

Does Universal Basic Income Decrease Labour Force Participation?

Introduction

This paper incorporates experimental research from a wide variety of sources to best understand the impact of Universal Basic Income (UBI)¹ on labour force participation. This question is important to economists because UBI appears to be an effective way of increasing social welfare, but it may have drawbacks such as decreased labour force participation. Although economists are unsure of UBI's impact, voters (and therefore politicians) are becoming increasingly comfortable with the idea – a 2019 poll found that 75% of Canadians are in favour of UBI. Notably, governments' UBI-like responses to COVID-19 (e.g. CERB) reached over 1.3 billion people worldwide, which may have shifted public opinion even more in the past year (Gentilini et al., 2020).

Although UBI is an old concept, it has never been fully implemented,² so our knowledge of its effects is mainly limited to conclusions drawn from carefully designed experiments (with participants usually numbered in the thousands), theoretical modelling, or unconditional transfers on a much smaller scale. Since these sources of information are not fully representative of UBI, there is not a consensus (academic or otherwise) on UBI's impacts on the labour force. In addition, UBI will have unique effects depending on the labour market and economic

¹ Throughout this paper, I will use Van Parijs' widely adopted definition of UBI: "a Basic Income is an income paid by a political community to all its members on an individual basis, without means test or work requirement." (Báñez et al., 2020)

² Specifically, there has not been a UBI policy which is unconditional, permanent, universal, and substantial enough to support individuals.

environment it is implemented in, and different effects depending on how it is implemented.

Therefore, attempting to make one sweeping generalization is neither appropriate nor helpful.

Before beginning, it is important to clarify that my analysis aims only to understand whether UBI affects labour supply, *not* whether any change in labour supply would be good or bad for a society – that question is beyond the scope of this paper.

Institutional Framework

The closest to a true implementation of UBI was Iran's Subsidy Reform Plan (طرح هدفمندسازی یارانه‌ها), which was enacted in 2010. As such, my review focuses disproportionately on it and its analysis. The Plan created cash transfers covering over 70 million Iranians, or roughly 95% of the population (Salehi-Isfahani and Mostafavi-Dehzoeei, 2018). Each of the monthly transfers was 28% of median per capita household income, and in 2011, total transfers amounted to roughly 6.5% of GDP (Salehi-Isfahani and Mostafavi-Dehzoeei, 2018). While these monthly transfers are large and nearly universal, they were implemented at the same time that subsidies for energy, bread, and other goods were cut, making this a roughly expenditure-neutral policy (Farzin and Guillaume, 2011). As such, the economic effects may have limited externally validity.³ The funds also come from national oil wealth, not taxation, which further limits the event's external validity.

On the other side of the globe, the Permanent Fund Dividend of Alaska is an annual payment to all Alaskan adults. It is paid from the Alaska Permanent Fund, which was established in 1976 as

³ While many UBI proposals are expenditure-neutral, few aim to (or are able to) achieve neutrality solely through cutting subsidies.

a way to redistribute the wealth of the Alaskan region to all citizens (Alaska Permanent Fund, n.d.). In 2020 it paid a US \$992 dividend to all adults (Wojtusik, 2020), or roughly 2.6% of median per capita income (U.S. Census Bureau, 2021). This program has received plenty of attention in the literature – Marinescu and Jones (2018), Feinberg and Kuehn (2020), and Dorsett (2021) have recently published research on the Permanent Fund’s societal impact.

The specific details of UBI implementation will largely determine its impacts. A truly *universal* basic income (i.e., an unconditional transfer to all citizens) will not feature cutoffs below which citizens do not receive transfers, but the effects of a cutoff-less program would be nearly identical to one with a sufficiently high income cutoff. Compared to a basic income or a guaranteed minimum income that only transfer cash to those deemed to specifically need it, UBI may create less of a “welfare trap” – a system which incentivizes those receiving the benefits to not participate in the labour market – because accepting a formal job offer would not mean that you receive a smaller transfer. Imposing cash transfer requirements would also plausibly distort the labour market even more by incentivizing employers to pay just below the cutoff, or lower than they would have if the cutoff were a gradual income gradient.

UBI’s implementation would also depend on the economic environment. Large cash transfers on this scale are expensive, and some states (especially developing economies with limited capital and natural resources) simply do not have the capacity or infrastructure to enact them. There are also concerns that large cash transfers could cause inflation. In fact, Japan’s recently formed Reiwa Shinsengumi political party proposed monthly payments of ¥30,000 (roughly US \$270) to each adult individual, but only during periods in which the 2% inflation rate target is not met – in other words, they wish to implement UBI in order to increase inflation (Reiwa Shinsengumi, n.d.).

The wealth of a country does not only determine the feasibility of UBI – it may also determine its impact. For example, Ghatak and Maniquet (2019) modelled the impacts of UBI and concluded that it would be an effective anti-poverty policy in developing nations, but not in developed ones.

