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Stephanie Fortune-Taylor 4510052 43
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SUBGROUP HETEROGENEITY IN THE WAKE OF UNEXPECTED CHANGE

AN ABSTRACT

SUBMITTED ON THE EIGHTEENTH DAY OF DECEMBER 2019

TO THE DEPARTMENT OF ECONOMICS

IN PARTIAL FULFILLMENT OF THE REQUIREMENTS

OF THE SCHOOL OF LIBERAL ARTS

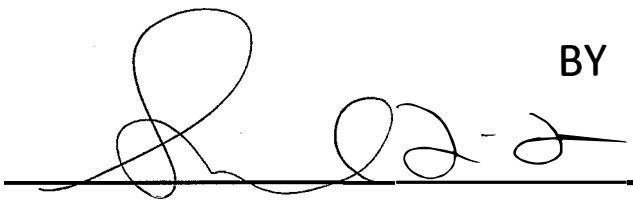
OF TULANE UNIVERSITY

FOR THE DEGREE

OF

DOCTOR OF PHILOSPHY

BY



Stephanie Fortune-Taylor

APPROVED: Steven M. Sheffrin, Ph.D
Director
James Alm, Ph.D
Elliott Isaac, Ph.D
Ross Hallren, Ph.D

ABSTRACT

It is well known that estimates of average treatment effects may obscure heterogeneous responses within treatment subgroups. In this dissertation, I use a common methodology—the triple differences estimator—to investigate subgroup heterogeneity within treatment cohorts. Within each chapter, I begin by using the difference-in-differences estimator to determine the average treatment effect of a natural experiment on a particular group. I then use the triple differences estimator to investigate whether subgroups within the treatment population respond differentially to the same intervention.

The chapter "Fertility Responses to the 1994 Earned Income Tax Credit (EITC) Expansion" investigates whether groups that have a revealed preference for larger families respond to the EITC expansion by increasing family size. The chapter "Non-Income Behavioral Responses to the 1994 EITC Expansion" uses a similar definition for revealed preference for family size. This chapter investigates whether families who used paid tax preparers or who *do not* have a revealed preference for larger size respond to the EITC expansion by reducing the number of children claimed on their taxes. The fianl chapter, "Worker-level Responses to Trade Shocks", estimates whether the welfare of certain classes of fragile manufacturing workers responds differentially to a sharp reduction and recovery in import penetration.

Each chapter uncovers surprising heterogeneity that diverges from the average treatment cohort effect. Families that prefer larger families increase family size post-expansion; post-expansion, families that use paid preparers reduce large family claims. For two out of three classes of fragile workers, increases in import penetration are associated with increased wage once manufacturing output recovers from shock. As a whole, this work points to the importance of taking a second look at salient underlying differences that may necessitate varied policy solutions to a common, unexpected occurrence.

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A DISSERTATION

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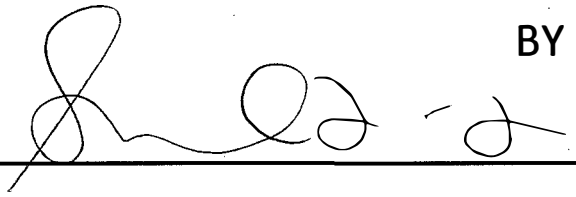

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Introduction

Unanticipated economic shocks can have varied effects across subgroups of a treatment cohort. This dissertation investigates the heterogeneity in response to an unexpected increase in a tax credit (chapters 1 and 2) and an unforeseen decrease in importing (chapter 3).

As the country's largest anti-poverty measure, the Earned Income Tax Credit (EITC) has effectively lifted millions of Americans out of poverty. Because credit amount is tied to family size, the EITC's structure could inadvertently encourage childbearing. Previous literature considering average fertility effects finds mixed, but generally negative associations between the credit increase and fertility. However, no other work has considered heterogeneous effects due to preference. In Chapter 2, I find that among groups that have a revealed preference for larger family sizes, the 1994 expansion is associated with as much as a 16 percentage point increase in childbearing. Because EITC recipients qualify for the credit based on their low incomes, and because the credit might not completely offset the cost of childrearing, fertility responses to the credit's expansion could result in an outcome that opposes the credit's antipoverty intent.

"Refund maximization"—making filing decisions, some of which are potentially noncompliant, that lower a taxpayer's tax liability—is prevalent among taxpayers of all incomes. In Chapter 2, I provide evidence that in the wake of the Earned Income Tax Credit expansion, low income filers took advantage of the EITC's change in benefits by increasing non-income based refund maximization activities, including filing as head of household, sharing dependents within kinship networks, and using paid tax preparers.

Difference-in-differences estimates indicate that these refund maximization activities increased by one to five percent in the post expansion period. Triple difference estimates indicate that certain EITC subgroups were as much as twice as likely to engage in refund maximization activities post expansion. These findings inform the discourse on the cultural applicability of certain tax code classifications given the interdependent financial networks governing low-income families.

Increased import penetration has typically been associated with adverse welfare effects for manufacturing workers. This relationship between trade activity and worker welfare has been documented empirically from the early 1980s to 2007. From 2008 to 2009, however, international trade underwent a period of unprecedentedly sharp decline called the Great Trade Collapse. By the beginning of 2010, manufacturing output had regained its pre-Collapse levels, and embarked on a period of growth that stabilized by 2011. In Chapter 3, I exploit the variation in penetration levels across manufacturing subsectors to investigate whether the adverse welfare effects from import penetration persisted after the Collapse. During this growth and stabilization period, I find that increasing import penetration was still generally associated with reduced wages and increased likelihood of unemployment, but that certain fragile groups benefitted from increased importing: both workers with a high school degree or less and workers associated with shrinking industries boasted higher wages in the growth and stabilization period. These results inform the discourse on proposed mercantilist policies that

unilaterally cast imports in a negative light, and point to the possibility that intermediate importing serves as a proxy for increased domestic productivity.

CHAPTER 2: EVIDENCE OF PREFERENCE HETEROGENEITY IN FERTILITY RESPONSES TO THE 1994 EARNED INCOME TAX CREDIT EXPANSION

I. Introduction

Among both men and women, the ability to support a child financially is one of the top considerations in having children (Weston, et al., 2004). U.S. birthing trends affirm this priority; fertility rates closely parallel numerous economic trends, including per capita income, unemployment, and foreclosure rates (Livingston, 2011). Though fertility has been trending downward for many years, childbearing's procyclical nature is well established; Americans are more fertile in times of plenty than in times of want.

While several international studies have found that subsidies do increase childrearing, in the United States, changes in fertility tied to increases in the Earned Income Tax Credit (EITC)—a means tested federal tax credit based upon family income and family size—have been more conservative in magnitude (Cohen, et al. (2013); Milligan (2005); Brewer, et al. (2012)). Nevertheless, none of these EITC studies incorporates a relevant fertility factor: preference. While, for some women, the decreased cost of childrearing would spur an increase in family size, for women who prefer not to have (more) children, additional income may not affect fertility decision-making. Since numerous studies have found that the EITC does not increase fertility generally, this paper investigates whether women

who are members of groups that have a revealed preference for larger family size respond to increases in the EITC by increasing childbearing.

To this end, I proxy for revealed preference by matching a woman's demographic indicators to indicators associated with median number of children. Specifically, I target two groups that have above average fertility rates: immigrants from high fertility countries and members of religious groups that have above average family size. I use Current Population Survey microdata for the years 1993-1999, along with macrodata on fertility rates from the World Bank, Jewish Virtual Library, and Pew Research Center, to identify EITC recipients within the immigrant and religious adherent groups. Exploiting the high rate of EITC take-up and leveraging the nation-wide EITC expansion effective in 1994, I employ the large-scale increase in the EITC as an exogenous shock to incomes in order to determine if women respond differentially to the increase in credit amount.

As a baseline check, I construct a difference-in-differences estimator that juxtaposes the fertility outcomes of all EITC eligible women in the post-increase era to all other women. Under my difference-in-differences specification in the period after the implementation of the benefit increase, I find that women eligible for EITC benefits had only slight changes in birthing compared to the rest of the population. This finding parallels estimates in the literature that the EITC expansion was associated with minimal changes in fertility.

I then create a triple differences indicator that contrasts the post-increase era fertility outcomes of EITC-eligible women members of high fertility groups to the outcomes of other all other women. I employ this triple differences strategy to determine whether an increase in income tied to family size encourages childbearing differentially. Under my triple differences specification, I find that among high fertility immigrant and high fertility religious adherent groups, the credit increase was associated with as much as a 16 percent increased incidence of childbearing.

This paper represents an extension to the existing literature by differentiating fertility outcomes according to a new metric: revealed preference for larger family size. While EITC recipient women did not generally respond by increasing fertility, EITC recipients from high fertility groups took advantage of the lower cost of childrearing and increased family size. Thus, for these women, the expansion was not simply an anti-poverty measure; it encouraged childbearing. Because EITC recipients qualify for the credit based on their low incomes, and because the credit may not completely offset the cost of childrearing, fertility responses to the credit's expansion have the potential to push families further into financial hardship as they grapple with the increased costs of supporting a larger family. In this sense, the policy intervention results in an outcome that opposes the credit's antipoverty intent.

The paper proceeds in the following fashion: Section 2 provides background on the EITC, and is followed by a review of relevant EITC and fertility literature in Section 3. Section 4 discusses the economic theory of my proposed rational choice model, and my empirical methodology. Section 5 details data, and Section 6 presents results and robustness checks. Section 7 is a discussion of the regression results, and Section 8 concludes.

II. Background

First implemented as part of the Tax Reduction Act of 1975, the EITC is a tax credit for low income workers that was originally meant to offset the burden of the Social Security tax and Medicare tax (Blank, 2002). The EITC has several provisions that make it unique.

First, EITC is means tested. At a take-up rate of 86 percent within its target population, EITC parallels both Temporary Assistance to Needy Families (TANF) and the Supplemental Nutrition Assistance Program (SNAP) in their shared objective to lift Americans out of poverty. EITC is only available to persons who fall within a certain range of incomes, determined by the number of eligible children in the home. Within the qualifying range, the income distribution is divided into three sub-ranges: the “phase-in”, “plateau”, and “phase-out” ranges (see Figure 1). Within the phase-in and phase-out ranges, recipients receive a percentage of the credit that increases (or decreases, respectively) with each

additional dollar earned. Within the plateau range, recipients receive one hundred percent of the credit. Figure 1 depicts the EITC schedule for the years 1993-1999, those utilized in the current study. The letters A, B, and C and corresponding dollar amounts on the table to the left of the figure denote the thresholds of the phase-in/plateau range and the plateau/phase-out range, and the end points of the phase-out range.

In addition to being income based, EITC is work dependent. Unlike other anti-poverty measures, such as TANF, SNAP, and Women, Infants, and Children (WIC), EITC receipt is wholly tied to the amount of earned income--generated by wages or self-employment income--that the taxpaying unit claims.

Third, EITC is refundable. Thus, the difference between the amount of taxes owed and the sum of taxes paid and credited can be received as a lump sum disbursement when a tax return is filed the following year.

Fourth, EITC receipt is limited to filers with social security numbers who are claiming children with social security numbers.[1] This social security number caveat functions as a default prohibition against unauthorized immigrants claiming the credit, as unauthorized workers use an Individual Taxpayer Identification Number, or occasionally a false social security number, for tax

[1] The Child Tax Credit, a smaller credit introduced in 1997 that is also targeted towards low to middle-income families with children, only requires that a filer has an individual taxpayer identification number (ITIN).

paperwork. This social security number caveat also sets EITC apart as a means-tested program specifically tied to legal residency, rather than merely need.[2] These unique features of the EITC target the intervention to a specific population: low to medium income U.S. citizens or residents who work, or who are married to a working citizen or resident.

III. Literature Review

There are numerous empirical studies that evaluate the potentially pro-natalist effect of programs that offset the cost of childrearing. Most fall into two groups: (1) international studies, which generally find that benefits encourage childbearing, and (2) U.S. studies, which have mixed results, but generally find that benefits only minimally affect fertility.

Because fertility rates below replacement are a problem for many developed countries, several countries employ family size-based tax concessions that are intentionally pro-natalist. Milligan (2005) finds that Quebec's "Allowance for Newborn Children", which was specifically aimed at increasing fertility, engendered a strong fertility response. The subsidy, which paid as much as $8,000 Canadian dollars to families during the late 1990s, increased fertility as much as 25%. Laroque & Salanie (2008) simulate the addition of a universal birth subsidy

[2] While it is true that unauthorized immigrants are not personally eligible to receive welfare, they, and all others who might be ineligible for welfare can receive benefits– including cash transfers, food stamps, and Medicaid – for eligible children. In fact, "child only" welfare cases 46% of the welfare caseload in 2011. (TANF Ninth Report to Congress).

of 150 euros per birth to the existing French taxation system. They estimate that the measure would increase fertility by 0.3 births per woman at the cost of 0.3 percent of France's GDP. Raute (2014) finds that the 2007 expansion of maternal leave transfers to include all German women, rather than only low-income women, caused a large, discontinuous jump in the fertility rate of women in the middle and upper-ends of the education and income ranges. The transfers, which were income dependent, but not means tested, and as large as 21,600 Euros, were responsible for a 6 percent and 13 percent increase in fertility for middle-educated and high-educated women, respectively, relative to their low educated peers.

Other benefit programs may not be intentionally pro-natalist, but have positive fertility effects. Cohen, et al. (2013) examines changes child subsidies in Israel in the late 1990s-early 2000s. The authors find that, among married women, changes in the subsidy for the marginal child are associated with positive fertility across all ethnic and religious groups. In addition, the authors find that the mean level of subsidy is associated with a 7.8 percent increase in fertility. The UK's late 1990s expansion of welfare benefits for low-income women resulted in a 50 percent increase in spending per child. Brewer, et al. (2012) finds that the benefit expansion resulted in a 15 percent increase in fertility among coupled women.

While international studies generally find large positive fertility effects for family-size dependent benefit increases, U.S. studies generally find smaller, mixed

effects for similar programs.[3] Whittington, et al. (1990), a classic study on the topic, finds that the personal exemption has a large positive impact on birthrates. In addition, Whittington, et al. notes that the regressive nature of the exemption means that the birth subsidy does not extend to high income taxpayers. Whittington, et al. (1990) is unique in finding large, positive effects of family size based tax concessions on fertility. Crump, et al. (2011) challenges Whittington, et al. (1990)'s results, finding a much smaller coefficient on the personal exemption coefficient.

Research addressing childbearing and the EITC—the best corollary to the international tax programs in terms of benefit generosity—comes to mixed conclusions. Crump, et al.(2011) extends its analysis of child tax subsidies to include the Child Tax Credit and EITC. They find that aggregate child tax benefits have no long run effect on fertility, but have a positive short run effect on fertility under certain specifications.

In their 2003 article, Baughman and Dickert-Conlin find that EITC had mixed effects based on race and marital status. Specifically, they consider the effect on fertility of both state EITC benefits and federal EITC expansions effective in 1991, 1994, and 1998. Baughman and Dickert-Conlin (2003) find that an

[3] Moffit's (1998) survey of articles that examine the effect of welfare on fertility covers the pre-Personal Work Opportunity and Reconciliation Act era. While the majority of studies assert that welfare has a significantly positive effect on fertility, a sizable minority of the studies estimate that welfare has no effect.

increase in EITC is positively associated with first births for both married and unmarried non-white women, but negatively associated for unmarried white women and statistically insignificant for married white women. Baughman and Dickert-Conlin (2009), however, finds that EITC expansions and state EITC variation are associated only with very small reductions in higher order fertility among white women.

Herbst (2011) finds that an increase in the EITC is associated with a reduction in the number of abortions, but also finds that this reduction does not result in an increase in the number of births. Using panel data to investigate cross-state variation in EITC implementation's effect on abortion rates, Herbst (2011) finds that increases in the maximum EITC are associated with an 8 percent decrease in childbirths, a 7 percent decrease in pregnancies, and as much as a 20 percent decrease in abortion rates.

Hoynes (2011) finds that the EITC does not affect the completed fertility rate—the number of children to which a woman will give birth in her lifetime. Meckel (2013) finds that while EITC does not affect completed fertility, the ability to claim the credit one year earlier is associated with a reduction in spacing between low educated mothers' first and second children.

Bastian (2018), however, finds that state EITC expansions affect the completed fertility rate. Using panel data spanning over 30 years, Bastian finds that a 10 percentage point increase in state EITC results in a 1.2 percentage point

increase in likelihood of having an additional child the following year. Event study estimates confirm that this increase is an increase in completed fertility, rather than simply a change in birth timing.

The difference between international women's large, positive responses and domestic women's small, mixed responses to family size based benefits is an unresolved question, and can stem from several sources nascent to the EITC. First, it is possible that U.S. women feel EITC benefits are too transient to tender fertility responses. The EITC program benefits change frequently (Moffitt 2016). Birthing a child results in a permanent change to a woman's family, and the nature of means-tested program eligibility means that, from year to year, a woman's benefit amount varies according to her family income. It is logical that the permanence of the financial responsibilities associated with an additional child, when juxtaposed to the impermanence of a yearly benefit of varying amount, might cause women to consider childbearing in response to a benefit increase to be too great of a financial risk.

Additionally, it is likely that for most women, the marginal financial cost of an additional child will not be fully offset by the marginal tax subsidy per child. Thus, though additional children might be made cheaper through tax concessions, they still might not be affordable for the low to middle income benefit recipients.

A third reason for the disparity between international and domestic responses could stem from aforementioned differences in program intent and

messaging. The EITC is not intended to augment fertility; it is an anti-poverty measure. Québec's Allowance for Newborn Children, Germany's maternal leave policy expansion, and Australia's "Baby Boom Bonus" were specifically marketed and designed to encourage childbearing. That those programs achieve their fertility goals, and the EITC realizes its anti-poverty goal, is completely reasonable.

