTulsa			<0.0001	<0.0001
false	−0.7517	(0.1413)		
true	0	0.0000		
Address_yrs			<0.0001	<0.0001
less than 1 year	−0.1412	(0.1597)		
1–5 years	0	0.0000		
6–10 years	−0.1393	(0.1535)		
more than 10 years	−0.7995	(0.1630)		
missing	0.1296	(0.3985)		
Intercept	−3.1715	(0.3791)		
Number of observations	11,517			

NOTES: Coefficients and standard errors (in parentheses) are in first two columns. Probabilities of F-tests for excluding particular groups of variables are in two rightmost columns.

SOURCE: Author's estimates.

The coefficient estimates can be interpreted with just a little more work, compared to the linear probability model. The linear probability model implies a model of the following form:

(C1) $p = B_0 + B_x*X$

Where p is the probability, in this case of moving to Tulsa, X is a vector of predictors, and B_x is the vector of coefficients on those predictors.

In such a model, the effect of a one-unit change in X—which is relevant for a categorical variable—is simply given by the specific coefficient B_x on that variable.

The logit model specifies the dependent variable as the natural log of the odds:

(C2) $\ln(p/(1-p)) = B_0 + B_x*X$

In such a model, a one-unit change in X has a fixed effect on the log of the odds of moving to Tulsa. The effect on the probability depends on the starting point.

To see how this can be used, suppose our starting point in the big sample is the average probability of moving to Tulsa of those who were approved for the program, which is 13.71 percent. The corresponding log of the odds is −1.8394. Suppose that we are considering someone approved for the program, and imagine the effects of changing approval from a value of 1 to zero—they are not approved. This multiplied (−1) times the coefficient on the approval dummy, so subtracting −1.2977. We end up with a new predicted log of the odds of −3.1371. This corresponds to a probability of moving to Tulsa of 4.16 percent. The moving probability has been reduced by 9.55 percent. This implied "but for" for this observation would be 69.7 percent (= 9.55 / 13.71).

Similar calculations can be done for other categorical variables. We can either start with the mean probability in the "treated sample," or start with the mean probability in the "untreated sample," or indeed any other plausible value that might be observed.

APPENDIX D: MORE DETAIL ON SOURCES FOR THE ASSUMPTIONS MADE IN MODELLING BENEFITS VERSUS COSTS OF TULSA REMOTE

This appendix gives more details on the assumptions made in modelling the plausible "economic development benefits" of Tulsa Remote. These assumptions were summarized in Table 10.

Retention of Tulsa Remote Members

Through year 5, I use three sources of information. First, I use information from Tulsa Remote on what percentage of members actually are still in Tulsa after one year. Second, I use survey information provided to me by Tulsa Remote on what percentage of Tulsa Remote members are still in Tulsa as of 2 years after they arrived. Third, I use Newman, O'Dell, and Fikri (2021) survey information from June 2021 on what percentage of Tulsa Remote members expect to still be in Tulsa in 5 years.

According to Tulsa Remote staff, the one-year completion rate in 2024 was 95.5 percent. Based on that statistic, I assume the average percentage of Tulsa Remote members in Tulsa during year 1 is the average of 100 percent (at beginning of year) and 95.5 percent (at the end), for a first-year average of 97.75 percent.

As for the two-year survey, Tulsa Remote surveys have 75.6 percent reporting they are still in Tulsa as of exactly two years after they arrived. However, the response rate for this survey is 25 percent. It seems likely that those who have left Tulsa are less likely to respond. For the middle retention scenario, I assume that the proportion remaining as of two years after arrival is 5 percentage points less, at 70.6 percent. For the high-retention scenario, I assume 75.6 percent remain. For the low-retention scenario, I assume 65.6 percent remain.

I then interpolate by assuming that the retention rate goes down at the same rate from the end of year 1 until the average of year 2 until the end of year 2, so that the decline from the end of year 1 until the end of year 2 is the retention rate from the end of year 1 to the average of year 2, but squared. The average retention rate for year 2 ends up being 82.1 percent for the middle-retention scenario, 85.0 percent for the high-retention scenario, and 79.2 percent for the low retention scenario.