UBI's impact would likely also be heavily dependent on how the requisite funds are raised. Many public proposals (such as Yang, 2020 and UBIWorks.ca, 2021) aim to raise said funds by cutting tax deductions for businesses and the wealthy, increasing certain (popular) taxes such as the inheritance tax, cutting funds to social programs which UBI would replace, and/or eliminating certain subsidies (although there are many other ideas which are not listed here). Each one of these proposed changes could affect labour demand through multiple channels (i.e. changes in business cycle characteristics, changes in the marginal cost of employees, etc.), which could in turn influence labour supply.

In summary, UBI's impact may strongly depend on the details of how it is implemented, which makes it difficult to predict its effects. Nonetheless, theorists have attempted to do so, and we review some of their findings in the next section.

Economic Predictions

While several models have been used to analyze the potential impact of UBI on the labour market, there is no clear winner. The simplest and most widely used model is the Neo-Classical Labour Supply Model, with the usual assumptions of downward-sloping labour demand, upward-sloping labour supply, an income leisure tradeoff, one representative price-taker firm,

one representative worker, and perfect competition (Borjas, 2015). If UBI is “properly” enacted,⁴ then under this Neo-Classical model, UBI will make workers wealthier, allowing them to afford more leisure (and less work).⁵

In reality, this model’s analysis contains several assumptions which will not hold. First is perfect information, which is broken via workers’ beliefs in the permanence of UBI. Like all ongoing public policies, UBI would be subject to the political zeitgeist of the times, so workers would probably not be confident that the transfers would continue over their entire lifetimes. This would cause the income effect to have a smaller negative impact on the supply of hours worked (depending on how short-lived workers perceive UBI to be), which could even result in no decrease in the supply of hours worked.⁶

Another assumption of the Neo-Classical model which will not hold is that of one representative firm. Many workers across the world are self-employed and/or entrepreneurs, and UBI would affect them differently than it would affect workers at large organizations. In the developing world, UBI would effectively provide a line of credit which was previously unavailable to the poor, thereby allowing self-employed workers to invest in themselves and their businesses. In the developed world, UBI would decrease the opportunity costs of entrepreneurship by providing a safety net in case of failure.

Finally, the Neo-Classical assumption of perfect competition does not hold, and this too will distance UBI’s actual effects from its predicted effects. Real labour markets often feature

⁴ I.e., if inflation does not “cancel out” the cash transfer, and most workers have more PPP-adjusted income than they had pre-UBI; and if workers believe that UBI will be in place permanently.

⁵ See: Figure 1. This is in the specific case of a \$1,000 cash grant to workers, and a 50% labor-earnings tax.

⁶ This may have been the case in Iran’s Subsidy Reform Plan – in 2013, two years after the Plan was enacted, President Rouhani succeeded President Ahmadinejad, and in 2018 he gutted the cash transfer program in the pursuit of budget austerity (Owliaei, 2018).

monopsony or oligopsony (Steinbaum, 2018; Staiger et al. 2010), in which case employers can use their disproportionate hiring power to their advantage. Monopsony informs one (popular) argument for UBI, which is that it would give workers the ability to refuse a job offer (Varoufakis, 2016).

It is worth noting that with the Neo-Classical model, as with others, the effect on hours supplied will depend on how the funds are raised. For example, an income tax that affects most workers would lead to a larger decrease in hours supplied in comparison to an income tax levied only on the wealthy (Borjas, 2015).

The Neo-Classical Model is far from the only theoretical approach to understanding UBI's impact, but study after study compares their findings to it, since Neo-Classical thinking still dominates the field of economics. Therefore, I will use it as the theoretical benchmark for when reviewing empirical results.

Review of Findings

Before summarizing the findings of any papers, it is important to acknowledge the empirical issues they may face. First, there is strongly limited external validity. To begin with, most societies today are deeply centred around working – most capable adults work and are expected to work. Implementing a UBI (or an experiment) in a society today would therefore only tell us the effect of UBI on societies where everybody expects to work, but it is possible that after several generations of living with UBI, norms and perceptions (and therefore labour force participation) will change in unforeseen ways.

Another serious caveat in any study is that UBI may push workers to substitute paid work in the formal labour market for unpaid work at home or elsewhere. This would masquerade as a decrease in labour supply in most quantitative analyses, even though there may actually be positive labour supply effects (depending on how one defines “labour”).