Last, it is possible that U.S. women have more rigidly structured family size preferences than their international counterparts. Then, decreases in the price of the marginal child would not encourage fertility. This paper's main contribution is an investigation of this "preferences" based reasoning. Preferences for children are influenced by social context, and vary by age, urbanization, education, race, and religion ((Matsumoto and Yamabe 2013); (Ding and Hesketh 2006); (Mosher and Barach 1996); (Martinez, Daniels and Chandra 2012)). In addition, preferences affect completed fertility (Jennings and Barber 2013). Differentiating groups of women based on revealed family size preferences make it possible to examine fertility responses to EITC credit expansions among women who prefer larger family size.

IV. Methodology

IV.1 Theoretical Model

To model women's fertility behavior, I follow Hotz, Klerman, and Willis (1997) and adapt a simple neoclassical model of consumer demand in which

women are consumers who maximize utility by choosing number of children based upon the price of children, preference for children and their budget constraint. I assume that there are no obstacles either to childbearing or contraception.

Women maximize a utility function

$$U=U(n, s) \quad (1)$$

where n is the number of children and s represents all other goods. U is twice differentiable, strongly monotonic, continuous and concave.

Women maximize utility subject to the budget constraint

$$M = P_s s + P_n n \quad (2)$$

where P_n is the per unit price of children and n is the number of children feasible at the given price.

P_s is the composite price of all other goods and M is family income

$$M=(i, e) \quad (3)$$

where i is earnings and e is means-tested entitlement income based upon number of children. Thus, in the absence of children, $P_n n = 0$, and women spend all of their income on all other goods, according to

$$M = P_s s \quad (4)$$

Both in the absence of children and for women who do not meet the means test, $e=0$. Then $M(i, e)$ is assumed to be fixed, and a woman's decision to have more children requires that she reallocate to childrearing some percentage of her family income devoted to all other goods.

For women who pass the means test, $M(e)$ increases up to a threshold based upon family composition, income, and filing status as number of children increase. Specifically, $M(e)$ increases both

(1) when expansions occur, independent of changes in fertility, and

(2) in response to fertility changes for first and second children, but not higher order births.

Then

$$\boldsymbol{M_{fert}(e) + M_{inc}(e) + M_0(i, e) = M(i, e)} \quad \textbf{(5)}$$

Where $\boldsymbol{M_0}$ is the baseline income before any fertility increase, as defined in **(2);** $\boldsymbol{M_{fert}}$ is the additional entitlement income that is awarded in response to the fertility increase; and $\boldsymbol{M_{inc}}$ is the additional EITC income associated with the program expansion and given to women with one or more children.

I assume information asymmetry regarding increases in $M(e)$. That is, women are unable to differentiate between $\boldsymbol{M_{inc}}$ and $\boldsymbol{M_{fert}}$, might be wholly unaware that $\boldsymbol{M_{inc}}$ exists**,** and, accordingly, are uncertain of the source of the additional

money received as a result of the EITC's expansion ($\boldsymbol{M_{inc}}$).[4] It follows that women may believe—erroneously—that expansion income increases were actually a result of fertility increases. Correspondingly, these women may assume that additional childrearing in years subsequent to the expansion period will be defrayed by similar increases in benefit amount.

The increase in number of children results in an increased total cost of childrearing for all women regardless of means, but varied changes in the per unit cost of childrearing. For women with two or more (post fertility increase) children, economies of scale may reduce the per child price of high cost expenditures such as childcare and housing. Then, the per unit cost of childrearing decreases as number of children increases, according to

$$\boldsymbol{P_{n_1} \geq P_{n_2} \geq P_{n_{3+}}} \qquad \textbf{(6)}$$

Where the index $\boldsymbol{k}$ on $\boldsymbol{P_{n_k}}$ indicates child parity, synonymous to the number of children that the women is rearing. Per child investments vary according to parent means, parent tastes, and child endowments, but I assume

$$\boldsymbol{P_n \geq dM/dn} \qquad \textbf{(7)}$$

[4] Beliefs regarding possible causes of increased refunds might be the shrewdness of their tax preparer, a personal filing decision, or a tax year birth (in which case $\boldsymbol{M_{fert}} \geq 0$, dependent on the number of children previously claimed).

That is, the additional income gained from entitlements for one more child is always less than or equal to the child's cost.[5] During and after the expansion period, increasing quantity of children n is made more feasible by the fact that EITC eligible women enjoy a reduced per unit price of childrearing,

$$P_{exp} = P_n - dM/dn < P_n \quad \textbf{(8)}$$

Where P_{exp} is the cheaper per unit price of childrearing for women who meet the means test.

Because additional children cause women to incur additional costs, and because of the a priori assumption that there are no obstacles to childbearing or fertility, it follows that we can separate all women into two categories—

Group 1: Women who want more children and are therefore open to increasing family size, and

Group 2: Women who do not want more children and are therefore uninterested in increasing family size.

I define preference additional for children γ as comprised of two discount factors:

[5] By "cost", I mean economic cost, as opposed to accounting cost. The economic cost of childrearingincludes cost of childcare, regardless of if the parent is uncompensated for the care, and therefore provides it for "free". It is possible that for a minority of women, their incomes are so heavily subsidized that the parental annual monetary outlay is less than the additional income received from increasing fertility. In this case $P_n < dM/dn$, and the family receives a net financial gain as a result of the additional child. I assume that these families are randomly distributed throughout the country, comprise an extremely small percentage of the overall population, and therefore can be omitted from the model.

(1) $\boldsymbol{\rho}^{\uparrow\downarrow}$ --a discount factor indicating the upper and lower bounds of willingness to pay for children, taking values $-1<\rho^{\downarrow}<\rho^{\uparrow}<\infty$, according to

$$\left(\frac{\boldsymbol{P_n}}{[\mathbf{1}+\boldsymbol{\rho}^{\uparrow\downarrow}]}\right)=\boldsymbol{P}_{\boldsymbol{wtp}}^{\uparrow\downarrow} \qquad \textbf{(9)}$$

The larger the family size a woman prefers, the wider the range of her $\boldsymbol{\rho}$, and, necessarily, the greater the price she is willing to pay per child, $\boldsymbol{P}_{\boldsymbol{wtp}}^{\uparrow}$, relative to her income $\boldsymbol{M}$. In addition, her desire to have a larger family means that she is willing to give up certain aspects of child quality, thus increasing her $\boldsymbol{\rho}^{\downarrow}$ and lowering her $\boldsymbol{P}_{\boldsymbol{wtp}}^{\downarrow}$ in order to reduce the aggregate total cost of childrearing.

(2) $\boldsymbol{\delta}$--a scale factor representing willingness to reallocate funds from all other goods to childrearing, taking values $-\infty \leq \boldsymbol{\delta} < 1$, according to

$$\left(\frac{\boldsymbol{P_s}}{[\mathbf{1}-\delta]}\right)=\boldsymbol{P}_{\boldsymbol{wtr}} \qquad \textbf{(10)}$$

Then the larger the family size a woman prefers, the smaller her scale factor $\boldsymbol{\delta}$, and, necessarily, the smaller the price she is willing to pay for all other goods, $\boldsymbol{P}_{\boldsymbol{wtr}}$.

Then preference for additional children $\boldsymbol{\gamma}$ is increasing in range of $\boldsymbol{P}_{\boldsymbol{wtp}}^{\uparrow\downarrow}$, decreasing in $\boldsymbol{P}_{\boldsymbol{wtr}}$, and is defined as

$$\boldsymbol{\gamma}=\boldsymbol{P_s}-\boldsymbol{P}_{\boldsymbol{wtr}}+\boldsymbol{P}_{\boldsymbol{wtp}}^{\downarrow} \qquad \textbf{(11)}$$

The decision to have more children is determined by the sign of the relationship

$$dU/dn = E(dM/dn) - \gamma \quad \textbf{(12)}$$

which is generated by maximizing utility subject to the constraints defining equations 2-10. That is, the determination that increased childbearing will increase (decrease) utility can be quantified as the difference between the expected change in income from the next birth and the lower bound of the preference price for more children. Women for whom dU/dn is positive respond by increasing childbearing. Women for whom dU/dn is negative respond by not increasing childbearing.

Data limitations prevent the calculation of dU/dn ; neither $E(dM/dn)$ nor γ is calculable. Nevertheless, I proxy for preference γ by using the average total fertility rate based on a woman's demographic group membership. Groups that maintain a high total fertility rate on EITC level income have made the de facto decision to lower $P^{\downarrow}_{wtp}$ and/or raise $P_s - P_{wtr}$, relative to their similarly incomed lower fertility peers. Under of the assumption of information asymmetry regarding $M_{fert}(e) + M_{inc}(e)$, I do not attempt to calculate dM/dn, but because women might have some knowledge that typically the third child does not result in additional income, I control for dM/dn by separating women into subgroups based upon whether the parity of additional childbirth would result in additional income.

I postulate that an increase in childrearing that increases utility, dU/dn under the theoretical model, is equivalent to an empirical increase in number of children during the expansion period. Y_{its} denotes the census outcome "number of children less than five" for woman *i* residing in state s within a time cohort t. Then demand for more children dn is modeled according to

$$dn \sim dU/dn \sim Y_{its} \quad \textbf{(13)}$$

IV.2 Reduced-form Econometric Model

I use difference-in-differences and triple differences methodologies with age of mother and presence of state EITC fixed effects to analyze the effect of the 1994 EITC expansion on the demand for children. While EITC receipt is determined by family size and income; I do not use income as an indicator of receipt, because of the potential endogeneity of this measure due to simultaneity. Specifically, persons might sort into a lower income group as a result of childbearing, given that childbearing among women employed outside of the home is associated with extended absence from work. To guard against this sample bias, I proxy for being of EITC income level by using education as a predictor of credit qualification. Following Eissa and Hoynes (2004), I impute EITC eligibility to women who have less than a high school diploma.

Because the EITC is a means-tested credit, expanded tax credits only benefit low-income persons who are legally permitted to work in the United

States. Thus, the law constitutes an exogenous shock to the incomes of citizen and authorized resident low–income families. Higher income families—proxied for by women who have a high school diploma or more advanced education--do not qualify for the EITC, and therefore constitute a control group for the EITC population. In addition, unauthorized immigrant families also do not qualify for the EITC, and will also be a component of the control population. Deviation of outcomes from smooth labor cohort trends are estimated systematically using two general models with specifications:

Model 1: Difference (EITC Education Level) in Difference (Period following 1995) with age of mother and presence of state EITC fixed effects

$$Y_{ts} = \beta_0 + \beta_1 EITC + \beta_2 Post_t + \lambda_1 EITC*Post_t + StateEITC_s + Age + \varepsilon_1$$

Model 2: Difference (Preference for children) in Difference (Period following 1995) in Difference (EITC Education Level) with age of mother and presence of state EITC fixed effects, by marital status and number of children

$$Y_{cts} = \beta_0 + \beta_1 Prefer_c + \beta_2 Post_t + \beta_3 EITC + \lambda_1 EITC*Post_t + \lambda_2 EITC*Prefer_c + \lambda_3 Post*Prefer_{tc} + \delta_1 Prefer*Post*EITC_{ct} + StateEITC_s + Age + \varepsilon_2$$

I use Model 1, the difference-in-differences model, for all EITC eligible women in the post-EITC expansion period. I juxtapose these post-expansion period women's demand to the demand of all EITC ineligible women and EITC eligible women living in the pre-expansion period. Y_{ts} denotes the census outcome "number of children less than one" for women residing in state s within a time cohort t. In this model, I do not separate women based upon authorization status,

because the purpose of the difference-in-differences estimator is only to serve as a baseline comparison of my model estimates to the estimates in the literature. The time cohort is based upon whether she is observed before or after the earliest date that evidence of a fertility decision could be witnessed. *EITC* is an indicator equal to one if the woman is an imputed EITC recipient based upon her education level being less than a high school graduate, and equal to zero otherwise. $Post$ is an indicator equal to one for fertility responses in the years 1996- 1999, and equal to zero for fertility responses in the years 1993-1995. $EITC*Post$ is the treatment interaction indicator equal to one for having EITC level education and being observed in the years 1996-1999. Specification 1 is estimated with no controls, specification 2 is estimated with age of mother fixed effects, and specification 3 is estimated with age fixed effects and presence of state EITC fixed effects s. Specifically, I include a categorical variable for whether the woman's state of residence has its own state EITC program, as women residing in states with a state EITC might have greater knowledge of eligibility requirements for the federal credit, and may also receive additional income that is a scaled proportion of the federal credit. ε_1 is the error term.

I use Model 2, the triple differences model, to analyze the demand of EITC recipient women in the post period whose demographic subgroup or neighborhood affiliation—proxied by state of residence--is associated with a revealed preference for more children. While data limitations do not allow me to

analyze individual women's family size preferences, statistics on the total completed fertility of demographic subgroups allow me to proxy for subgroup preferences based upon revealed preferences for family size. This preference assignment by demographic group is fully in keeping with the unit of observation, which is the aggregated EITC recipient by preference status by post-expansion period cell, rather than the individual woman.

The denotations for *EITC, Post, EITC*Post, Age,* and *State* given in the triple differences Model 2 parallel those for the difference-in-differences Model 1. Unique variables to Model 2 are Y_{cts}, denoting the census outcome for women of revealed preference for children c in reform cohort t, and the interaction terms *EITC*Prefer* and *Post*Prefer*. Under the triple differences methodology, my coefficient of interest is δ_1 on the variable *Prefer*Post*EITC*$_{ct}$, an interaction term that denotes being an EITC recipient in the post expansion period who prefers more children. The model's specifications parallel those of Model 1, with no controls as specification 1, age of mother fixed effects as specification 2, and state of residence EITC and age of mother fixed effects under specification 3. Specification 2 is my preferred specification because it eliminates the variation due to the dissimilar age distributions of the natural born population and the imputed authorized foreign-born population. While specification 3 also controls for age, the additional control for presence of state EITC is not consistently instructive, as state EITCs vary greatly in generosity and tenure.

IV.4 Identification

In order to identify the effect of EITC expansion on fertility, the ideal approach would be to use data from a controlled experiment that randomly assigns taxpayers either to a treatment group that receives additional EITC benefits or a control group that does not receive the benefits. This random assignment would allow me to draw causal inference if I were to find a statistically significant differential fertility response for those who receive the expanded benefits. Though no such controlled experiment exists, the natural experiment of the 1994 EITC expansion provides a promising alternative for a number of reasons.

First, the expansion was nationally implemented, affecting all eligible women across the country. Second, the credit increase was dramatic, changing as much as a 67 percent from tax year 1993 to 1994, and 153 percent over the expansion period. Third, the increase was largely unanticipated by the benefitting population, thus constituting an exogenous shock to the target population's incomes. Chetty et al.(2013) notes that EITC recipients generally are inattentive to changes in the tax code, and information frictions generally prevent the population from altering decision-making prior to witnessing increased tax returns. Even if they were aware of changes in the EITC schedule, Edin, et al.(2013) finds that most EITC recipients view their tax returns as a complete entity, rather than as comprised of separate credits that change differentially from year to year.

To identify groups of women that have a revealed preference for larger family size, are on the margin for increasing childbearing, and might respond to a credit increase by increasing fertility, I investigate completed fertility for two specific subpopulations that have a documented revealed preference for larger family size: those who are authorized residents or citizens that emigrated from high fertility countries, and those from states with a high percentage of adherents from religious groups that have a revealed preference for larger family size.

IV.4i High Fertility Immigrants

To determine which immigrants have a revealed preference for larger families, I sort countries by total fertility rate. I consider the 133 countries that have a higher total fertility rate than the U.S. median total fertility rate of 2.056 children per child as high fertility countries (World Development Indicators, 2000). To confirm that within native country of origin (COO) preferences extend to the U.S. context, I use Current Population Survey pre-expansion period summary statistics to analyze fertility rates for women from designated high fertility countries. Results in Appendix Table A1 indicate that women from high fertility countries have an average fertility rate that is between 38.4 percent (when compared with prefer women's 1.07 children/woman) and 64.6 percent (when compared with low fertility immigrant women's 0.902 children/woman) higher than other groups in this study.

Next, I go through a stepwise process to determine which women from high fertility countries are authorized residents, and are therefore eligible for EITC. Unauthorized residents, who pay billions of dollars in taxes annually, are ineligible for EITC, so credit increases would not benefit them financially (Internal Revenue Service 2014). [6] Because the Census does not ask respondents about their legal immigration status, however, I use a systematic metric to impute authorized or unauthorized classifications. In a methodology adapted from the residual method, and similar to the method used in Passel (1987), Cortes (2004), and Cortes (2012), I classify women as authorized or unauthorized based upon their year of immigration, country of immigration, and the frequency of persons with same COO citizenship being granted asylum in their year of emigration. [7]

To begin, I separate non-citizens into "IRCA eligible" or "IRCA ineligible". The Immigration Reform Control Act (IRCA), granted amnesty to persons who arrived before January 1, 1982 and resided in the country continuously. Thus, I classify non-citizen women whose year of immigration is 1981 or earlier as IRCA

[6] Many unauthorized immigrants file taxes but they do so either with a false or expired social security number or with an Individual Taxpayer Identification Number (ITIN) (Hallman 2018). Those who file with a false social security number cannot receive refunds because the IRS only issues refund to persons whose social security number matches their name (a rather difficult thing to orchestrate when using a false social security number).

[7] Cortes (2004) admits that measurement error exists when using this methodology, and notes that slippage in defining arrival groups will make potentially authorized groups look more like unauthorized groups. The result of this is that estimates are likely to be downward biased. Passel(1986) admits that the residual method has limitations, but notes that, prior to the introduction of the method, estimates of the undocumented population were based "on little more than speculation"

eligible, legal residents, and EITC eligible. These 1981 or earlier emigrants are therefore considered “treated”.

Following Cortes (2004), I categorize women whose year of arrival is 1982 or later as either “refugees/asylees” or “economic immigrants”. I make this determination based upon the woman’s country of citizenship, year of immigration, and the frequency of accepted refugee/asylee applications for persons from that country. For example, I classify a Cuban woman who emigrated in 1986 as a refugee, but a Jamaican woman who emigrated in the same year as an economic immigrant. Because all refugees and asylees are legal residents, they have social security numbers, and are eligible to receive EITC. Thus all women classified as refugees or asylees are selected into treatment.