Newman, O'Dell, and Fikri (2021) report survey results that as of June 2021, Tulsa Remote members say that 43 percent say they are moderately or extremely likely to stay for five years more, and an additional 12 percent say they are slightly likely to stay for five years more, with the rounded sum being 56 percent. I assume that the average respondent has been in Tulsa for one year, so these responses on average are projecting to the end of year 6. For the middle retention scenario, I assume that 49 percent (=43 plus one-half of 12 percent) of those there as of the end of year 1 will still be there as of the end of year 6. For the low retention rate scenario, I assume that 43 percent will still be there as of the end of year 6. For the high retention scenario, I assume 56 percent will still be there. I then assume that the combined retention rate from the end of year 2 to the end of year 6 that is implied by that calculation results from multiplying together the product of half-year to half-year retention rates that are the same for each half-year from the end of year 2 to the end of year 6. I use the half-year implied retention rates for years 2, 3, 4, 5 and 6 as the averages for the entire year.

For the middle- and low-retention rate scenarios, I assume these half-year to half-year retention rates from the end of year 2 until the end of year 6 then continue until halfway through year 10. I then assume that the cumulative retention rate as of year 10 does not change after year 10. As mentioned in the text, the implicit assumption is that after this date, even though some

Tulsa Remote members may leave, other former members will return. In addition, Tulsa Remote members may attract other remote workers and other college graduates to Tulsa. For the high-retention rate scenario, I assume that the cumulative retention rate as of year 7, which is 51.2 percent, continues for all subsequent years.

Ratio of Tulsa Remote Household Workers to Members: 1.406

The survey reported in Newman, O'Dell, and Fikri (2021) had responses from 508 Tulsa Remote households, who had 206 additional adult workers in those households. The ratio of total adult workers in Tulsa Remote households to Tulsa Remote members is 1.406 = (508 + 206) / 508.

Multiplier for Member Household Jobs: 1.41

Tulsa Remote (2024) reported that 2,252 jobs held by members yielded a total of 3,554 jobs in Tulsa, for a multiplier of 1.58 (= 3554 / 2252). But the model needs a multiplier for total jobs in member households. I assume a multiplier of 1 for the population shock to other household members, assuming they do not bring along remote employment. This multiplier of 1 for a supply shock is based on the estimates in Howard (2020), which are consistent with a broader research literature showing close to one-for- one employment effects of migration shocks (see Table 2 of Bartik 2019c). The multiplier used of 1.41 is then a weighted average: [1.58*508 + 1.00*206] / (508+206). If we instead used the 1.50 multiplier of member jobs from Newman, O'Dell, and Fikri (2021), the resulting weighted multiplier would be lower at 1.36. But some of the other working adults in member households may also be working remotely, which increases the multiplier. If the effective multiplier for other working adults in member households is 1.18, then the weighted average would once again be 1.41. At any event, lowering

the multiplier slightly to 1.36 does not qualitatively change the results. The overall benefit-cost ratio would be 3.73, rather than the baseline benefit-cost ratio, with a multiplier of 1.41, of 4.31.

However, note that some significant multiplier greater than 1 is crucial. If one takes the baseline scenario and lowers the multiplier to 1, the benefit-cost ratio declines from 4.31 to MINUS 0.62. In this case, the program almost immediately increases local costs due to the population growth driving up housing prices. In that case, the program on net drives out jobs, other than the jobs held by Tulsa Remote members, from the Tulsa economy, and this causes significantly lower employment rates for the original Tulsa residents. All of this is avoided with a reasonable multiplier figure that exceeds 1. Multipliers are indeed important.

Note that the multiplier interacts with the housing cost variable. If we modify the scenario by having a multiplier of 1, but with housing supply being elastic enough that housing prices and overall prices do not increase, the benefit-cost ratio goes back up to 2.11.