Experimental studies in particular have severe external validity concerns – they will be subject to the Hawthorne effect and the streetlight effect (Widerquist 2018), and it is easy to “manipulate the data to say what we expect... and convince public opinion of that result.” (Báñez et al., 2020) They are also, by definition, not universal, since not everybody is included in the experiment, and they often focus on a specific subset of the population (e.g. the RCT in Gary, Indiana, which focused on Black families (Moffitt, 1979)). As a study becomes more selective about who receives funds, it will less accurately assess the spillover effects of an entire community receiving UBI (differences could arise from stigma, network effects, labour demand shifts, and other changes). UBI field experiments are also usually temporary (Báñez et al., 2020), which has its own host of problems. If a study fails to convince subjects that it will continue for many years or decades, perceptions will cause the income effect to obscure the results, and the study will end up estimating the impacts of short-term cash transfers (even if the policy *is* actually enacted for many years or decades). Due to these limitations, I believe that field experiments are less informative than observations from the real world, so my analysis focuses on the latter. Of the real-world analyses, two stand out.

Iran

As Iran’s Subsidy Reform Plan may be the closest to a true implementation of UBI, it may be the best empirical evidence available. According to Salehi-Isfahani and Mostafavi-Dehzoeei (2018), Iran’s Subsidy Reform Plan had positive or statistically insignificant negative impacts on the

supply of hours worked, across gender and income quantile. Their difference-in-difference specification⁷ uses variation in the period where transfers were received, and with it the authors find that hours worked decreased by 0.11 standard errors for men and increased by 3.06 standard errors for women. They also report similar coefficients for several variants of their original specification. One possible explanation of the positive effect for women is that cash transfers allow women to pay for childcare and/or allow elderly members of the household to stop working and take care of children while the younger women work, as Ardington et al. (2009) find in their analysis of pension transfers in South Africa.

Interestingly, Salehi-Isfahani and Mostafavi-Dehzooei find a smaller decrease in labour supply on rural workers than on their urban counterparts.^{8 9} This apparently contradicts the Neo-Classical model, which predicts that poorer individuals (rural citizens, in this case) who are receiving a higher transfer relative to their earned income would face stronger income effects and therefore be more likely to stop working (Borjas, 2015). However, it is corroborated by real-world evidence from the aforementioned Ardington et al. (2009) analysis, which exclusively examined rural households.

Salehi-Isfahani and Mostafavi-Dehzooei's fixed effects models¹⁰ (which they believe to be stronger than their difference-in-difference, due to weaker assumptions) also do not find evidence of any decrease in hours worked. In fact, they find significant positive impacts on hours worked for men.

⁷ See: Equation 1.1 and Table 1.2.

⁸ Neither negative impact is statistically significant.

⁹ See: Tables 1.6 and 1.7

¹⁰ See: Equation 1.2 and Table 1.5.

It is important to note that both of their main difference-in-difference and OLS fixed effects specifications have low R^2 values,¹¹ so receiving the cash transfer treatment is not a strong predictor of hours worked.

Since I see this paper as the most informative of UBI to date, I will go into depth in explaining its caveats:

Caveat #1: The classification of individuals into treatment vs. control groups is inferred from the amount that was transferred to them (individuals were transferred different amounts depending on the time period). While the authors use the same methodology as Salehi-Isfahani et al. (2015), who were accurate to within 5% of administrative data, any random measurement error in the treated vs. un-treated classification will result in attenuation bias (which could explain their results).

Caveat #2: As we can see in Tables 3 and 4, their control and treatment groups are not randomly assigned. For the authors' difference-in-difference estimates to be unbiased, they need to assume that assignment is random after conditioning on the characteristics they can observe. In other words, the parallel trends assumption may not hold.¹²

Caveat #3: As I noted in the “Economic Predictions” section, Iranians may have correctly predicted that the Subsidy Reform Plan would be short-lived. If this is true, then the findings of Salehi-Isfahani and Mostafavi-Dehzooei will only be externally valid for implementations of UBI programs that workers believe are temporary. On the other hand, this may actually make their results *more* externally valid in general, since workers will

¹¹ These small R^2 values are from models that include household characteristics and other relevant predictors of labour supply, so they are upper bounds on the predictive power of treatment.

¹² It is still somewhat reasonable to assume that parallel trends holds – see: Figure 2

probably believe that UBI programs will be short-lived (at least, until said program survives for some years or decades).

Caveat #4: Due to sanctions in 2011 and other substantial changes in Iran's economy, Salehi-Isfahani and Mostafavi-Dehzoeei are limited to analyzing a brief one-year period. It is entirely possible that Iran's Subsidy Reform Plan had no negative labour supply effects in the first year it was implemented, but that there are negative effects in the long-run. As mentioned in the "Economic Predictions" section, workers' perceptions of program duration can have a large effect on the income effect and labour supply, and, as mentioned in the beginning of this section, societies take time to adjust to policies.

Unfortunately, the Iran Subsidy Plan has not received the attention it deserves in the labour literature; to my knowledge, this is the only analysis of its impacts on the labour market. Hence, we now turn our attention to Alaska.