I consider non-citizen women who arrived after 1981 and are not classified as refugees/asylee to be “unauthorized”. This group invariably contains numerous authorized immigrants: it subsumes women as disparate as graduate students employed as teaching assistants and self-employed caregivers. Nevertheless, I err on the side of caution and consider all post-1981 non-refugee/asylee arrivals as unauthorized because of the heterogeneity of the unauthorized population. Unauthorized immigrants are not monolithic; while illegal immigrants are often portrayed as Latino southern border crossers, 45% of illegal immigrants are individuals who were legally residing in the U.S. at some point, but overstayed their visas (Passel, 2006). Thus, I will not exclude individuals, even based on

characteristics such as education level, national origin, or years of residency. This conservative approach to identification means that my estimates may suffer from underestimation, as persons in the control immigrant population might also be authorized, and therefore treated.

IV.4ii Religious adherents

Frejka and Wassesn (2006) find that, without differentiating by religious affiliation, strongly held religious values are positively associated with larger family size. Among U.S. religious groups, Orthodox Jews and Latter-day Saints (LDS) stand out as groups that have a revealed preference for large families.[8] Cohen, et al. (2013) notes that family planning is discouraged in Orthodox Jewish communities, and fertility rates in the population are almost double the U.S. fertility rate (Pew Research Center 2015).[9] Wilkinson and Tanner (1980) assert that LDS are encouraged to have large families. The LDS fertility rate is not as high as the Orthodox Jewish fertility rate, but at 3.4 children per family, it is much higher that the U.S. fertility rate (Pew Research Center 2015). Because LDS and Orthodox Jewish groups have a revealed preference for larger families, I designate women who are members of these groups as preferring more children.

[8] Other religious groups, such as Amish/Hutterites and Muslims, also have high fertility rates, but their small numbers (in the case of Amish/Hutterites) and diffuse populations (in the case of Muslims) make studying the fertility of these populations difficult.

[9] In 2013, the Orthodox Jewish fertility rate was 4.1 children per family, as opposed to the 2013 general U.S. rate of 2.2 children per family (Pew Research Center 2015).

Because data limitations do not allow me to observe women's religious affiliation, I use density of religious adherence by state of residence. The highest concentrations of Jews are found in New York (7 percent) and New Jersey (6 percent). The highest concentrations of LDS are found in Utah (55 percent), Idaho (19 percent) and Wyoming (9 percent).

In addition, Jennings and Barber (2013) find that neighborhood effects impact fertility. In their study, the authors found that although women might have personal preferences for a smaller family size, neighborhood preferences for larger family size were determining factors influencing women to have larger families. Thus, it is possible to have spillover effects of increased fertility on non-adherents living close to Orthodox Jewish and LDS women. Because LDS and Orthodox Jewish women are disproportionately represented in Utah, Idaho, Wyoming, New York, and New Jersey, and because of the potential for their neighbors to be affected by these groups' revealed preferences for larger family size, I designate all women residing in these states as preferring more children.

It is important to notes that I use women's demographic group revealed preference to proxy for preference for more children. Because my unit of observation is the EITC status x time period X preference status cell, rather than the individual woman, using revealed preference of the demographic group is a sound way to proxy for group preference.

IV.4iii Appropriate Counterfactual

As parallel pre-treatment trends are an assumption for performing difference in difference analysis, I present graphs of average number of children less than five by treatment and control groups. As shown in figure 5, the trends for all treatment subgroups ad their respective controls remain parallel throughout the pre-treatment period (the years 1993-1995). High fertility religious adherents trend similarly to their control group, comprised of women from states without a disproportionate representation of women from high fertility religious groups (Panel A). High fertility immigrant omen's number of children less than five trend similarly to all other (Panel B). prefer women-, depicted in Panel C, have higher numbers of young children on average, but the difference between prefer women and their control—comprised of documented immigrants who are from low fertility countries, undocumented immigrants regardless of country of origin, and women from the states and territories other than New Jersey, New York, Idaho, Wyoming, and Utah—remained consistent over the pre-treatment period.

IV.4iv Timing

Determination of the treatment versus control time cohort begins by identifying when taxpayers would have sufficient information about the

expansion to respond to the increased credit amount. The EITC expansion was part of the Omnibus Reconstruction Act of 1993 (OBRA-93), which was introduced to the House on May 25, 1993 and signed into law on August 10, 1993. Making several assumptions–that EITC income level taxpayers are attentive to changes in tax legislation, that these taxpayers understand the increased tax credit associated with an additional child, that taxpayers would make a fertility decision immediately upon information receipt, that conception could occur immediately if the decision have a child had been made, and that the gestational period would be 40 weeks–a woman could tender a fertility response to the law as early as early as March of 1994.

There are many reasons, however, that such an early start to the treatment period is unlikely. As mentioned heretofore, many taxpayers respond slowly to tax changes because of inattention and other frictions. Second, Tach and Halpern-Meekin (2014) note that most EITC recipients do not know what EITC is; rather, they assume that their large refund is a reward or bonus given to low income persons who work and have children. Thus, information about an EITC expansion might not be understood to be an announcement of an increase of an EITC level income taxpayer's refund. Thirdly, even if an individual obtained and understood information about the EITC expansion, Chetty, et al. (2013) assert that EITC taxpayers often make decisions based upon word of mouth and testimonial information. Thus, it is possible that the earliest source of information considered

credible comes from an associate (or the taxpayer herself) who received a larger refund as a result of the expansion.

I build my pre-treatment versus post-treatment cohorts based upon this "testimonial" time frame; that is, I assume that fertility responses to the expansion can be made as early as when an individual receives her larger tax refund or hears information from an associate about a larger refund. OBRA-93 legislation was effective for the tax year 1994. Individuals could file their 1994 taxes as early as January 1, 1995, but because employers did not have to provide W-2s until January 31, 1995, I consider January 31 to be the earliest filing date. Taxpayers who e-filed their returns could receive their returns within twenty-one days. Thus, women could be alerted to their or an associate's increased refund as early as late February, 1995. Whittington, et al. (1993) estimates that women's average time to birth once a conception decision has been made is 24-31 months, but given that the gestation period is 40 weeks, a woman could tender a fertility response as early as the end of November, 1995. Because CPS data is gathered yearly, I start the post treatment period in the year 1996. Thus, because there might be women in the pre-treatment cohort who are treated, my results will be biased downwards, and will suffer from underestimation. A timeline of the heretofore mentioned events is provided in Figure 6 for clarity.

V. Data

Current Population Survey (CPS) microdata are well suited to the analysis of fertility outcomes based upon preferences. The chosen years of observation–1993 to 1999–allow for identification of several key variables. The "age of youngest child" variable allows me to determine if the woman has had a child in the past year. I code women as having a positive fertility response if their youngest child is less than one. In addition, the "number of children" and "age of eldest child" variables allow me to determine how many EITC eligible children a woman has. For children to qualify for EITC, they must be younger than 19, or 19-24 and a full-time student (IRS 2018).

The birthplace variable facilitates the process of positing whether a non-citizen is a

refugee/asylee, and the year of immigration variable allows determination of when the resident came to the United States to stay. This crucial variable allows imputation of "authorized" or "unauthorized" status to non-citizens, as persons who arrived before December 31, 1981 were eligible for blanket amnesty, but persons who arrived on January 1, 1982, or later were ineligible.

From the IPUMS website (King et al., 2010) I use the 1994–2000 sample of March observations, which reports information gathered in the previous calendar years (1993-1999). I retain women 18 and over as my treatment age range. I limit the population to this range, rather than literature standard of over 15,

because of the lack of parallelism in age ranges for the potentially authorized versus the potentially unauthorized population.

Data on completed fertility rates by country of origin are sourced from the World Development Indicators for the year 2000 (World Bank Group 2017). Fertility rates by country of origin are listed in Appendix table 1. I denote countri3es with higher completed fertility rates than the US as “high fertility”.

Table 1 Panels A, B, and C capture the difference of means calculations for the years before the treatment period (1993-1995) for all prefer, immigrant prefer, and religious prefer women, respectively. Table 1 Panel A, which gives pre-expansion summary statistics for women distinguished by preference classification, indicates that the prefer and non-prefer EITC populations are statistically indistinguishable in family total income, marital rates, and fertility rates (P-value=0.17, 0.38, and 0.47, respectively). On other accounts, however, the groups are markedly different. Prefer women are approximately nine and a half months younger than non-prefer women. Examination of the age category from Panels B and C shows that the driver for this disparity is the much younger average age of high fertility country of origin EITC recipients who are fully ten years younger than low fertility country of origin EITC recipients. The prominent disparity in age distributions for the subpopulations is makes it vital to use age fixed effects by individual year, rather than multiyear age bins.

In addition to the difference in age, prefer women have larger family sizes than non-prefer women pre-expansion. On average, prefer women have families that are 43 percent higher than their non-prefer counterparts (1.33 children versus 0.93 children). As with age, the difference in family size is driven by the high fertility country of origin immigrant subpopulation; depicted in Panel B, these women have an average family size of 1.73 children. This large family size, combined with the fact that the high fertility immigrant population has a much lower average age, is notable. It demonstrates that high fertility immigrants' revealed preference for comparatively larger families persists, even after coming to the United States. In addition, the younger average age of women from high fertility countries suggests that a greater proportion of these women, as compared with the other groups whose average ages range from 44.9 to 48.9 years, might not have completed fertility. Thus, if the means were balanced by age, it could expected the average number of children per high fertility immigrant woman to be even higher.

The distinctiveness of the high fertility immigrant population is likely due to the ability to more cleanly identify these women by using the residual method in concert with the provided microdata. Conversely, because I use states of residence with high religious adherent populations as my metric for identifying high fertility religious groups, the large family size effects are difficult to isolate. In fact, when comparing high fertility religious adherents to all other women, the

most highly statistically significant difference was marital status, with 42.6 percent of religious adherents being married, and 51.4 percent of all other being married.

Figure 5 Panels A, B, and C, depict the pre-treatment trends for prefer women, high fertility immigrants, and high fertility religious adherents, respectively. While on average, number of children less than five in the treatment and control populations are dissimilar, these averages trend comparably during the pre-treatment period. [10]

VI. Results and Robustness Checks

Because number of children is discrete, I model group fertility as a Poisson distribution. Under my theoretical framework and the specifications of the distribution, I calculate demand for children as the incidence rate ratio (IRR) of childbearing for women in a specific group. The IRR assesses whether the incidence of an event is over-represented given the proportion of the population that the target group comprises. The difference between 1.000 and the calculated IRR is the differential between what is expected (given population proportion) and what occurred. Thus, significant coefficients below (above) 1 indicate that the expansion is associated with a rate of childbearing that is lower (higher) than expected, given the target group's population proportion.

[10] I use the variable "number of children less than five" in order to give a longer birthing history for immigrants. Data on year of immigration is first available for Census year 1994 (calendar year 1993), so by using "number of children less than five" as my outcome, I can extended the fertility observation window back to 1989, thus giving a more comprehensive snapshot of immigrant childbearing.

Table 2 presents regression estimates both for EITC recipients inclusively and EITC recipients who prefer more children. Among recipients generally, the increase in benefit generosity had mixed effects on demand for children. Both under the specification with the no controls (column 1), and my preferred specification with age-by-year controls (column 2), the benefit increase resulted in an approximately 3 percentage point decrease in childrearing. In the third specification, which controls whether a woman's state of residence has a state EITC, the benefit increase resulted in a 1.4 percentage point increase in childrearing. These estimates are in line with the estimates of Baughman and Dickert-Conlin (2003), who find that EITC increases childbearing by close to a percent in certain subpopulations; they are also in line with those of Herbst (2011), who finds that EITC is associated with a 3.7 percentage point reduction in pregnancy and births. Thus, generally speaking, the EITC is not pronatalist.

Table 2 also presents estimates for the prefer subpopulation of EITC recipients. There is a marked difference between the outcomes of prefer versus the general EITC recipients. Under the preferred specification (column 2), prefer recipients exhibit an increased incidence of childrearing in the post expansion period. Prefer women have a 7.5 percentage point higher incidence of childrearing than would be expected, given the percentage of the population they comprise. As contrasted with the general EITC population under specification 2, prefer women are 10.8 percent more likely to have a baby in the

post expansion period.

Tables 3 disaggregates the prefer population by number of children. The greatest increases in credit magnitude occur when women have their first or second child, as the benefit amount always increases for the first child, then increases again for the second child. Women who have their first or second child are always eligible for more money. Third and higher order births are eligible for increases that may accompany the credit's expansion annually, but are not entitled to guaranteed increases for additional childrearing.

Estimates in Table 3 indicate that prefer EITC recipients having their first or second child increased childrearing by 3.2 percent under the preferred specification (column 2). Prefer recipients having a higher order birth also increased childrearing—by 2.3 percent under all specifications.

Table 4 disaggregates the prefer population by marital status, then further separates married recipients into those with one or two children (entitled to more money) and those with three or more children (eligible for increases contingent upon the credit's annual expansion). My preferred specification indicates that married prefer recipients were 3.8 percentage points more likely to have had a baby in the post expansion period than would be expected, given their population proportion. Married prefer recipients having their first or second child were 4.1 percentage points more likely to have had a baby in the post expansion period. Conversely, married prefer recipients

with three or more children demonstrate no significant change in their number of children in the post expansion period.

Table 5 estimates the effect of the EITC credit expansion on unmarried women generally, then further separates unmarried credit recipients into those entitled to more money (women with one or two children) and those only eligible for higher benefits when the credit undergoes its annual expansion (those with three or more children). Specification 2--my preferred specification--indicates that generally, unmarried prefer recipients were no more likely to give birth in the post-expansion era than other women. In fact, specification 3 indicates that unmarried prefer recipients were less likely to give birth in the post-expansion period, given the percentage of the population they comprise. When I separate the estimates by number of children, however, I find that unmarried prefer women who had two or more children prior to the expansion exhibited a marked significant fertility response. Specifically, post-expansion, unmarried prefer women gave birth at a rate 15.5 percent higher than would be expected given the percentage of the proportion that they comprise.

Tables 6 and 7 disaggregate the results by prefer subgroup. Table 6 indicates that the increased incidence of childbearing among married recipients is driven by high fertility immigrant women; post reform, this group had a 10.1 percentage point increased incidence of childbearing under the preferred specification. Table 7 indicates that the increased incidence of childbearing

among unmarried persons (16.4 percentage points higher under specification 2) is driven by women from high fertility religious groups having higher order births. While unmarried women from high fertility immigrant groups had a much lower incidence of childbearing given their proportion of the population (34.4 percentage points lower, under specification 2, Table 6), further state-specific breakouts in table 8 indicate that the increased incidence of unmarried childbearing was driven by women from Utah, Idaho, and Wyoming, states with high LDS populations.

To verify that preference was a key motivator for increasing childbearing in the wake of the expansion, I rank-ordered the poorest states by income over the period. I then made residence in one of these six lowest income states its own treatment group. If state price level is correlated with average state income, then the nationally homogeneous EITC increase would have had a disproportionate effect on women from low-income states because increased benefits will "go further" in poor states. Thus, women in these states might be more likely to increase incidence of childrearing than their EITC recipient peers in higher average income states, as poorer state EITC-recipients' real cost of childrearing is reduced by a greater amount. Results from this "Low-Income State" treatment group are presented in Table 9.

As Table 9 indicates, neither married nor unmarried EITC recipient women from low-income states showed a statistically significant

disproportionate rate of childbearing given the percent of the population that they comprise, though the magnitudes of some coefficient estimates are quite large.

It is possible that women from the highest fertility countries of origin exhibit responses that are correspondingly greater in magnitude and/or or more consistently affirmative (and therefore of higher statistical significance) as compared with their peers, given that these women might be the most accustomed to larger families. That is, women's preference for children might be directly correlated with average family size in their country of origin. Following this logic, women who hail from a country where the average number of children is six or higher might have a greater preference for large families than those women who hail from countries where the average number of children is four or five children. To investigate whether women from the *highest* fertility countries of origin exhibit an amplified differential fertility response—in excess of the prefer response already investigated--relative to other women, I divide respondents into quartiles based upon the completed fertility rate of their country of origin. I denote foreign-born authorized women surveyed in the post-expansion period who hail from second and third quartile countries as treated, and all other women as control. I present these estimates in Table 10.

While several specifications that disaggregate women by number of children suggest that highest fertility country of origin women's responses are

statistically indistinguishable from zero, I find that overall, these women are less likely than their peers to increase family size in the wake of the benefit expansion. Unmarried women from the highest fertility countries are, on average two-thirds less likely as control group unmarried women to increase childbearing, while married women are roughly twenty-five percent less likely.[11]

Finally, as a further robustness check, and to guard against the possibility that systematic fluctuations in the fertility of control group women could introduce omitted variable bias, I restrict the control group to women whose academic background more closely parallels the treatment group's. Thus, I limit the control group to women who have no more than some college, and investigate whether, given this restricted control group, treatment cohort fertility effects persist. I present these results in Table 11.

Generally, limiting the control group increases the magnitude and significance of the estimates, and decreases the variation in estimate magnitude across specifications. Using the less restrictive control group under specification 2, married women have a 3.8 percentage point higher incidence of childbirth in the post-recession period (see table 4). Using the more restrictive control group, however, more than triples the incidence of childbearing in the post-recession

[11] There may be insufficient granularity in census reporting to accurately capture the country of origin of some persons from high-fertility countries. Seven of the ten highest fertility 3rd quartile countries are classed under either "Africa—not classified and not elsewhere contained" or "Asia—not classified and not elsewhere contained".

period, and results in a 14.3 percentage point increase in the incidence of childrearing in the post-recession period. Using the less restrictive control group under specification 2, unmarried women reporting a higher order birth have a 15.5 percentage point higher incidence of childbirth in the post-recession period (see table 5). This result is estimated with 90 percent confidence. While using the more restrictive control group only increases the incidence of childbearing in the post-recession period by 0.09 percentage points, the statistical significance level rises from the 90 percent to the 95 percent confidence level.

In summary, prefer women generally, prefer women with various numbers of children, and married prefer women all increase childrearing in response to the credit expansion; the greatest increase post-expansion is from unmarried women giving birth to their third or higher parity birth. These findings are robust to three specifications, and two control groups.