Wage Premium on Tulsa Remote Direct Jobs for Member Households: Zero in baseline

This is derived from Tulsa Remote's figures for wages and salaries of Tulsa Remote members, and Newman, O'Dell and Fikri (2021) figures on total compensation of other working Tulsa Remote household members. Newman et al (2021) report average compensation for other working household members of $34,759 ($6,500,000 ÷ 187). But it is clear that the latter figure is an under-estimate, as no figure is reported for additional household income, beyond member income, for one-quarter of these 187 extra workers. I assume conservatively that this undercount is 25 percent. (If the undercount was random, one-quarter missing would inflate the reported numbers by one-third, but probably some of the missing numbers are lower.) Therefore, I assume average compensation for other working household members that is one quarter higher at

$43,449. Adjusting to the 2019 national prices used in the model, the average compensation for other working household members is $44,705.

Tulsa Remote figures for straight wage and salaries of Tulsa Remote members, in 2019 national prices, averaged $87,869 from 2020 to 2024. According to BLS statistics for December of 2025, average benefits in private sector jobs were 41.9 percent of the straight wages and salaries. If we apply this benefit rate to Tulsa Remote members, and assume that for every 1000 Tulsa Remote members there are 406 additional adults in the member household, then the average compensation of persons in Tulsa Remote households would be $101,618. The model's default is to assume that directly created jobs pay the national average for export-base jobs, which is $98,154. The $101,618 figure is 3.5 percent above this default. This implies a 3.5 percent wage premium. However, we don't actually know the benefit rate for Tulsa Remote members. It might be less than 41.9 percent of straight wages, it might be more. Therefore, for the baseline model, I assumed a wage premium of zero. However, a 3.5 percent wage premium or even a 10 percent wage premium are also plausible numbers.

Cost of Tulsa Remote: $15,425/adjusted member

Tulsa Remote reports that over the last three years, Tulsa Remote's total costs have been $15,425 per "adjusted member," where each first-year member is weighted by the number of months they were in Tulsa. According to Tulsa Remote, few if any costs of Tulsa Remote are due to members beyond the first year.

For the cost calculations, this cost per adjusted member is calculated by multiplying the number of members by the average proportion of the first year they remain. Based on 95.5 percent lasting the full year, I assume the average months during the first year if the average of 95.5 percent and 100 percent, or 97.75 percent. The model's calculations are done by assuming

1000 members initially in 2023, with the benefits and costs later translated into per member terms. Therefore, first year costs of the program are assumed to be the product of $15,425 times 1000 members times 97.75 percent = $15,087,938.

Demand Effects of Tulsa Remote Spending Assumed to Equal 68.3 Percent of Model's Usual Public Service Costs

The model assumes that public spending creates jobs both directly and due to multiplier effects. The $10,000 that is paid as a transfer to Tulsa Remote members is assumed to only have about half of this normal effect, because a higher proportion will be spent on non-local goods. If we assume about 95.5 percent of Tulsa Remote members receive stipends, 955 out of 1,000, total stipends paid will be 95.5 percent times 1000 or $9,550,000. The remaining $5,527,938 will be straight public services. A weighted average of 50 percent public service effects for the $9.55 million, and 100 percent for the remaining $5.5 million plus, yields an average of 68.3 percent.

Supply-Side Effects of Tulsa Remote: 0.57 Long-Run Jobs per College Graduate in Tulsa Remote Households, with Tulsa Economy Adjusting to Long-Run at 9 Percent per Year. 80 Percent of Tulsa Remote Members Assumed to Be College Graduates (BA or More)

The 80 percent comes from the database used in this report. Other sources report slightly higher percentages. For example, Newman, O'Dell and Fikri (2021) report 88 percent bachelor's or higher. The 0.57 jobs per college graduate was a long-run figure previously calculated in Bartik (2018b, p. 92). This is derived from estimates of how an area's college graduate percentage affects its job growth, which was estimated in Bartik (2018b). Glaeser and Saiz (2004) got similar effects of college grads on growth. The adjustment rate of 9 percent per year is the general adjustment rate used in the model, and is derived originally from Helms (1985). The adjustment rate directly estimated in Bartik (2018b) is slightly higher at 11 percent per year. The

college graduate “effect” is calculated based on how many added college graduates the program adds to Tulsa as of year 10, when the retention rate is assumed to stabilize.

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