Alaska

Jones and Marinescu (2018) is the most informative analysis of Alaska's Permanent Fund's impact on employment. The Permanent Fund is similar to Iran's Subsidy Plan in that it redistributes wealth derived from natural resource extraction and is distributed to most of the adult population. However, the transfers only amount to 2.6% of Alaska's median per capita income (versus 28% in Iran), and the cash transfers have occurred each year since 1982.¹³ In sum, these transfers have every characteristic of UBI, except for their magnitude.

Jones and Marinescu estimate that the Alaska permanent fund has not affected full-time employment, and increased the part-time labour participation rate by 1.8 percentage points.

¹³ For a detailed history of payments, see: <https://pfd.alaska.gov/Division-Info/Summary-of-Applications-and-Payments>

They believe that their results are caused by the cash transfer increasing consumption, which in turn increases labour demand. They estimate that Alaska's employment to population would decrease by 0.6 percentage points if the income effect was not cancelled out by this increase in demand, and that this ratio would increase by 0.5 percentage points if the increase in demand was not cancelled out by the income effect.¹⁴

The weakest point of Jones and Marinescu's analysis is their data – since Alaska's transfers are relatively small, they use synthetic controls to construct a counterfactual for Alaska. While their synthetic Alaska follows the real one reasonably well,¹⁵ it is difficult to estimate the validity of their counterfactual.

The Iranian and Alaskan case studies both conclude that the income effect does not dominate the labour market, and so there is not evidence of a substantial decline in hours supplied. This clashes with the Neo-Classical model's predictions but does not clash with other empirical findings. For example, Marinescu's meta-analysis of unconditional cash transfers in the United States concludes that when there is an income effect on labour supply (sometimes there is no significant effect), it appears that a 10% increase in unearned income decreases labour supply by roughly 1% (Marinescu, 2018) .

Conclusion

The empirical sources that I examine (which I believe to be the best estimations available) have found little to no evidence of UBI (or UBI-like policies) decreasing the supply of labour.

¹⁴ Here, they use the estimate of the unearned income elasticity of labour supply from Cesarini et al. (2018), who study Swedish lottery winners and find small but significant elasticities.

¹⁵ See: Figure 3

However, there is nothing resembling a consensus in the literature, mostly¹⁶ because UBI is too complex to reliably model, and there is a lack of conclusive empirical evidence. It is nearly impossible to create an experiment that accurately recreates the conditions of UBI on a local scale (and large-scale experiments are unaffordable), and it is also nearly impossible to empirically analyze a program which has not occurred. This leaves us with theoretical analyses and simulations, both of which unfortunately lack the nuance to capture UBI's complex effects¹⁷ and are subject to their creators' assumptions.

Even when outcomes are clearly defined in a study's data (e.g. in experimental settings with high levels of observational precision), economists have interpreted them with opposing conclusions, creating "a political Rorschach test... [which] reveal[s] more about the observer than the observed." (Widerquist, 2005)

Another reason for the lack of consensus on implementing UBI is that there are many direct (e.g. NIT, GIS, and GBI) and indirect (e.g. job training, healthcare) alternatives which have their own merits, and may be more beneficial than UBI (Green et al., 2021).

If UBI were a cheap and easy policy to enact, these doubts might not be as important. However, enacting UBI is a massive commitment and change, so policymakers and economists need to be confident that it will be beneficial before they implement it.

The sensitivity of UBI to environmental variables combines with the lack of empirical evidence to make it unclear where it could be successfully implemented. For example, UBI appeared to have no negative impact on Iran's labour supply, but North American NIT experiments in the

¹⁶ There are also personal and political influences on economists in the UBI debate. For one, UBI is somewhat incompatible with mainstream Neoliberal economics, so accepting UBI would be awkward (Acemoglu, 2019). On the other hand, heterodox economists may be conversely motivated (Varoufakis, 2016).

¹⁷ For example, they fail to account for the entrepreneurial effects mentioned in previous sections.

mid-late 1900s found negative impacts on labour supply (Widerquist, 2005). It is unclear whether the difference in effects is due to differences between the two settings in wealth, policy mechanism, time trends, local trends, technology, capital, societal norms and beliefs, or other factors. With enough studies and evidence, we would have enough variation to tease apart the various causal factors at play, but there is far from enough evidence.

There are several key issues which need to be investigated in the future. As mentioned in the “Empirical Review” section, experimental studies are hamstrung by lack of long-term guarantees and non-universality of eligibility. Slightly lower on the list of priorities is understanding people’s perceptions. In particular, future studies need to explicitly ask subjects how long they believe they will receive transfers, since this determines the strength of the income effect. This would have the added benefit of allowing researchers to estimate the income effect across a wide range of covariates. Finally, while this is challenging, future studies should aim to measure as much non-market or informal labour as possible, since failing to take that into account will overestimate negative labour supply effects.