VII. Discussion

Though my estimates indicate that increased income associated with childrearing increases demand for children, the reality that childrearing involves a nontrivial time and monetary investment makes it necessary to ask whether rational actors would consider the increased income enough to alter their demand structure. For example, potential large financial costs associated with a child's first year of life include hospitalization during birth and recovery,

mother's loss of income due to unpaid maternity leave, and childcare. As such, though prefer women exhibit largely statistically significant positive fertility responses to the EITC expansion, a logical question is whether the credit amount is sufficient to increase childbearing. There are several reasons why rational agents could choose to respond to the credit increase by increasing fertility

First, parents have great discretion over the costs associated with childrearing. One method of calculating the cost of childrearing is to use the "average cost of an additional bedroom" approach (Lino 2013). This method estimates the housing cost for childrearing as the additional cost—including furniture and utilities--associated with having an extra bedroom in the family's home. In addition, the additional bedroom approach assumes that children in two child families do not share a room. While this method might effectively estimate the cost of childrearing for some families—Lino (2013) asserts that such a method would likely underestimate housing costs for families who chose specific neighborhoods for schools and particular homes for larger yards—it likely overestimates housing costs for lower income families, who might choose to economize by having multiple family members sharing rooms.

Second, women who meet the EITC means test might also qualify for other means tested programs--such as Women, Infant, and Children (WIC), Supplementary Nutrition Assistance Program (SNAP), Medicaid, and Assistance to Families with Dependent Children/ Temporary Assistance for Needy Families

(AFDC/TANF)—that offset the cost of hospitalization and provide food benefits and income. In addition, kinship networks; unlicensed, in-home providers; and federally-funded daycares are viable options for low or no-cost childcare. Indeed, Lino, et al. (2017) assert that the cost of childrearing and family income are positively correlated.

Third, the EITC credit amount did not differentiate by marital status during the years in this paper's estimation window. Thus, the credit imposed a default marriage penalty on taxpayers, as a married family with two parents and costs commiserate with spending for two adults would be eligible for the same credit as a single/head of household family with costs commiserate with spending for one adult (holding number of children equal). To better illustrate this penalty, suppose that two one-child couples—one married, and one unmarried—file their 1994 taxes. If the married couple, who necessarily files jointly in order to be eligible for the EITC, has a joint income that falls within the maximum credit range, then the family's earnings can be no greater than $11,000. If the unmarried couple's individual incomes fall within the maximum credit range, then because they necessarily file separately, their combined income can be as much as $11,000 X 2 or $22,000, double that the married couple's income. If the married couple decides to have an additional child, then their credit for two children will be $2,528, a 76.8% increase over the previous year's pre-expansion one child credit of $1,424. If the unmarried couple decides

to have an additional child and each parent claims one child, then the couple's combined credit will be $2038 per child X 2, for a total credit of $4076 for two children. Thus, relative to married families, the EITC tax schedule and credit expansion was more generous to unmarried families during the treatment period, providing substantial monetary concessions for additional childrearing. It therefore follows that unmarried prefer women would exhibit the greatest fertility responses to credit increases.

Children require considerable emotional, temporal, and financial outlays, and it is not reasonable to assert that rational actors would increase childrearing with the thought that the EITC would completely finance the cost of childrearing. For women on the margins who were open to having more children but might have needed additional funds to do so, however, my estimates indicate that the credit increase made increased childbearing feasible.

VIII. Conclusion

Like the majority of literature in the field, I find that the expansion of the EITC resulted in only slight changes in birthing compared to the rest of the population. Under my preferred specification, the expansion was associated with a 3.2 percentage point decrease in birthing within the target population. These average treatment effects do not distinguish for fertility by preference for children, however.

This paper's contribution is a novel way to examine heterogeneous fertility effects using demographic group revealed preference for larger families as a proxy for group preference for increasing family size. Under my triple differences specification that interacts EITC receipt in the post-expansion period with preference, I find that among women who prefer increasing family size, the credit increase was associated with as much as a sixteen percent increased incidence of childbearing. Further disaggregation of the preference groups indicates that the increased incidence of childbearing among religious adherents was the driver for first and second births, while increased incidence of childbearing among high fertility immigrants was the driver for third and higher births.

The ramifications of an anti-poverty program being unintentionally pro-natalist are potentially far-reaching. Children in poverty are more likely to be obese, have diagnosed depression and anxiety, and be born into families where the mother is in poor health (Ekono, et al. 2016). The EITC is amount can be substantial, and recipients may qualify for additional means-tested benefits that further defray the cost of childrearing. Nevertheless, the reason EITC recipients qualify receive the credit is to supplement their low incomes. If these combined entitlements do not fully offset the cost of additional childrearing, families who respond to the credit increase by having additional children will be worse off

financially, and the anti-poverty measure will have resulted in an outcome opposite of its intent.

Year	Children	A	B	C	EITC	+$
1993	0	-	-	-	-	0
	1	7,750	12,200	23,050	1,434	1,434
	2+	7,750	12,200	23,050	1,511	77
1994	0	4,000	5,000	9,000	306	306
	1	7,750	11,000	23,755	2,038	1,732
	2+	8,425	11,000	25,296	2,528	796
1995	0	4,100	5,130	9,230	314	314
	1	6,160	11,290	24,396	2,094	1,780
	2+	8,640	11,290	26,673	3,110	1,330
1996	0	4,220	5,280	9,500	323	323
	1	6,330	11,610	25,078	2,152	1,829
	2+	8,890	11,610	28,495	3,556	1,727
1997	0	4,340	5,430	9,770	332	332
	1	6,500	11,930	25,750	2,210	1,878
	2+	9,140	11,930	29,290	3,656	1,778
1998	0	4,460	5,570	10,030	341	341
	1	6,680	12,260	26,473	2,271	1,930
	2+	9,390	12,260	30,095	3,756	1,826
1999	0	4,530	5,670	10,200	347	347
	1	6,800	12,460	26,928	2,312	1,965
	2+	9,540	12,460	30,580	3,816	1,851

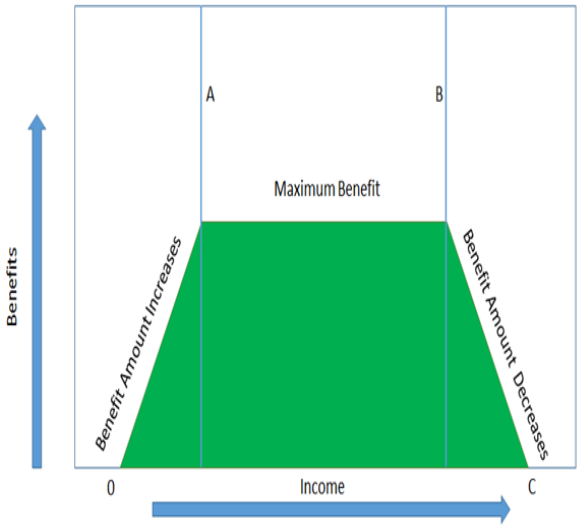


Figure 1: EITC Income Schedule

Note: *The EITC schedule varies according to year, income, and number of children. This figure is not exactly to scale: The slopes of the given phase in and phase out regions—labeled "benefit amount increases" and "benefit amount decreases"--vary according to number of children.*

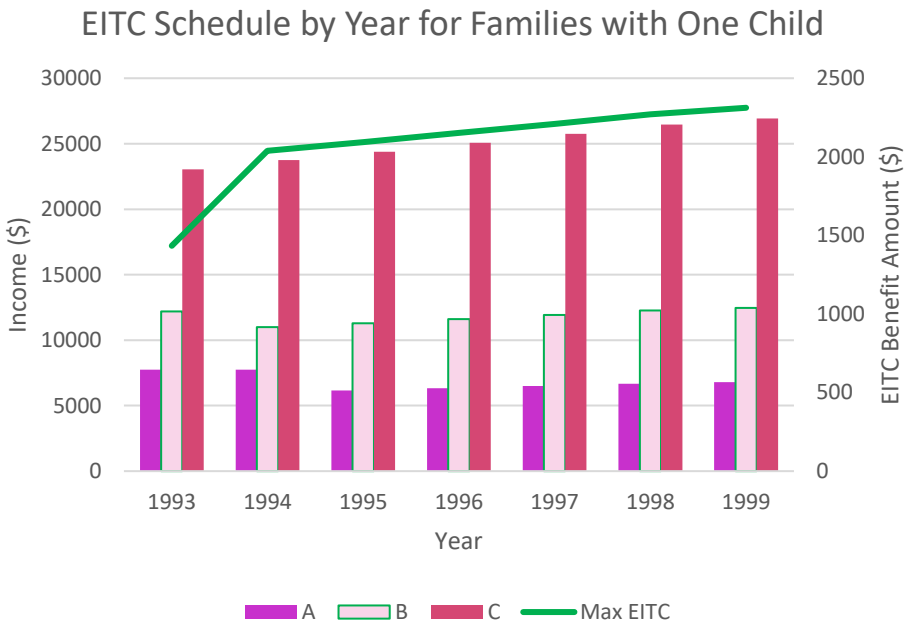


Figure 2: EITC Schedule by Year, One Child

Note: *The EITC schedule varies according to year, income, and number of children. The letter "A" represents the* ***minimum*** *earnings needed to receive the maximum EITC. The letter "B" represents the* ***maximum*** *earnings allowed to receive the maximum EITC. The letter "C" represents the* ***maximum*** *earnings allowed to receive any EITC. This chart shows values for a family claiming only one child.*

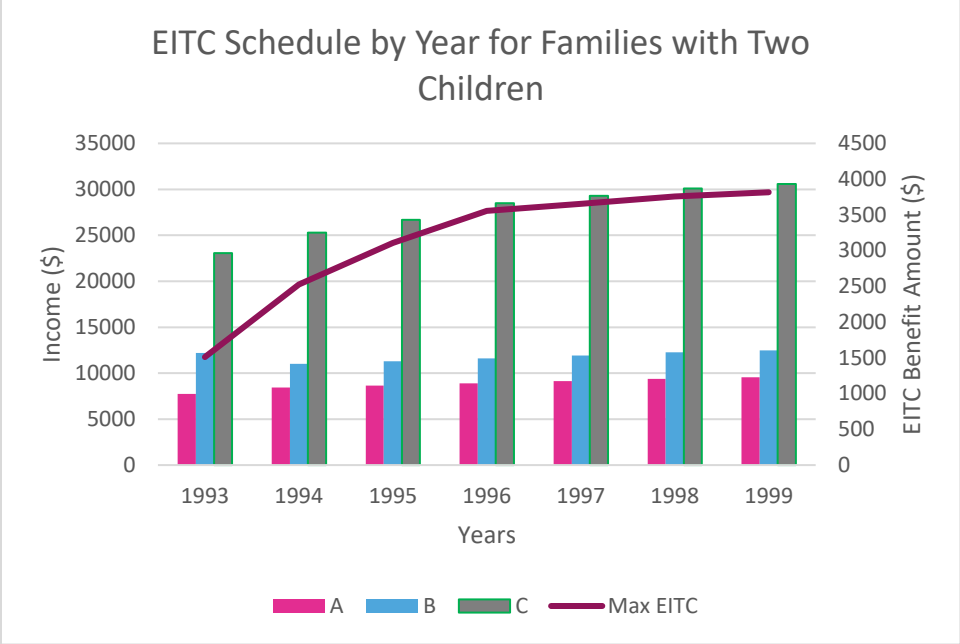


Figure 3: EITC Schedule by Year, Two Children

Note: *The EITC schedule varies according to year, income, and number of children. The letter "A" represents the* ***minimum*** *earnings needed to receive the maximum EITC. The letter "B" represents the* ***maximum*** *earnings allowed to receive the maximum EITC. The letter "C" represents the* ***maximum*** *earnings allowed to receive any EITC. This chart shows values for a family with two or more children.*

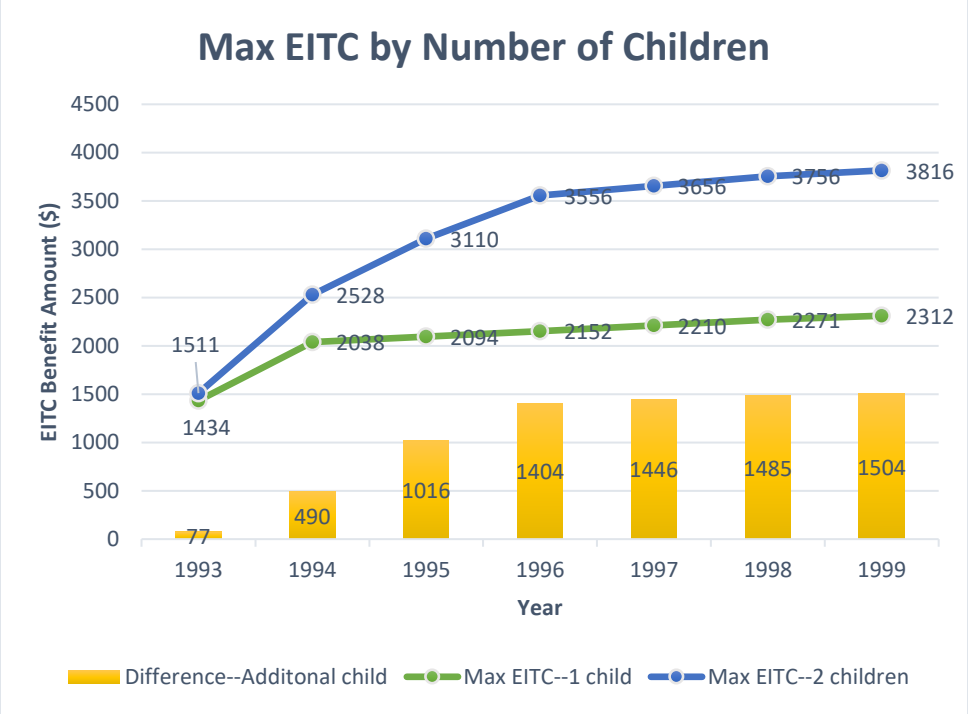
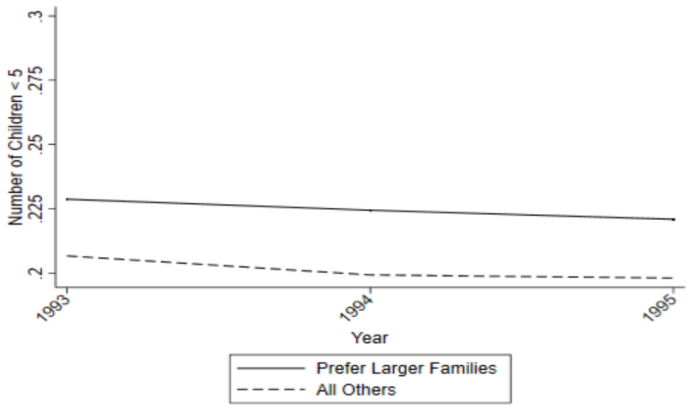
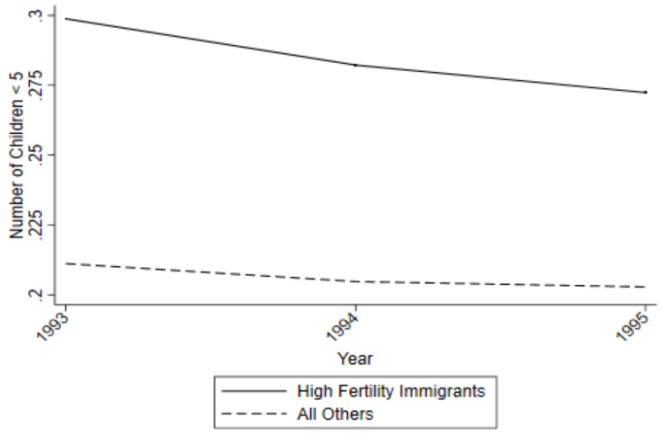


Figure 4: Maximum EITC benefit amount by Year

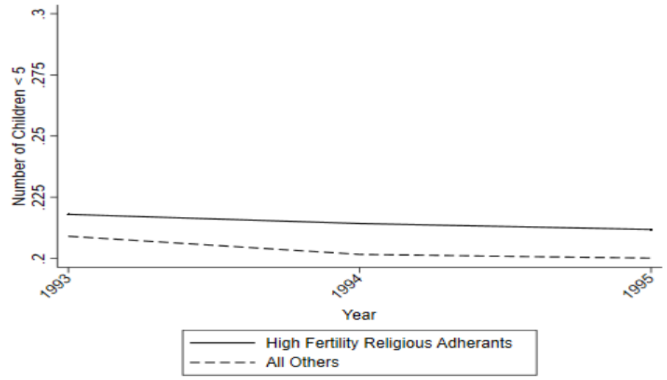
Note: *The EITC benefit varies according to year, income, and number of children. This chart shows the progression of benefit generosity over time according to the number of children a family claims. In the pre-expansion period (1993), the maximum benefit for a family with 2 or more children was only 77 dollars more than the maximum for a family with one child. Over the expansion period, however, the difference in benefit generosity widened as a result of the steadily increasing benefit for 2+ child families.*



Panel A: All Prefer Women



Panel B: High Fertility Immigrants



Panel C: High Fertility Religious Adherents

Figure 5: Fertility Trends of Number of Children Less than Five by Subgroup

***Note**: Within each of the three subgroups, treatment and control cohorts trended similarly in the pre-treatment period.*

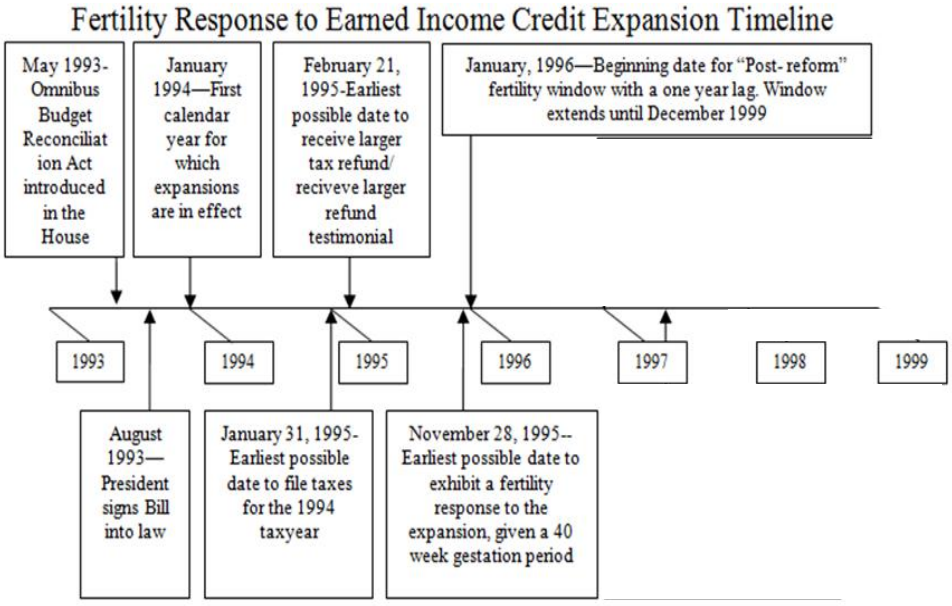


Figure 6: EITC Fertility Timeline

Note: *Fertility decisions made in the years 1993-1995 are considered the control period, while fertility decisions made in the years 1996-1999 are considered the treatment period.*

Table 1: Pre-Period (before 1997) Summary Statistics

Variable	Prefer*EITC	Non-Prefer*EITC	Difference (Prefer-Non-Prefer)	P-value Ho: diff = 0 Ha: diff != 0
Family's total income	24412.01	23955.83	456.18	Pr(|T| > |t|) = 0.17
Married	0.507	0.501	0.006	Pr(|T| > |t|) = 0.38
Number of Children	1.332	0.932	0.400	Pr(|T| > |t|) = 0.00
Age	44.930	45.713	0.783	Pr(|T| > |t|) = 0.00
Number of children <1	0.036	0.038	-0.002	Pr(|T| > |t|) = 0.47
Observations	7,199	27,419	20,220	NA

Panel A: Prefer EITC Recipients versus Non-Prefer EITC Recipients

Variable	High fertility immigrant* EITC	Low fertility immigrant* EITC	Difference (High-Low)	P-value Ho: diff = 0 Ha: diff != 0
Family's total income	21896.30	26988.49	-5092.20	Pr(|T| > |t|) = 0.00
Married	0.625	0.609	0.016	Pr(|T| > |t|) = 0.28
Number of Children	1.731	1.009	0.722	Pr(|T| > |t|) = 0.00
Age	38.059	48.913	-10.854	Pr(|T| > |t|) = 0.00
Number of children <1	0.076	0.032	0.044	Pr(|T| > |t|) = 0.00
Observations	6,415	1,201	5,214	NA

Panel B: High Fertility COO EITC Recipients versus Low Fertility COO EITC Recipients

Note: *Women aged 15-44 are represented in both the authorized and unauthorized populations in the years 1993-1995 (CPS years 1994-1996). Because of the disparity between year of immigration (1981 or earlier) and year of birth, women age 15 are omitted from the 1997 (CPS year 1998) sample, women ages 15-16 are omitted from the 1998 (CPS year 1999 sample), and women age 15-17 are omitted from the 1999 (CPS year 2000 sample).*

Table 1: Pre-Period (before 1997) Summary Statistics (continued)

Variable	Religious* EITC	Non-Religious* EITC	Difference (Religious-Non-Religious)	P-value Ho: diff = 0 Ha: diff != 0
Family's total income	24271.630	24015.590	256.034	Pr(|T| > |t|) = 0.514
Married	0.427	0.514	0.087	Pr(|T| > |t|) = 0.000
Number of Children	1.003	1.017	-0.014	Pr(|T| > |t|) = 0.509
Age	45.689	45.529	0.160	Pr(|T| > |t|) = 0.562
Number of children <1	0.036	0.034	0.001	Pr(|T| > |t|) = 0.092
Observations	4,746	29,872	-140,011	NA

Panel C: High Fertility Religious EITC Recipients versus Non-High Fertility Religious EITC Recipients

Note: *Women aged 15-44 are represented in both the authorized and unauthorized populations in the years 1993-1995 (CPS years 1994-1996). Because of the disparity between year of immigration (1981 or earlier) and year of birth, women age 15 are omitted from the 1997 (CPS year 1998) sample, women ages 15-16 are omitted from the 1998 (CPS year 1999 sample), and women age 15-17 are omitted from the 1999 (CPS year 2000 sample).*

Table 2: Incidence Rate Ratio for Births in the Post-Expansion Period

		(1)	(2)	(3)
EITC recipients		0.969***	0.970***	1.015***
		(0.001)	(0.001)	(0.001)
EITC recipients*Prefer		1.047***	1.025***	1.025***
		(0.008)	(0.008)	(0.008)
No controls		X		
Age indicator			X	X
State EITC				X
EITC recipients	Observations	11,632	11,632	11,632
	Pseudo R^2	0.003	0.003	0.003
EITC recipients*Prefer	Observations	11,632	11,632	11,632
	Pseudo R^2	0.013	0.118	0.118

Note: *"Prefer" includes two main groups: (1) immigrants from 133 high fertility countries (those with a higher fertility rate than the year 2000 U.S. rate of 2.056 children/woman) and (2) women from regions with a high concentration of high fertility religious groups (states of Utah, Idaho, New York, New Jersey, and Wyoming).*
*Significant at the ***1%, **5%, and *10% level.*

Table 3: Incidence Rate Ratio for Births to Prefer EITC Recipients in the Post-Expansion Period by Number of Children

		(1)	(2)	(3)
Eligible for more money (1^{st} or 2^{nd} birth)		1.045*** (0.009)	1.023*** (0.009)	1.023*** (0.009)
Higher parity (3^{rd} + birth)		1.032** (0.014)	1.032** (0.014)	1.032** (0.014)
No controls		X		
Age fixed effects			X	X
State EITC control				X
Eligible for more money	Observations	8,508	8,508	8,508
	Pseudo R^2	0.045	0.146	0.146
Higher parity	Observations	3,124	3,124	3,124
	Pseudo R^2	0.011	0.011	0.011

Note: *"Prefer" includes two main groups: (1) immigrants from 133 high fertility countries (those with a higher fertility rate than the year 200 U.S. rate of 2.056 children/woman) and (2) women from regions with a high concentration of high fertility religious groups (states of Utah, Idaho, New York, New Jersey, and Wyoming).*

*Significant at the ***1%, **5%, and *10% level*

Table 4: Incidence Rate Ratio for Births to Prefer EITC Recipients in the Post-Expansion Period by Number of Children Born to Married Women

		(1)	(2)	(3)
Married		1.042*** (0.008)	1.038*** (0.008)	1.038*** (0.008)
Married—more money		1.041*** (0.010)	1.041*** (0.010)	1.041*** (0.010)
Married—higher order		1.012 (0.014)	1.012 (0.014)	1.012 (0.014)
No controls		X		
Age fixed effects			X	X
State EITC control				X
Married	Observations	9,319	9,319	9,319
	Pseudo R^2	0.019	0.042	0.043
Married—more money	Observations	6,785	6,785	6,785
	Pseudo R^2	0.178	0.179	0.179
Married—higher order	Observations	2,534	2,534	2,534
	Pseudo R^2	0.008	0.008	0.037

Note: *"Prefer" includes two main groups: (1) immigrants from 133 high fertility countries (those with a higher fertility rate than the year 200 U.S. rate of 2.056 children/woman) and (2) women from regions with a high concentration of high fertility religious groups (states of Utah, Idaho, New York, New Jersey, and Wyoming).*

*Significant at the ***1%, **5%, and *10% level*

Table 5: Incidence Rate Ratio for Births to Prefer EITC Recipients in the Post-Expansion Period by Number of Children Born to Unmarried Women

		(1)	(2)	(3)
Unmarried		0.980 (0.026)	0.963 (0.026)	0.930** (0.025)
Unmarried—more money		1.009 (0.029)	1.009 (0.029)	1.010 (0.029)
Unmarried—higher parity		1.163** (0.090)	1.155* (0.090)	1.155* (0.090)
No controls		X		
Age fixed effects			X	X
State EITC control				X
Unmarried	Observations	2,313	2,313	2,313
	Pseudo R^2	0.010	0.044	0.046
Unmarried—more money	Observations	1,723	1,723	1,723
	Pseudo R^2	0.063	0.063	0.063
Unmarried—higher order	Observations	590	590	590
	Pseudo R^2	0.196	0.199	0.199

Note: *"Prefer" includes two main groups: (1) immigrants from 133 high fertility countries (those with a higher fertility rate than the year 200 U.S. rate of 2.056 children/woman) and (2) women from regions with a high concentration of high fertility religious groups (states of Utah, Idaho, New York, New Jersey, and Wyoming).*

*Significant at the ***1%, **5%, and *10% level*

Table 6: Incidence Rate Ratio for Births to Immigrant Prefer EITC Recipients in the Post-Expansion Period by Number of Children

		(1)	(2)	(3)
Married		1.092** (0.047)	1.101 ** (0.048)	1.101*** (0.048)
Married—more money		1.052 (0.071)	1.052 (0.071)	1.052 (0.071)
Married—higher order		1.006 (0.066)	1.006 (0.066)	1.005 (0.066)
Unmarried		0.703** (0.113)	0.656*** (0.106)	0.633*** (0.102)
Unmarried—more money		1.025 (0.178)	1.027 (0.178)	1.026 (0.178)
Unmarried—higher order		Omitted due to collinearity	Omitted due to collinearity	Omitted due to collinearity
No controls		X		
Age fixed effects			X	X
State EITC control				X
Married	Observations	9,319	9,319	9,319
	R^2	0.013	0.037	0.037
Married—more money	Observations	6,785	6,785	6,785
	R^2	0.200	0.200	0.200
Married—higher order	Observations	2,534	2,534	2,534
	R^2	0.009	0.009	0.009
Unmarried	Observations	2,313	2,313	2,313
	R^2	0.009	0.043	0.044
Unmarried—more money	Observations	1,723	1,723	1,723
	R^2	0.025	0.025	0.025
Unmarried—higher order	Observations	590	590	590
	R^2	0.153	0.158	0.158

Note: *"Prefer" includes two main groups: (1) immigrants from 133 high fertility countries (those with a higher fertility rate than the year 200 U.S. rate of 2.056 children/woman) and (2) women from regions with a high concentration of high fertility religious groups (states of Utah, Idaho, New York, New Jersey, and Wyoming). Families are eligible for "more money" if they are increasing family size from 0 to 1 children or from 1 to 2 children. "Higher order" families—those that claim 3 or more children-- do not receive additional benefits for increasing family size.*

*Significant at the ***1%, **5%, and *10% level*

Table 7: Incidence Rate Ratio for Births to Religious Prefer EITC Recipients in the Post-Expansion Period by Number of Children

		(1)	(2)	(3)
Married		1.044***	1.030***	1.029***
		(0.010)	(0.010)	(0.010)
Married—more money		1.040***	1.040***	1.040***
		(0.012)	(0.012)	(0.012)
Married—higher order		1.017	1.017	1.017
		(0.021)	(0.021)	(0.021)
Unmarried		1.064**	1.048	1.017
		(0.033)	(0.032)	(0.032)
Unmarried—more money		1.007	1.007	1.007
		(0.033)	(0.033)	(0.033)
Unmarried—higher order		1.164*	1.164*	1.164*
		(0.105)	(0.105)	(0.105)
No controls		X		
Age fixed effects			X	X
State EITC control				X
Married	Observations	9,319	9,319	9,319
	R^2	0.019	0.044	0.044
Married—more money	Observations	6,785	6,785	6,785
	R^2	0.209	0.210	0.210
Married—higher order	Observations	2,534	2,534	2,534
	R^2	0.031	0.031	0.031
Unmarried	Observations	2,313	2,313	2,313
	R^2	0.010	0.043	0.045
Unmarried—more money	Observations	1,723	1,723	1,723
	R^2	0.017	0.017	0.017
Unmarried—higher order	Observations	590	590	590
	R^2	0.189	0.194	0.194

Note: *"Prefer" includes two main groups: (1) immigrants from 133 high fertility countries (those with a higher fertility rate than the year 200 U.S. rate of 2.056 children/woman) and (2) women from regions with a high concentration of high fertility religious groups (states of Utah, Idaho, New York, New Jersey, and Wyoming). Families are eligible for "more money" if they are increasing family size from 0 to 1 children or from 1 to 2 children. "Higher order" families—those that claim 3 or more children-- do not receive additional benefits for increasing family size.*

*Significant at the ***1%, **5%, and *10% level*

Table 8: Incidence Rate Ratio for Births to EITC Recipients in Wyoming, Idaho, and Utah in the Post-Expansion Period by Number of Children

		(1)	(2)	(3)
Married		1.087***	1.062***	1.062***
		(0.0245)	(0.024)	(0.024)
Married—more money		1.043	1.042	1.042
		(0.028)	(0.028)	(0.028)
Married—higher order		1.025	1.025	1.025
		(0.047)	(0.048)	(0.047)
Unmarried		1.439***	1.352***	1.294**
		(0.168)	(0.158)	(0.152)
Unmarried—more money		1.027	1.027	1.027
		(0.142)	(0.142)	(0.142)
Unmarried—higher order		1.152	1.160	1.160
		(0.405)	(0.409)	(0.409)
No controls		X		
Age fixed effects			X	X
State EITC control				X
Married	Observations	9,319	9,319	9,319
	R^2	0.019	0.044	0.044
Married—more money	Observations	6,785	6,785	6,785
	R^2	0.209	0.210	0.210
Married—higher order	Observations	2,534	2,534	2,534
	R^2	0.031	0.031	0.031
Unmarried	Observations	2,313	2,313	2,313
	R^2	0.010	0.043	0.045
Unmarried—more money	Observations	1,723	1,723	1,723
	R^2	0.017	0.017	0.017
Unmarried—higher order	Observations	590	590	590
	R^2	0.189	0.194	0.194

Note: *"Prefer" includes two main groups: (1) immigrants from 133 high fertility countries (those with a higher fertility rate than the year 200 U.S. rate of 2.056 children/woman) and (2) women from regions with a high concentration of high fertility religious groups (states of Utah, Idaho, New York, New Jersey, and Wyoming). Families are eligible for "more money" if they are increasing family size from 0 to 1 children or from 1 to 2 children. "Higher order" families—those that claim 3 or more children-- do not receive additional benefits for increasing family size. Significant at the ***1%, **5%, and *10% level*

Table 9: Robustness Check--Incidence Rate Ratio for Births to EITC Recipients in Low- Income States in the Post-Expansion Period by Number of Children

		(1)	(2)	(3)
All		1.170	1.214	1.223
		(0.198)	(0.205)	(0.207)
More money		1.116	1.125	1.134
		(0.256)	(0.259)	(0.261)
Higher order		1.055	1.055	1.054
		(0.310)	(0.310)	(0.310)
Married		1.166	1.189	1.192
		(0.205)	(0.210)	(0.210)
Married—more money		1.043	1.047	1.047
		(0.264)	(0.265)	(0.265)
Married—higher order		1.007	1.007	1.007
		(0.297)	(0.297)	(0.297)
Unmarried		0.888	0.966	0.966
		(0.750)	(0.815)	(0.815)
Unmarried—more money		1.026	1.028	1.028
		(0.888)	(0.890)	(0.890)
Unmarried—higher order		omitted	omitted	omitted
No controls		X		
Age fixed effects			X	X
State EITC control				X
All	Observations	11,632	11,632	11,632
	R^2	0.011	0.115	0.116
All—more money	Observations	8508	8508	8508
	R^2	0.046	0.140	0.141
All—higher order	Observations	3124	3124	3124
	R^2	0.014	0.014	0.014
Married	Observations	9319	9319	9319
	R^2	0.015	0.042	0.042
Married—more money	Observations	6785	6785	6785
	R^2	0.194	0.195	0.195
Married—higher order	Observations	2534	2534	2534
	R^2	0.008	0.008	0.008
Unmarried	Observations	2313	2313	2313
	R^2	0.010	0.044	0.044
Unmarried—more money	Observations	1723	1723	1723
	R^2	0.0240	0.024	0.024
Unmarried—higher order	Observations	590	590	590
	R^2	0.162	0.167	0.167

Note: *"Prefer" includes two main groups: (1) immigrants from 133 high fertility countries (those with a higher fertility rate than the year 200 U.S. rate of 2.056 children/woman) and (2) women from regions with a high concentration of high fertility religious groups (states of Utah, Idaho, New York, New Jersey, and Wyoming). Families are eligible for "more money" if they are increasing family size from 0 to 1 children or from 1 to 2 children. "Higher order" families—those that claim 3 or more children-- do not receive additional benefits for increasing family size. Significant at the ***1%, **5%, and *10% level*

Table 10: Incidence Rate Ratio for Births to EITC Recipients Highest Fertility (3rd and 2nd Quartile) Groups in the Post-Expansion Period by Number of Children Born

		(1)	(2)	(3)
All		0.656***	0.590***	0.592***
		(0.053)	(0.048)	(0.048)
More money		0.851*	0.759***	0.761***
		(0.081)	(0.072)	(0.072)
Higher order		0.952	0.956	0.956
		(0.274)	(0.275)	(0.275)
Married		0. 794**	0.764***	0.764***
		(0.066)	(0.064)	(0.063)
Married—more money		1.046	1.046	1.046
		(0.101)	(0.101)	(0.101)
Married—higher order		1.005	1.005	1.005
		(0.302)	(0.302)	(0.302)
Unmarried		0.385**	0.325**	0.326**
		(0.177)	(0.150)	(0.150)
Unmarried—more money		1.012	1.009	1.009
		(0.584)	(0.583)	(0.583)
Unmarried—higher order		omitted	omitted	omitted
No controls		X		
Age fixed effects			X	X
State EITC control				X
All	Observations	11,632	11,632	11,632
	R^2	0.011	0.115	0.116
All--more money	Observations	8,508	8,508	8,508
	R^2	0.047	0.141	0.142
All—higher order	Observations	3,124	3,124	3,124
	R^2	0.010	0.010	0.010
Married	Observations	9,319	9,319	9,319
	R^2	0.015	0.042	0.042
Married—more money	Observations	6,785	6,785	6,785
	R^2	0.209	0.210	0.210
Married—higher order	Observations	2,534	2,534	2,534
	R^2	0.005	0.005	0.005
Unmarried	Observations	2313	2313	2313
	R^2	0.010	0.043	0.043
Unmarried—more money	Observations	1723	1723	1723
	R^2	0.020	0.020	0.020
Unmarried—higher order	Observations	590	590	590
	R^2	0.162	0.166	0.166