In general, I would not recommend implementing UBI, since our uncertainty in its effects is vastly overshadowed by the magnitude of its impacts. However, this recommendation might change depending on the specific environment. For example, developing countries have more people in poverty, so they may face a lower opportunity cost of large social changes. As another example of situational nuance, Green et al. (2021) concluded that UBI was not the optimal policy for British Columbia, and that a multitude of targeted policies should be implemented instead. If BC has the institutional strength and organization to correctly implement and interweave dozens of policies, then their recommendation may be correct. However, if BC is too corrupt and

disorganized to effectively implement the recommended policies, I might recommend implementing UBI since it is egalitarian and relatively easy to implement.

Ultimately, the jury is out on UBI's impact on the labour market, and the burden of proof still rests on its advocates rather than its opponents. However, thanks to Salehi-Isfahani and Mostafavi-Dehzooei, Jones and Marinescu, and others, there is now enough evidence for UBI to be treated as a serious policy proposal rather than a populist pipe dream.

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Figures

Figure 1

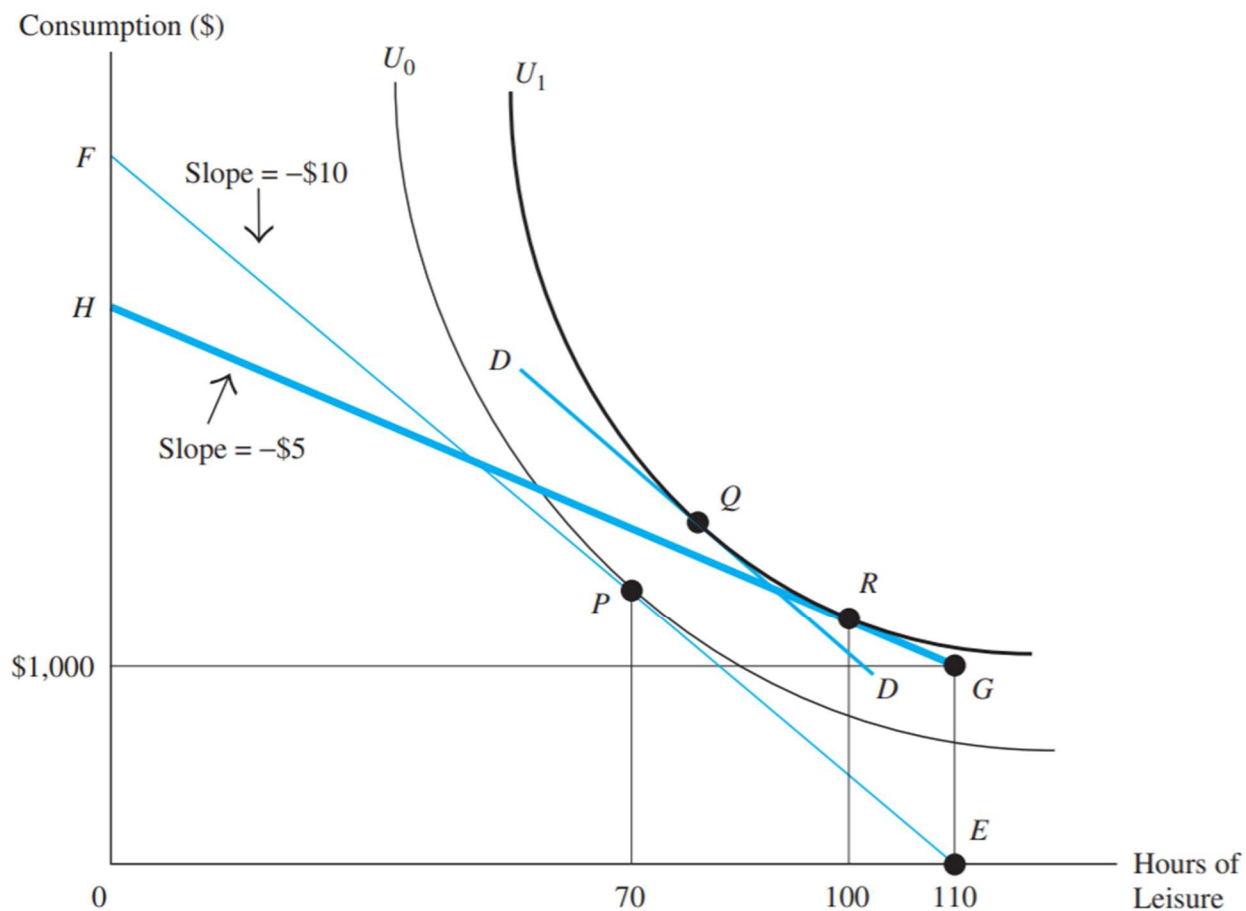


Figure 2: Examining Parallel Trends – by Quintile of Pce

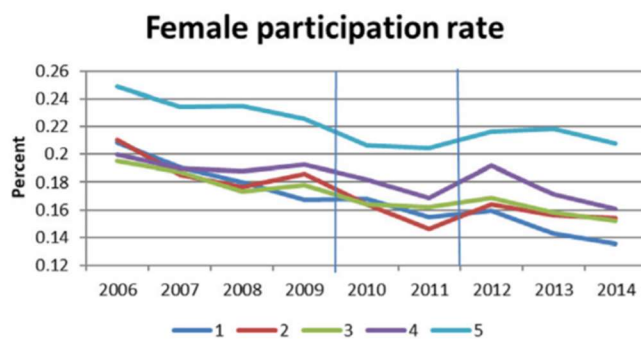
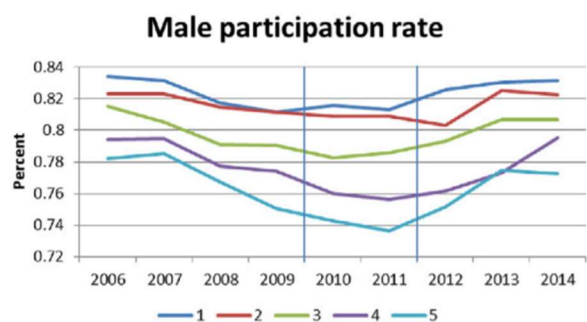
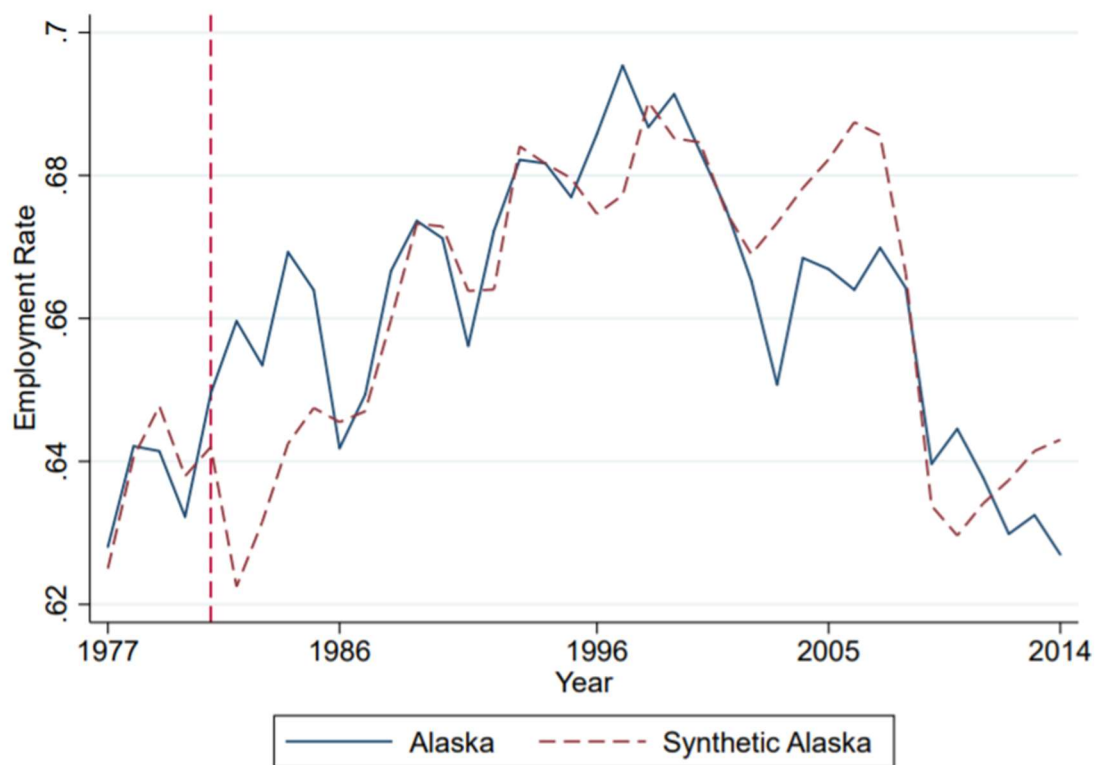


Figure 3: Synthetic versus Real Alaskan Employment Rate



Equations & Tables

Equation 1.1: DiD

$$Y_{it} = \alpha_0 + \alpha CT_i + \beta Year_t + \delta CT_i \times Year_t + X_{it}\beta + \epsilon_{it},$$

Where

- Y_{it} = Hours worked
- CT_i = 1(HH only received transfers in the second period)
- X_{it} = Individual and household characteristics

Table 1.1: Understanding Equation 1.1

	Comparison (CT=0)	Program (CT=1)	Difference groups
Year=0 (2010)	α_0	$\alpha_0 + \alpha$	α
Year=1 (2011)	$\alpha_0 + \beta$	$\alpha_0 + \alpha + \beta + \delta$	$\alpha + \delta$
Difference years	β	$\beta + \delta$	δ