Note: *"Prefer" includes two main groups: (1) immigrants from 133 high fertility countries (those with a higher fertility rate than the year 200 U.S. rate of 2.056 children/woman) and (2) women from regions with a high concentration of high fertility religious groups (states of Utah, Idaho, New York, New Jersey, and Wyoming). Families are eligible for "more money" if they are increasing family size from 0 to 1 children or from 1 to 2 children. "Higher order" families—those that claim 3 or more children-- do not receive additional benefits for increasing family size. Significant at the ***1%, **5%, and *10% level*

Table 11: Robustness Check--Incidence Rate Ratio for Births to EITC Recipients in the Post-Expansion Period by Number of Children Born
Control Group: Women with some college or less education

		(1)	(2)	(3)
All		1.043***	1.058***	1.062***
		(0.010)	(0.010)	(0.010)
More money		0.978*	0.997	0.998
		(0.012)	(0.012)	(0.012)
Higher order		1.022	1.022	1.022
		(0.016)	(0.016)	(0.016)
Married		1.136***	1.143***	1.143***
		(0.011)	(0.011)	(0.011)
Married—more money		1.067***	1.067***	1.067***
		(0.014)	(0.014)	(0.014)
Married—higher order		0.989	0.989	0.989
		(0.016)	(0.016)	(0.016)
Unmarried		0.968	0.950	0.946
		(0.026)	(0.025)	(0.025)
Unmarried—more money		1.029**	1.029**	1.029**
		(0.030)	(0.030)	(0.030)
Unmarried—higher order		1.169**	1.164**	1.164**
		(0.081)	(0.081)	(0.081)
No controls		X		
Age fixed effects			X	X
State EITC control				X
All	Observations	7,795	7,795	7,795
	R^2	0.006	0.110	0.111
All—more money	Observations	5,485	5,485	5,485
	R^2	0.048	0.138	0.138
All—higher order	Observations	2,310	2,310	2,310
	R^2	0.111	0.112	0.112
Married	Observations	5,681	5,681	5,681
	R^2	0.012	0.049	0.049
Married—more money	Observations	3,921	3,921	3,921
	R^2	0.235	0.236	0.236
Married—higher order	Observations	1,760	1,760	1,760
	R^2	0.116	0.116	0.116
Unmarried	Observations	2114	2114	2114
	R^2	0.024	0.063	0.063
Unmarried—more money	Observations	1564	1564	1564
	R^2	0.238	0.238	0.238
Unmarried—higher order	Observations	550	550	550
	R^2	0.262	0.265	0.265

Note: *"Prefer" includes two main groups: (1) immigrants from 133 high fertility countries (those with a higher fertility rate than the year 200 U.S. rate of 2.056 children/woman) and (2) women from regions with a high concentration of high fertility religious groups (states of Utah, Idaho, New York, New Jersey, and Wyoming). Families are eligible for "more money" if they are increasing family size from 0 to 1 children or from 1 to 2 children. "Higher order" families—those that claim 3 or more children-- do not receive additional benefits for increasing family size. Significant at the ***1%, **5%, and *10% level*

CHAPTER 3: NON-INCOME BEHAVIORAL RESPONSES IN THE WAKE OF THE 1994 EITC EXPANSION

I. Introduction

The Earned Income Tax Credit (EITC) is one of the largest anti-poverty programs in America. However, as with all tax programs, there is always concern that behavioral responses by the target population to maximize their benefit from the program will be counter to the program's goals or generate non-compliant overpayments. Previous investigation into this topic primarily focuses on how workers increase their EITC tax benefit by smoothing the reporting of self-employed earnings or adjusting their work decisions on the intensive margin.(See LaLumia, 2009; Saez, 2010; Jones, 2013; Chetty, 2014).

Less work has been done, however, on non-income based strategies to minimize tax liability via "refund maximization"—the process of making strategic filing decisions, some of which are potentially noncompliant--that lower a taxpayer's tax liability. Edin, et al.(2013) gives anecdotal evidence that EITC recipients alter their tax status in order maximize their refunds by increasing withholdings on their W-4 forms, filing returns as head of household rather than married, and sharing dependents among multiple caregivers.

One of the shortcomings in the current literature on non-income based refund maximization is that many papers present estimates as a snapshot in time rather than as evolving window. This static analysis prevents inference of how taxpayers receive, disseminate, and incorporate information about a decidedly complex credit program. For example, using compliance data, McCubbin(2000) finds that the incidence of misreporting number of children is increasing in EITC size and tax benefit, but due to data limitations, only examines tax year 1994.

Blumenthal, et al. (2005) finds that approximately one-third of EITC claimants were actually ineligible for the credit, but, again due to data limitations, only analyzes tax year 1988.

This paper seeks to fill this gap by presenting year-over-year estimates, difference-in-differences estimates, and triple differences estimates of three non-income based taxpayer responses to the 1994 EITC expansions over the 4 year post-event window following the expansion. Specifically, I investigate whether taxpayers responded to credit increases by altering their filing status, sharing dependents among kinship networks, or patronizing paid preparers. I use administrative data from the Internal Revenue Service (IRS) Public Use Files (PUF) for tax years 1993-1998 as my data source.

Year-over-year estimates indicate that the 1994 EITC credit expansion did induce EITC recipients to increase in non-income based refund maximization. I find that household head filings gradually increased over the period, for a total increase of eight percentage points. Additionally, the incidence of dependent sharing increased, as evidenced by an 8.2 percent decrease in the variance of number of children over the period. Finally, paid preparer patronage increased over the period, rising a net five percentage points.

These results provide information on the gradual transport of tax knowledge within the EITC population. Most of the estimated refund maximization adaptations to the changed tax code began in the year after the expansion year, suggesting that it takes some time for information to disseminate throughout the target population. This delay raises the concern that the EITC's usefulness may be attenuated by a general lack of awareness on how to optimize behavior compliantly over the course of the calendar year. Therefore, investigating how taxpayers respond to changes in program design and noting how long it takes them to respond—though some

responses might technically be noncompliant—provides insight into how this financially fragile population creatively navigates the intricacies of low-income resource acquisition.

The paper proceeds as follows: section two discusses background on the credit and past literature on behavioral adaptations to the EITC. Section three details the IRS administrative data used and descriptive statistics. Section four lays out the empirical methodology for the models investigated, including the year-over-year estimates, event study estimates, difference-in-differences and triple differences. Section five covers results and robustness, and section six concludes.

II. Background and Literature Review

The EITC is a fully refundable credit that is considerable in size and extensively examined as a potential source of tax noncompliance. As a fraction of the tax gap—the difference between the voluntary compliance rate and specific categories of noncompliance—EITC noncompliance comprises only 3.5 percent. Nevertheless, the EITC's duality as a social welfare program and a tax credit, combined with its nontrivial rates of noncompliance, and its Office of Management and Budget classification as a high-risk program susceptible to a high percentage of improper payments, have subjected the credit to a high degree of scrutiny (Drumbl, 2016).

Previous literature provides evidence that EITC recipients engage in two main behaviors unrelated to earned income that are designed to maximize credit amount. First, several sources cite misreporting children as one of the most pervasive mechanisms for refund maximization. Edin, et al.(2013) finds though recipients might not have a clear understanding of the credit—or even know the credit by name—many have an understanding of how EITC credit amount plateaus after a given number of children. Thus, in order to maximize credit amount, the authors cite testimonials of women's decisions to allow others within their kinship network to claim "excess"

children, then split the return. Liebman(2000) finds that EITC noncompliance in 1988 stemmed largely from misreporting of dependent children. Furthermore, in tax year 1990, when credit receipt was contingent upon having children, Leibman finds that 13.2 percent of EITC claimants did not have qualifying children. When examining subpopulations, Liebman(2000) found that male head-of-householders had the highest rates of noncompliance. Over fifty percent of male household heads claiming the EITC did not have a qualifying child either in their household or subfamily within the household.

McCubbin (2000) examines IRS compliance data that samples the returns of EITC filers from 1994, and finds that misreporting EITC qualifying children is the most pervasive error. In addition, the probability of misreporting is increasing in credit amount; that is, as potential credit amount increases, the likelihood of erroneously claiming a child also increases. The IRS's National Research Program(2014) finds that qualifying child errors—occurring in 30 percent of overclaims—are the second most important reporting error.

In addition to misreporting of children, other work discusses the role of tax preparers in refund maximization. Blumenthal, et al.(2005) finds that among ineligible filers, the use of tax preparers was associated with a higher incidence of improper EITC claims. A possible rationale for this association is that preparers have little incentive to verify eligibility, since they earn additional income from clients by offering high interest refund anticipation loans. Jones(2017) finds that numerous preparation mechanisms are positively associated with noncompliance. Jones is able to estimate the direct effect of paid-preparer incentives on tax noncompliance, and finds that online tax preparation, the availability of refund anticipation products, and the use of paid tax preparers are all positively associated with tax noncompliance.

In addition to misreporting children and using tax-preparers to access larger refunds, a third opportunity to refund boost may arise from a taxpayer's filing status decision. The IRS's literature notes:

> *If you qualify to file as head of household, your tax rate usually will be lower than the rates for single or married filing separately. You will also receive a higher standard deduction than if you file as single or married filing separately.*
> (IRS, 2015, p. 8)

Thus, as is clearly laid out in IRS literature, filing as head of household will result in a larger refund. Generally speaking, unmarried persons—whether separated, divorced, or never married—with a live-in dependent are eligible to file as head of household. While IRS publications lay out rules for filing as head of household, there are several conditions for filing that are difficult to verify, hard to understand and potentially exploitable. First, a married person who has lived apart from her spouse for at least half the year can file as head of household, contingent upon her having a claimable dependent (IRS, 2015). Confusingly, even if the filer cannot claim the dependent on her taxes, under certain conditions, she can still claim the dependent in order to file as head of household (IRS, 2015). In addition, if the spouses are apart because one spouse is in juvenile detention center, the they are still considered to be living together, their time apart does not count towards the six month separation metric. if the spouses are apart because one spouse is detained in prison, however, the spouses are considered to be living apart, and the filing spouse can claim head of household status.

Like the complicated rules governing EITC qualification, the detailed and unclear rules governing head of household filing create opportunities for both intentional and unintentional noncompliance. Though am unaware of any peer-reviewed articles that estimate the EITC

recipients' use of head of household filing status to refund maximize, the IRS calls taxpayers incorrectly filing as single or head of household in order to qualify for the EITC its second most common EITC error (IRS, 2018).

Possibly the most glaring threat to compliance facing potential EITC filers is the complexity of the laws governing the credit. As expositied in the discussion of head of household filing, even the most fundamental aspects of tax law, including filing status and exemptions, may be complicated, and contain case specific exceptions and special rules

[1]. Yearly amendments and definition changes create misunderstandings and confusion. The National Taxpayer Advocate cites the complexity of tax laws affecting individuals as "the most serious and burdensome problem facing America's taxpayers" and considers tax law complexity the root cause of many of the issues on its "Top 20 Problems" list (2000, p. 4).

Logically, noncompliance can arise from such confusion, and IRS perceived noncompliance is met with penalties. Even penalties that are seemingly benign—for example, a frozen return that is resolved in the taxpayer's favor after a few months—could be problematic for the low-income EITC population. Mendenhall (2012) finds that 84 percent of EITC recipients used their refund to pay bills and debt. Fennell (2006) finds that tax-payers use overwithholding

[1] For example, assume a single parent has four children who live with her, and for whom she pays all expenses: a sixteen-year-old high school student, an eighteen-year-old college student, a twenty-year-old unemployed job hunter, and a twenty-two-year old graduate student. Under the current (2018) tax code, the sixteen-year-old would be considered a qualifying child for the purposes of the Child Tax Credit and EITC, and a dependent for the purposes of head of household filing status. The eighteen-year-old would be considered a qualifying child for the purposes of the EITC and a dependent for the purposes of head of household filing status, but not a qualifying child under the Child Tax Credit. The twenty-year-old would be considered a dependent for the purposes of the head of household, but not a qualifying child under wither credit. The twenty-two-year old would be considered a qualifying child for the purposes of the EITC and a dependent for the purposes of head of household but would not be a qualifying child under the Child Tax Credit (Internal Revenue Service, 2019).

as a forced savings mechanism, and Jones (2012) finds that low-income taxpayers are the most likely to overwithhold. Indeed, in interviews with EITC recipient women, Edin(2013) finds that many refund boost by increasing withholding and decreasing their exemptions on their IRS W-4 forms, specifically to ensure that they can receive a windfall at refund time.

Frozen returns, requests for documentation, and audit proceedings occur at higher rates for EITC claimants than for other taxpayers. While the likelihood of audit is estimated to be less than one percent (Slemrod 2007), simply claiming the EITC increases the odds of audit. Greensten and Wancheck (2017) find that EITC claimants are almost doubly overrepresented within the audit universe.[2] Haynes(2017) found that EITC claimants comprised 48 percent of the audit pool for taxyear 2004. EITC claimants have little technical tax experience, are the least able to afford professional help in the event of audit, and often experience financial hardship due to examination and enforcement process (National Taxpayer Advocate, 2000).

While confusion with complex rules is likely a major source of noncompliance, it would be naïve to assume that tax evasion is not democratized, and that the EITC population is shielded from the deliberate noncompliance that exists in higher income populations.[3] The interdependence of kinship networks facilitates information sharing on how to "beat this game for refunds for which they (EITC claimants) are not entitled" (National Taxpayer Advocate, 2000), and some taxpayer (and unscrupulous tax preparer) behaviors, such as certifying that a child lives in one's household, or "creating phantom businesses generating 'earned income'"…are none

[2] In 2014, when there were approximately 147 million taxpayers, including 28 million EITC claimants (about 19 percent of filers), EITC filers constituted 39 percent of the of the taxpayers audited (Greenstein & Wancheck, 2017).

[3] It is neither this paper's intent nor capacity to quantify fraudulent behavior—such analysis would require information to which I am not privy.

other than “garden variety cheating…worthy of punishment” (Book, 2003). For taxpayer’s whose noncompliance is determined to be intentional, and therefore fraudulent, serious penalties may apply. Taxpayers engaging in fraud may become ineligible for EITC benefits for as many as ten years (IRS, 2018).

Theoretical models of tax evasion consider the probability of being caught and penalized, the gravity of the penalty, and the offender’s tolerance for risk (Slemrod, 2007). Penalties might not be an effective noncompliance deterrent for EITC recipients, however, largely because EITC claimants might be wholly unaware of the penalties (Carroll J. S., 1987). Even if the taxpayer is aware of the penalty, the efficacy of penalties as a deterrent to noncompliance is inconsistent. Alm, et al. (1992) finds that penalties increase compliance incumbent upon a significant increase in the likelihood of detection, and Smith (1990) finds that deterrence factors are less likely than ethical values to correlate with compliant behavior. Reckers, et al. (1994) finds that while changing a taxpayer’s marginal tax rate from a lower to higher level correlates with a higher rate of noncompliance, among taxpayers who had ethical beliefs regarding tax compliance, changing the marginal tax rate had almost no effect on compliance. Indeed, Carroll (1987) notes that “attempts to deter noncompliance without understanding the sources and processes of noncompliance are unlikely to provide a satisfactory answer to the problem”(p. 332).

Three sources of noncompliance that may not be adequately addressed by traditional penalties are dissatisfaction with the government, the perception that certain taxpayers are being preferentially treated, and a belief that the noncompliant act is in the best interest of one’s family. Several authors cite satisfaction with the government as an important factor determining whether taxpayers decide to avoid paying taxes (Carroll, 1987; Slemrod, 2007; Hayes, 2017). Taxpayers who feel that the tax system is fair (Forest & Sheffrin, 2002) or that the government is equitable

and is working towards their best interests are more likely to cooperate in paying their taxes, even if such cooperation is not in their best personal material interest (Levi, 1998).

Book (2003, p. 1150) notes, however, that reports of widespread noncompliance might have the unintended consequence of making compliers feel like "suckers" for paying an honest tax, and Carroll (1989) asserts that honest taxpaying in a field of perceived noncompliance may engender negative self-evaluations and feelings of regret. As a possible solution, Sheffrin and Triest (1992)suggest that rather than reporting generally about rampant noncompliance, governments report specific instances of noncompliance that have received swift and strict penalties.

Finally, Carroll notes that some persons engage in noncompliant behavior if thy think it will be in their family's best interest. Book(2003) relates a scenario where a divorced mother who receives public assistance and $6,000 in child support from her husband, a worker who earns $18,000. Because the pair's son resides with the mother except on one weekend day, the father—who would qualify for the credit according to income—is not eligible for the credit, as the child does not pass the residence test. In such a case, the father might consider the noncompliant act of claiming his son and receiving the almost $2,000 in EITC credit as being in his family's best interest, and claim the child rather than follow the letter of the law. Notably, if a key reason that low-income persons engage in willful noncompliance is need, rather than greed, then an EITC claimant might gamble detection, as the credit could increase one's annual income by forty

percent.[4] Perhaps Book (2003, p.1194) summarizes the conundrum of tax evasion/avoidance/compliance best:

> Freezing a refund… the inability to really make a civil penalty stick, and the absence of criminal prosecutions of individual taxpayers, suggest that for taxpayers the cost of detection is far less than the expected benefit of cheating. For many taxpayers wishing to game the system, the cost is simply not receiving an EITC to which they were not entitled. That low cost, when compared with the possible receipt of (*thousands)*[1] makes cheating an attractive risk.

III. Data and Descriptive Statistics

In this study, I use publically available administrative microdata from the IRS to analyze whether the probability of three refund maximization activities--choosing head of household filing status, paying for tax preparation, and sharing dependents across kinship networks--altered differentially in the post-EITC expansion period. The nationally representative data covers the pre-expansion year of 1993, the expansion year 1994, and the post expansion period of 1995-1998. These IRS Statistics of Income Public Use Files are well suited to the analysis of tax noncompliance within the EITC-eligible population, and have been historically used to assess the impact of changes in the tax code.

The files, which contain between 94,202 and 132,108 records dependent on year, contain detailed information compiled from a stratified probability sample of unaudited individual income tax returns, Forms 1040, 1040A, and IO4OEZ. Numerous variables salient to this particular study are included. "AGIRI", which indicates the record's adjusted gross income (AGI) range, is sufficiently granular for analysis of where a record falls on the EITC schedule, and is given in

[4] For a family with 2 or more children earning 8890 in 1996, the EITC credit amount was 3556, and would increase a family's annual income by 40 percent, from 8890 to 12446.

thousand dollar increments up to twenty thousand dollars, and five thousand dollar increments between twenty and thirty thousand dollars (the EITC AGI range). The data includes the number of dependent children (critical for determining the potential EITC amount) and what type of return was filed (1040 EZ is not eligible for the EITC).

The data is not comprehensive; head of household filing status has found to be highly correlated with noncompliance rates within the male population, but there is no gender data. I do not have definitive information on whether the EITC was claimed; as is standard in the literature, I use the fact that the data indicates whether the EITC schedule was attached as a proxy for EITC filing. Nevertheless, the data includes information that can be used to control for a number of covariates that have been found to be correlated with noncompliance. Under specifications with the full set of controls, I control for self-employment status (Saez, 2010 finds that self-employed EITC claimants bunch disproportionality at kinkpoints). In addition, I control for state of residence, since numerous factors vary across states, such as presence of state EITCs, fertility rates and average family size, and entitlement program block grant administration. Last, to account for fluctuations in the business cycle, I include year fixed effects.

Descriptive statistics for the pre-expansion period (Table 1) indicate that EITC recipients were over fifty-five percent more likely to prepare their own taxes than non-recipients. On average, recipients had more children than non-recipients; this distinction is almost deterministic, however, because the program's construct did not allow persons without children to claim the credit in the pre-expansion period. EITC recipients were overwhelmingly (14.5 times) more likely to file as head of household than were non-recipients, and thirty-three percent less likely to file as married filing jointly.

IV. Empirical Methodology

A key implication of empirical research in public finance that follows taxpayer decisions to augment non-monetary features of their tax profile is that taxpayers are unevenly able to respond to expansions in the EITC schedule. Filing status, number of children claimed, and use of paid tax preparers ultimately factor into credit amount, preparation costs, and information asymmetries. I specify five equations under three models to determine whether taxpayers differentially alter their filing profile in the post expansion period.

Under model one, I use three equations. First, I estimate changes in filing status. There are six filing status categories: single, married filing jointly, married filing separately, head of household, widowed with a dependent child, and married filing separately claiming spousal exemption. Faced with increased benefits for EITC qualification, we might expect to see a change in status choice; for example, single or married filers might shift to filing as head of household, as head of household filing may lead to a lower taxable income and greater refund (Department of the Treasury, 1998). Thus, in the wake of increased credit amount, I investigate whether there is a differential change in filing status to head of household. As state tax laws vary widely in allocations for EITC recipients, I control for state of residence. I control for time specific trends by using year fixed effects.

Specifically, I investigate changes in filing status according to the following equation:

$$Y_{icpst} = Post_t * EITC_{it} + \beta_2 Post_t + \beta_3 EITC_i + \beta_4' X_{ipcst} + \varepsilon_{icpst} \quad (1a)$$

where Equation 1a represents a probit specification of filing status decision. The latent variable Y_{icpst} represents the propensity for filing a federal return as head of household. A return is filed as head of household when $Y_{icpst} \geq 0$. The coefficient of interest is β_1, which estimates the effect

of being an EITC recipient in the post expansion period on decision to file as head of household. X_{icpst} is a vector measuring the effect of paid preparation, Schedule C filing, state of residence *s*, and time *t* on the propensity to file as head of household. ε_{icpst} is the error term.

I then turn to estimating the proportion of EITC taxpayers who utilize paid tax preparer services. While there are several tax preparation categories—taxpayer prepared, paid preparer, IRS prepared, Voluntary Income Tax Assistance preparer, and tax counseling for the elderly--the vast majority of returns (upwards of 99 percent, see Table 1) are either prepared by the taxpayer or by a paid preparer. Faced with increased benefits for EITC qualification, we might expect to see a shift in tax preparation method. The complicated rules for EITC credit qualification, along with taxpayers' nominal understanding of how the credit is administered, might lead taxpayers to elect for preparation by a tax professional in order to maximize their refund. Thus, in the wake of increased credit amount, I investigate whether there is a differential change in return preparation method.

Specifically, equation 1b is a probit of the following form:

$$Y_{icfst} = Post * EITC_{it} + \beta_2 Post_t + \beta_3 EITC_i + \beta_4' X_{icfs} + \varepsilon_{icfst} \quad \text{(1b)}$$

Under specification 1b, the latent variable represents Y_{icfst} represents the propensity for filing a federal return that prepared by a paid tax professional. X_{icfs} is a vector measuring the effect of filing status *f*, Schedule C filing *c*, state of residence *s* , and time *t* on the propensity to file with paid return preparation. The remainder of the variables and coefficients from Equation 1a retain their denotations.

Lastly under model one, I investigate whether the average number of 3+ children families changed differentially in the post expansion period. Under model 3, if EITC recipient families

responded to the expansion by increasing dependent sharing within kinship networks, then one would expect a reduction in the average number of 3+ children families.

Specifically, equation 1c is a probit of the following form:

$$Y_{icpst} = Post * EITC_{it} + \beta_2 Post_t + \beta_3 EITC_i + \beta_4' X_{icpst} + \varepsilon_{icpst} \quad (1c)$$

Under specification 1c, the latent variable represents Y_{icpst} represents the propensity for filing a federal return that claims 3 or more dependents. X_{icfs} is a vector measuring the effect of paid preparer *p*, Schedule C filing *c*, state of residence *s* , and time *t* on the propensity to file with 3 or more dependents. The remainder of the variables and coefficients from Equation 1a and 1b retain their denotations.

Next, under model two, I use an event study to investigate change in variance in number of children over the expansion period. Taxpayers with more than two children may respond to the credit increase by allowing persons within their kinship network to claim their additional children, as the EITC gives the same credit for 2 children as for 3 children or more. I thus investigate whether the prospect of increased returns resulted in smaller proportion of 3+ children families due to dependent reallocation, as measured by a shortening of the distributional right tail and a reduction of the variance[5]. I can estimate the change in this dependent sharing by

[5] Because the variance is a measure of dispersion, a smaller variance indicates that values are more tightly clustered around the mean. Tight clustering around the mean in response to a policy change, combined with almost no change in the mean value through the policy change window, indicates that people may be "shuffling" children (i.e. going from families with four children and zero children, to families with two children and two children) in order to maximize EITC benefit amount. While both scenarios are mean preserving, going from the first scenario (4-0) to the second scenario (2-2) reduces the variance.

calculating the variance in number of children over the expansion period. A decrease in the variance would be indicative of increased dependent sharing, as there would be fewer extreme values.

I examine the variance in number of dependents within and between filing status-EITC receipt-year cells over the period from 1993 to 1998. I explore the sources of dependent inequality by first decomposing overall dependent inequality into within- and between-group components.

That is:

$$\frac{1}{N_t}\sum(c_{it}-\bar{c}_t)^2 = \frac{1}{N_t}\sum_g\sum_{i\in g}\left(c_{it}-\bar{c}_{gt}\right)^2+\frac{1}{N_t}\sum_g N_{gt}\left(\bar{c}_{gt}-\bar{c}_t\right)^2 \quad (2)$$

where taxpayers are indexed by *i*, time by *t*, and groups by *g*. N_{gt} and N_t denote the number of taxpayers in each group and overall; c_{it} , $\bar{c}_{gt}$, and $\bar{c}_t$ are number of EITC qualifying children, the average number of children within each group, and the overall average number of children.

Lastly, under model 3, I use triple differences estimation to determine whether the average number of 3+ children families changed differentially in the post expansion period. Under model 3, if EITC recipient families responded to the expansion by increasing dependent sharing within kinship networks, then one would expect a reduction in the average number of 3+ children families.

Under model 3, I look at two specific EITC recipient subgroups: those who use paid tax preparers, and those who have a revealed preference for larger family sizes. Jones(2017) finds that paid preparation is positively associated with tax noncompliance, and rationalizes that paid preparers who stand to gain from refund anticipation loans have little incentive to verify taxpayer

dependents. It follows that, in the wake of the expanded EITC benefits, EITC taxpayers who use paid preparers might disproportionately engage dependent sharing. To estimate these taxpayers' response, I use triple differences, where the variable of interest is the interaction between EITC receipt and tax preparation in the post expansion period.

That is:

$$Y_{icpst} = \beta_0 + \beta_1 Paid_p + \beta_2 Post_t + \beta_3 EITC_i + \lambda_1 EITC_i * Post_t + \lambda_2 EITC_i * Paid_p + \lambda_3 Paid_p * Post_t + \delta_1 EITC_i * Paid_p * Post_t + X_{icst} + \varepsilon_{icpst} \quad (3a)$$

where variable Y_{icpst} represents the propensity for filing a federal return with 3 or more children in the post period. A return is filed as a 3+ child household when $Y_{icpst} \geq 0$. The coefficient of interest is δ_1, which estimates the effect of being an EITC recipient in the post expansion period who uses a paid preparer on decision to file with 3 or more children. X_{ics} is a vector measuring the effect of Schedule C filing *c*, state of residence *s,* and time *t* on the propensity to file as with 3 or more children. ε_{icpst} is the error term.

Chapter 2 of this work finds that EITC subgroups that demonstrate a revealed preference for larger family size pre-expansion increase childbearing in the post expansion period. One of these subgroups is EITC recipients in states with high concentrations of religious adherents of faiths that encourage larger families (hereafter "high concentration"). It follows that, in the wake of expanded EITC benefits, while EITC recipients high concentration states demonstrate a comparative increase in 3 or more child families in the post-expansion period, those EITC recipients residing other states would not alter childbearing, but instead would respond by increased refund maximization via increasing dependent sharing. I investigate whether there was

a differential change in the proportion of post period EITC recipients with 3+ children who live in high concentration states.

That is:

$$Y_{ichst} = \beta_0 + \beta_1 HiConcen_h + \beta_2 Post_t + \beta_3 EITC_i + \lambda_1 EITC_i * Post_t + \lambda_2 EITC_i * HiCon_h + \lambda_3 HiCon_h * Post_t + \delta_1 EITC_i * HiCon_h * Post_t + X_{icst} + \varepsilon_{ichst} \quad (3b)$$

where variable Y_{ichst} represents the propensity for filing a federal return with 3 or more children in the post period. A return is filed as a 3+ child household when $Y_{ichst} \geq 0$. The coefficient of interest is δ_1, which estimates the effect of being an EITC recipient in the post expansion period who lives in a high concentration state on decision to file with 3 or more children. X_{ics} is a vector measuring the effect of Schedule C filing *c*, state of residence *s,* and time *t* on the propensity to file as with 3 or more children. ε_{ichst} is the error term.

V. Results

Overall, results indicate that the EITC expansion is associated with an increase in refund maximization activities. Specifically, when compared with their non-claiming and pre-expansion peers, post-expansion EITC claimants disproportionately increased filing as heads of household and increasingly paid professionals to prepare their taxes. In addition, EITC filers disproportionately decreased filings with 3 or more children.

Under model one, I used difference-in-differences to estimate changes in rates of head of household filings (model 1a), paid preparer filings (model 1b), and 3 or more children filings (model 1c). Results indicate that the EITC expansion is associated with an increase in household head filings within the target population. Figure 7 shows that while household head filings remained relatively unchanged among non-EITC claimants--hovering around 4 percent of the population--among EITC filers, the figure increased by eight percentage points from 1993 to 1998.

The proportion remained relatively stable from 1993 to 1995, then jumped by six percentage points to 57 percent in 1996, and increased gradually thereafter. Year-over-year estimates (Table 13a) indicate that household head filings experienced the greatest increase in magnitude—by over eleven percent--two years after the event, and increased by incrementally thereafter. Probit difference-in-differences estimates (Table 13b) indicate with over 99 percent certainty that the expansion increased head of household filings between one and two percent, based upon controls used. This increase in household head filing is in line with anecdotal evidence from Tach, et al.(2013) that filing as head of household rather than single is a mechanism for refund maximization within the EITC population.

Results under model 1b indicate that the EITC expansion led households within the target population to increase paid tax preparer service use. Figure 8 shows while paid tax preparer filings increased slightly among non-EITC claimants--from 75 to 77 percent of the population--among EITC filers, the figure increased by seven percentage points from 1993 to 1998, for a net five percentage point increase. As with household head filings, the proportion remained relatively stable from 1993 to 1995, then increased steadily from 1996 onward, to 68 percent in 1998. Year-over-year estimates (Table 14a) indicate that paid preparer filings increased steadily from two years after the event onward, for a total increase of over eight percent. Probit difference-in-differences estimates (Table 14b) indicate with over 99 percent certainty that the expansion increased paid preparer filings between three and five percent, based upon controls used. This increase in paid preparer use follows given work on paid preparation and EITC receipt. McTigue (2014) finds that returns filed by paid preparers tend to be overestimate refund amount. Maag (2005) finds that taxpayers who know about the EITC and use a paid preparer are more likely to receive the credit than taxpayers who know about the EITC and prepare their return themselves.

The Tax Policy Center(2019), states that preparers might inform clients of both their EITC eligibility and their eligibility for other assistance programs, potentially compounding the benefit of preparer use to access the credit.

Results under model 1c indicate that the expansion of Earned Income Tax Credit benefits resulted in a decreased variance in the number of children claimed by EITC filers. Figure 9 shows that the dispersion in number of children was 0.97 in 1993, one year before the expansion; over the period where benefits for one or two children families increased, observations became more tightly clustered around the mean. By the end of the 4-year expansion window, variance had decreased by 11 percentage points to 0.86. During the same period over which the variance decreased for EITC recipients, the variance remained relatively consistent for non-recipients. The variance in 1998--the end of the 4-year expansion window—decreased by only 2.6 percent (1.11). Year-over-year estimates (Table 15a) indicate that, with the exception of first year after the program's implementation, the variance in number of child dependents decreased steadily, for a total decrease of 11.3 percent.

As an additional robustness check, I calculate the average number of children per taxpayer during the period. If the change in variance is due to an increase in childbearing in response to the expanded benefits for larger families, one would expect the mean number of children to increase. Instead, as illustrated in Figure 10, the mean remained fairly consistent hovering around 1.67 children per claim for EITC filers, and 0.69 children per claim for non-EITC filers. These estimates are in line with work McCubbin(2000) that asserts that dependent misreporting is increasing in credit amount. It therefore follows that, in the wake of the credit expansion, EITC recipients increased dependent sharing, resulting in reduced incidence of larger families, increased incidence of smaller families, and tighter clustering about the mean. Because

the total number of children were reallocated rather increased, the mean did not change appreciably.

Probit difference-in-differences estimates of the change in 3 or more child families(Table 15b) were less certain than estimates of changes in filing status and paid preparer use. While the specifications with no controls (column 1) and that control for self-employment status (column 2) with indicate with over 95 percent certainty that the expansion reduced 3 children or larger families by one percent, other specifications that include state of residence, year fixed effects, and self-employment status give estimates that are small and not statistically significant.

Because the difference-in-differences results seem at odds with the rest of the results under this model, I explore the possibility that statistically significant responses of specific subgroups of EITC claimants are being masked within the aggregate estimates presented of allEITC claimants. Triple differences results under model 3a, presented in Table 16a, column IV, indicate that post period EITC claimants who used paid preparers were slightly less likely (1 percent) to file returns that claimed 3 or more children than their peers. Triple differences results under model 3b, presented in Table 16b, column IV, indicate that post period EITC claimants who live outside of high concentration states were also less likely (2 percent) to claim 3 or more children. These results are robust to specifications with no controls, with controls for self-employment status, with controls for both self-employment status and state of residence, and with controls for self-employment status, state of residence, and year fixed effects. These findings are in harmony with Jones'(2017) work, that finds that paid preparer use is associated with both payment to taxpayers who are EITC ineligible and overpayment to eligible EITC recipients. They are also in line with findings from Chapter 2 indicating that certain EITC subgroups—among them recipients in high concentration states—responded to the expansion by increasing childbearing in the post period.

VI. Conclusion

Overall, results affirm that in the wake of the EITC expansion, taxpayers increasingly utilized non-income based strategies to lower their tax liability and boost their tax refunds. Specifically, the post expansion era witnessed a 1-2 percent in household head filings and a 3-5 percent increase in paid preparer utilization among EITC claimants across multiple specifications. Potentially anomalous reporting of children, a commonly cited noncompliant behavior among EITC recipients, was more statistically noisy to estimate. Because families with more than two children receive the same credit as two child families, sharing dependents across kinship networks permits the non-custodial taxpayer to claim EITC benefits for the misreported "extra" children, while still allowing the custodial taxpayer to receive maximum benefits for the remaining children they claim. Estimates indicate that in the post-expansion era, the variance in number of children decreased by approximately 11 percent, demonstrating that fewer taxpayers filed with large families. This decreased variance, along with an almost constant mean, is an indication of increased sharing of dependents across kinship networks. However, difference-in-differences estimates of change in number of 3 or larger child families, though negative, were rather small, and did not pass significance tests at conventional levels. Triple differences estimates of post-expansion EITC subgroups revealed that claimants using paid preparers and claimants from regions with disproportionately smaller family sizes reduced filings with 3 or more children by 1-2 percent in the post expansion era.