Equation 1.2: OLS with Fixed Effects

$$Y_{it} = \alpha_0 + \alpha T_{it} + X_{it}\beta + \lambda_i + \theta_t + u_{it}$$

Where

- T = Treatment intensity = $\frac{\text{Transfers}^*}{\text{Expenditures per cap}}$
- X_{it} = Family or individual characteristics
- λ_i = Unobserved individual effect
- θ_t = Time effect

* The authors adjust Transfers to account for subsidy changes:

= (Transfers) – (Increased Expenditures on Previously Subsidized Items)

Table 1.2: Results from Equation 1.1 DiD Specification

	Men (1)	Women (2)
Year \times Treatment	-0.18 (1.53)	9.88** (3.22)
Year	-2.08* (0.86)	-0.96 (1.68)
Treatment	-0.20 (1.41)	-9.45** (2.96)
Age	0.99 (0.56)	-1.89 (1.84)
Age squared	-0.01 (0.01)	0.03 (0.02)
Province level unemployment	-18.78 (29.93)	-122.41* (58.37)
Education level:		
Less than primary	1.87 (2.10)	5.37 (3.99)
Primary completed	1.76 (2.26)	-1.53 (4.43)
Lower secondary	2.49 (2.52)	3.88 (4.60)
Upper secondary	0.86 (2.11)	1.09 (4.20)
Tertiary	0.65 (4.56)	-6.24 (6.03)
Log unearned income	-0.40** (0.07)	-0.04 (0.12)
Controlled for:		
Urban	Yes	Yes
Province	Yes	Yes
Marital status	Yes	Yes
R^2	0.120	0.282
Observations	3624	810

Table 1.3: Summary statistics for comparison and program

	Comparison	Program	P-value of difference in means
% urban	67.48 (46.85)	70.57 (45.59)	0.04
Household size	4.26 (1.58)	4.31 (1.67)	0.28
Labor force participation rate (%)	50.33 (50.00)	47.45 (49.95)	0.07
Employment rate (%)	44.06 (49.65)	40.23 (49.06)	0.01
Hours of work per week [†]	47.74 (19.19)	48.48 (18.62)	0.42
Per capita expenditures (million rials)	32.52 (28.14)	36.13 (28.94)	0.00
% literate	86.90 (33.75)	85.87 (34.85)	0.34
Age	36.93 (10.62)	36.30 (11.10)	0.06
% Female	52.26 (49.96)	53.08 (49.92)	0.60
Years of education	8.07 (5.06)	8.60 (5.31)	0.01
Marital status:			
Married (%)	76.35 (42.50)	69.79 (45.93)	0.00
Widow (%)	2.02 (14.05)	2.14 (14.47)	0.78
Divorced (%)	0.76 (8.67)	1.44 (11.91)	0.03
Never-Married (%)	20.87 (40.64)	26.63 (44.22)	0.00
Observations	1336	3811	

Notes: The sample refers to individuals 20–59 years old observed during the last three months of the Iranian year 2010/2011, corresponding to 21 December 2010 to March 20, 2011. Third column reports the p-value of a *t*-test for the means of the two groups being equal. [†] Only employed individuals are considered for hours of work statistics. Standard deviations in parentheses.

Table 1.4: Attrition

	Rural(%)	Urban(%)	Total(%)
Attrited			
Yes	22.3	32.1	26.8
No	77.7	67.9	73.2
Attrition by home ownership			
Rent	34.4	46.8	42.9
Own	20.6	25.8	22.7
Attrition by pce quintiles			
1	22.1	26.9	23.4
2	22.2	31.7	26.2
3	21.8	33.6	28.1
4	22.1	32.7	28.9
5	24.7	34.4	31.8

Note: pce stands for per capita expenditures.

Source: Authors' calculations using HEIS 2010–2011.

Table 1.5: Results from Equation 1.2 OLS FE Specification

	Men (1)	(2)	Women (3)	(4)
Intensity of treatment	0.055* (0.022)	0.069** (0.021)	-0.020 (0.043)	0.005 (0.053)
Change in unearned income	-0.038* (0.017)	-0.028 (0.015)	-0.005 (0.035)	-0.009 (0.037)
Log unearned income		-0.196* (0.090)		-0.015 (0.143)
Age		-0.028 (0.531)		-2.012 (1.071)
Age squared		0.000 (0.006)		0.023 (0.014)
Change in province unemployment		40.993 (40.210)		-146.821 (154.125)
Less than primary		0.833 (1.851)		2.567 (3.744)
Primary completed		1.855 (1.760)		11.049 (5.821)
Lower secondary		-0.190 (1.902)		-8.854 (4.806)
Upper secondary		1.351 (1.785)		2.568 (5.373)
Tertiary		4.265 (3.179)		-7.825 (12.937)
Controlled for:				
Urban and province		Yes		Yes
Marital status:		Yes		Yes
R^2	0.004	0.040	0.000	0.111
Observations	5137	5137	1176	1176

Notes: Intensity of treatment is the ratio of cash transfers (net of change in expenditures) to last year's per capita expenditures. Individuals 25–59 years old. Columns 2 and 4 include controls of first period characteristics. Unearned income is exclusive of cash transfers. Standard errors in parentheses are clustered at district level. * $p < 0.05$, ** $p < 0.01$.

Table 1.6: Alternative DiD Specification

	(1) All	(2) Bottom 40%	(3) Urban	(4) Rural	(5) Youth	(6) Men	(7) Women
Not a test province × Year	−0.57 (2.50)	−3.45 (2.52)	−4.38 (3.34)	−3.00 (3.09)	−4.48 (6.76)	−4.25 (3.31)	−0.98 (4.12)
Year	−0.51 (2.48)	2.03 (2.48)	4.86 (3.24)	0.35 (2.97)	5.11 (6.10)	2.22 (3.25)	3.85 (3.59)
Not a test province	−12.27* (5.14)	−12.90 (7.33)	18.79* (8.34)	−3.29 (7.15)	−4.55 (14.42)	−12.82 (7.86)	−2.04 (13.60)
Age	0.77 (0.42)	0.84 (0.64)	0.30 (0.97)	1.37 (0.81)	24.61 (28.67)	0.73 (0.72)	1.27 (1.36)
Age squared	−0.01 (0.01)	−0.01 (0.01)	−0.00 (0.01)	−0.02 (0.01)	−0.51 (0.64)	−0.01 (0.01)	−0.02 (0.01)
Province unemployment	−101.96** (20.29)	−107.35** (31.23)	−181.88** (45.31)	−64.47* (31.52)	−67.34 (61.89)	−119.52** (34.99)	−69.29 (38.89)
Log unearned income †	−0.41** (0.06)	−0.53** (0.10)	−0.60** (0.17)	−0.45** (0.11)	0.32 (0.24)	−0.52** (0.11)	−0.03 (0.21)
Observations	4784	2266	716	1550	358	1904	362
R ²	0.114	0.185	0.222	0.159	0.324	0.198	0.327

Notes: The control group consists of the households from three provinces that participated in the cash transfer program earlier (Not a test province = 0); all others are assigned to the treatment group (Not a test province = 1). All regressions include independent variables for level of education, marital status, and province/urban fixed effects. † Unearned income excludes cash transfers. Individuals aged 25–59 only. Columns (2) to (7) are restricted to households in bottom 40% of per capita expenditure. Standard errors in parentheses are clustered at district level. * $p < 0.05$, ** $p < 0.01$.

Source: Authors' calculations using data from the 2010–2011 panel.

Table 1.7: Alternative OLS FE Specification

	(1) All	(2) Prime age	(3) Urban	(4) Rural	(5) Men	(6) Women
Intensity of treatment	0.04 (0.02)	0.04 (0.03)	0.09* (0.05)	0.01 (0.03)	0.06* (0.03)	0.01 (0.07)
Test province × Intensity of treatment	0.05 (0.09)	0.04 (0.09)	−0.05 (0.09)	0.11 (0.12)	−0.02 (0.09)	0.19 (0.18)
Test province	2.78 (4.83)	0.79 (3.95)	2.66 (4.00)	−0.92 (13.55)	2.39 (4.65)	−2.42 (9.30)
Change in unearned income†	−0.02 (0.01)	−0.03 (0.02)	−0.03 (0.02)	−0.02 (0.02)	−0.04* (0.01)	0.01 (0.03)
Age	−0.69 (0.38)	−0.55 (0.48)	−0.59 (0.66)	−0.31 (0.63)	−0.39 (0.52)	−2.47* (1.25)
Age squared	0.01 (0.00)	0.01 (0.01)	0.01 (0.01)	0.00 (0.01)	0.00 (0.01)	0.02 (0.01)
Change in province unemployment	−126.86 (97.72)	−16.77 (79.40)	−17.44 (82.87)	−61.85 (130.70)	9.22 (85.15)	−172.10 (250.96)
Log unearned income†	−0.04 (0.08)	−0.10 (0.09)	−0.12 (0.12)	−0.06 (0.11)	−0.11 (0.11)	−0.06 (0.15)
Observations	5348	4708	2098	2610	3854	854
R ²	0.045	0.048	0.050	0.054	0.057	0.132

Notes: Test province is an indicator for households from three provinces that participated in the cash transfer program earlier (Test province = 1 for three provinces). Prime age are individuals aged 25–59. Columns 2–6 are restricted to prime age individuals. All regressions include independent variables for level of education, province, urban and marital status. † Unearned income excludes cash transfers. Standard errors in parentheses are clustered at district level. * $p < 0.05$, ** $p < 0.01$.

Source: Authors' calculations using data from the 2010–2011 panel.