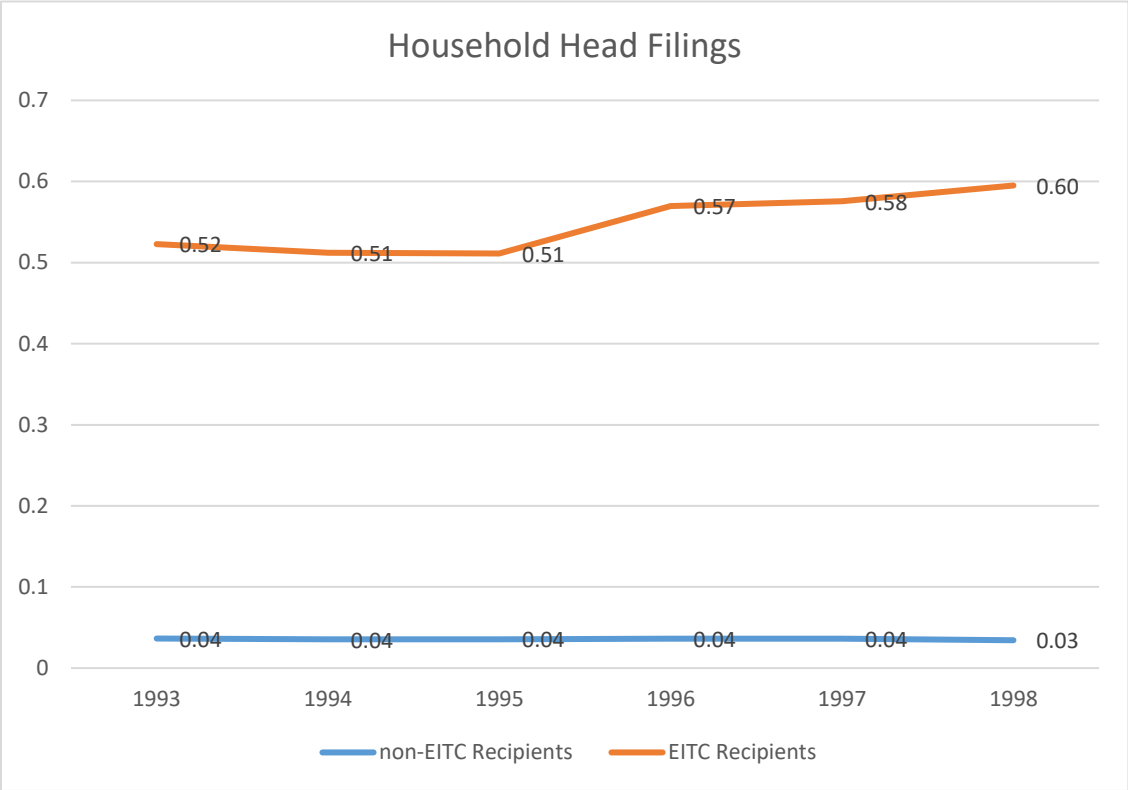
Many studies of refund maximization practices are considered investigations of noncompliant filing behavior, and are used to inform the discussion on tax evasion within the EITC population. While the estimates of this study might fit well in such discussions, this paper's

assessments of taxpayers' responsiveness to changes in the tax code actually provides information on the defined tax categories' potential inapplicability to the EITC population. If within the population, there is greater financial fluidity within kinship networks than within higher income populations, then it is possible that certain behaviors, while technically noncompliant, might simply be illustrative of a more communal approach to resource allocation and provision. An example of this potential tax category blurring is evidenced in the "head of household" classification. If multiple unmarried adults live in a household and share the responsibility of rearing children and paying household expenses, then the IRS' qualifying metric that the head of household be the individual that pays a majority of the household expenses might not unambiguously fall to one person in the household. It follows that multiple persons in the household might identify themselves as the household head--an act that is noncompliant. Indeed, Meyer (2017) asserts that though EITC overpayments constitute 3-4 percent of the tax gap, a nontrivial share of these overpayments could be considered compliant had they been awarded to a different family or household member.

Finally, this analysis informs the discourse on complex rules governing need-based programs. EITC recipients are more likely than non-recipients to be audited, and Guyton, et al. (2018) finds that the act of being audited reduces EITC taxpayer' likelihood of claiming the credit and filing taxes as much as four years after the audit. Because the rules governing eligibility are

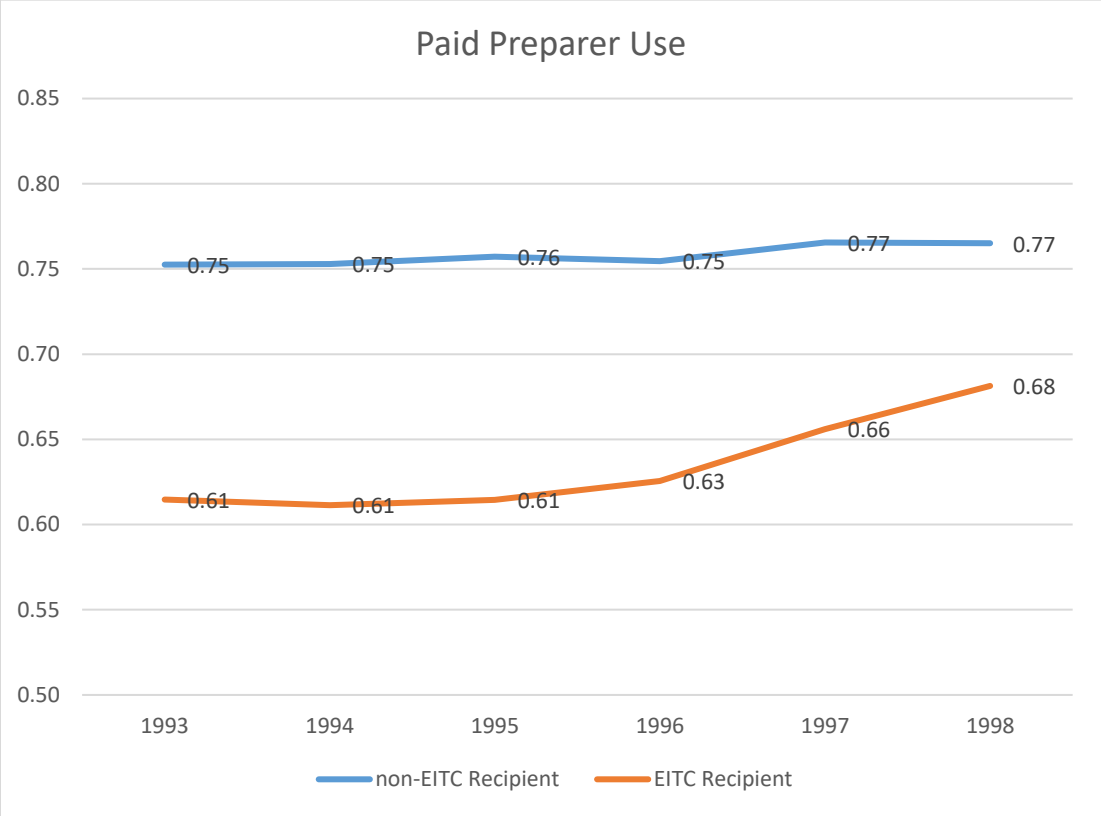
well known to be complex, the penalty for tax errors are severe, and credit receipt is the difference between living above or below the poverty line for 3 million children annually, this analysis serves a step towards open discourse between taxpayers attempting to minimize their tax liability and the IRS investigating to ensure compliance with the tax code.

Figure 7: Rate of Household Head Filings by Year



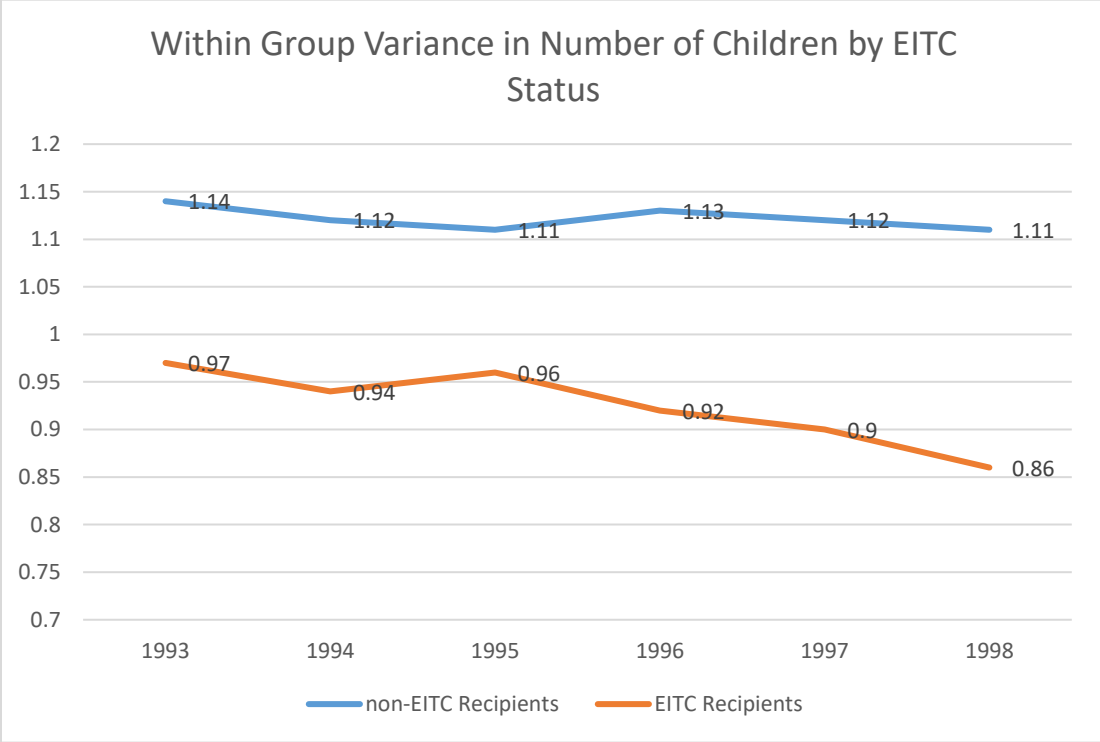
***Note**: This figure depicts how the rate of household head filings trended for proxied EITC recipients (filers who attached the EIC schedule on their tax form) and non-recipients. The EITC expansion went into effect in tax year 1994.*

Figure 8: **Rate of Paid Preparer Usage by Year**



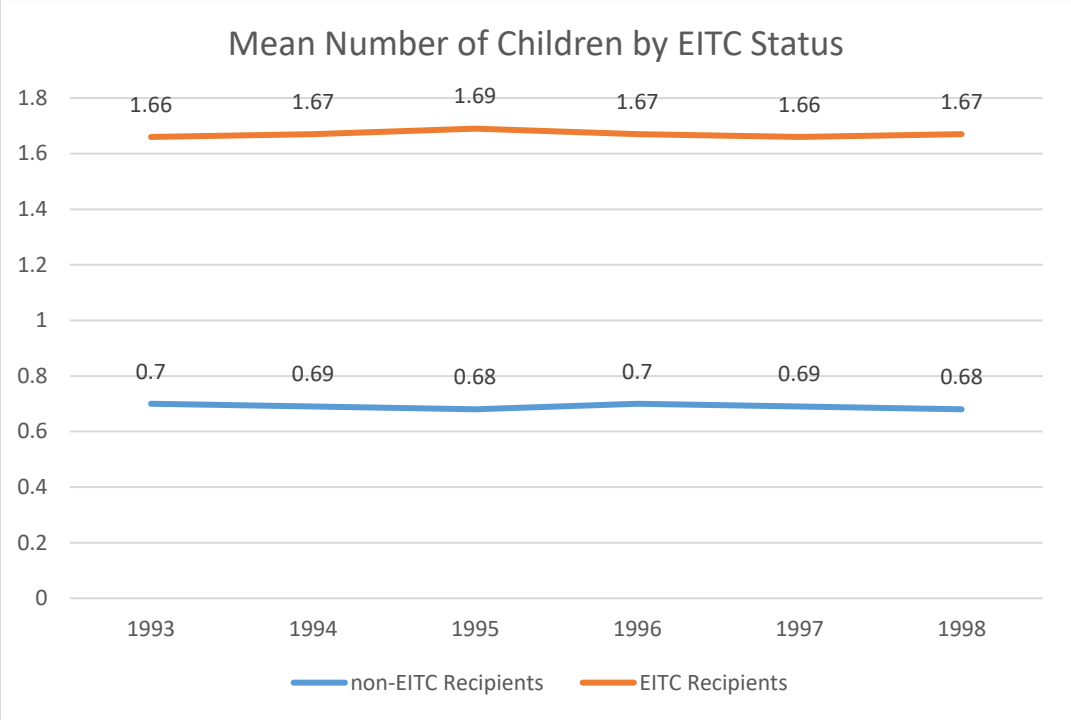
***Note**: This figure depicts how the rate of paid tax preparer usage trended for proxied EITC recipients (filers who attached the EIC schedule on their tax form) and non-recipients. The EITC expansion went into effect in tax year 1994.*

Figure 9: Within Group Variance of Number of Children



***Note**: This figure depicts how the within-group variance trended for proxied EITC recipients (filers who attached the EIC schedule on their tax form) and non-recipients. The EITC expansion went into effect in tax year 1994. The falling variance suggests that EITC recipients are filing with less extreme numbers of dependents, and could be suggestive of dependent sharing across kinship networks.*

Figure 10: Mean Number of Children



***Note**: This figure depicts how the average number of children trended for proxied EITC recipients (filers who attached the EIC schedule on their tax form) and non-recipients. The EITC expansion went into effect in tax year 1994. The stable mean number of children, taken in concert with the falling variance (figure 9) is suggestive of dependent sharing across kinship networks.*

Table 12—Pre-Period (1993) Descriptive Statistics

		non-EITC Recipients	EITC Recipients
Number of Children		0.709	1.663
Filing Status	Single	0.260	0.025
	Married filing jointly	0.680	0.450
	Married filing separately and not claiming a spousal exemption	0.023	0.000
	Head of household	0.036	0.523
	Widower with dependent child	0.000	0.003
	Married filing separately and claiming a spousal exemption	0.000	0.000
Tax Preparer	Taxpayer	0.243	0.379
	Paid preparer	0.753	0.615
	IRS prepared	0.000	0.002
	Voluntary Income Tax Assistance preparer	0.001	0.008
	Tax counseling for the elderly	0.002	0.001
Schedule C or F indicator	Neither Schedule C or F present	0.635	0.687
	Schedule C present only	0.298	0.260
	Schedule F present only	0.041	0.038
	Schedule C and F present, Schedule C Gross receipts larger	0.015	0.008
	Schedule C and F present, Schedule F Gross receipts larger	0.010	0.007

Table 13a—Event study estimates of change in filing status type

		-1	0	1	2	3	4	Post-Pre net change
Head of household	Estimate	0.52	0.51	0.51	0.57	0.58	0.60	+0.08 +15.00%
	Year-over-year raw change	------	-0.01	0.00	0.06	0.01	0.02	-------
	Year-over-year percent change	------	-1.96%	0%	+11.76%	+1.75%	+3.45%	-------

Table 13b: Difference-in-differences estimates of change in filing status type

		(1)	(2)	(3)	(4)
Head of household	Parameter Estimate	0.16*** (0.02)	0.17*** (0.02)	0.13*** (0.02)	0.10*** (0.02)
	Marginal Effects	0.02*** (0.00)	0.02*** (0.00)	0.01*** (0.00)	0.01*** (0.00)
	No controls	x			
	Filed Schedule C		x	x	x
	State of Residence			x	x
	Year fixed				x
Observations		670,411	670,411	670,411	670,411
R^2		0.250	0.285	0.301	0.301

Statistically significant at the 99 percent (***), 95 percent (**), and 90 percent level (*)

Table 14a—Event study estimates of change in paid tax preparer

		-1	0	1	2	3	4	Post-Pre Net Change
Paid Tax Preparer	Estimate	0.61	0.61	0.61	0.63	0.66	0.68	0.07 8.34%
	Year-over-year raw change	-----	0.00	0.00	0.02	0.03	0.02	-----
	Year-over-year percent change	-----	0.00%	0.00%	+3.28%	+4.76%	3.03%	-----

Table 14b: Difference-in-differences estimates of change in paid tax preparer

		(1)	(2)	(3)	(4)
Paid Tax Preparer	Parameter estimates	0.09*** (0.02)	0.11*** (0.02)	0.17*** (0.02)	0.18*** (0.02)
	Marginal Effects	0.03*** (0.00)	0.03*** (0.00)	0.05*** (0.00)	0.05*** (0.00)
	No controls	x			
	Filed Schedule C		x	x	x
	State of Residence			x	x
	Year fixed				x
Observations		670,411	670,411	670,411	670,411
R^2		0.003	0.121	0.180	0.180

Statistically significant at the 99 percent (***), 95 percent (**), and 90 percent level (*)

Table 15a—Event study estimates of change in variance of dependents

		-1	0	1	2	3	4	Pre-Post Net Change
Child dependents	Estimate	0.97	0.94	0.96	0.92	0.9	0.86	-0.11 - 11.34%
	Year-over-year raw change	-----	-0.03	+0.02	-0.04	-0.02	-0.04	-----
	Year-over-year percent change	-----	- 3.09%	+2.13%	- 4.17%	- 2.17%	- 4.44%	-----

Table 15b: Difference-in-differences estimates of change in number of 3+ dependent families

		(1)	(2)	(3)	(4)
3+ Child families	Parameter estimates	-0.05** (0.02)	-0.05*** (0.02)	-0.01 (0.02)	-0.01 (0.02)
	Marginal effects	-0.01** (0.00)	-0.01** (0.00)	-0.00 (0.00)	-0.00 (0.00)
	No controls	x			
	Filed Schedule C		X	x	x
	State of Residence			x	x
	Year fixed				x
Observations		670,411	670,411	670,411	670,411
R^2		0.004	0.006	0.027	0.027

Statistically significant at the 99 percent (***), 95 percent (**), and 90 percent level (*)

Table 16a: Triple Differences estimates of change in number of 3+ dependent families who use paid preparers in the post period

		(1)	(2)	(3)	(4)
3+ Child families	Parameter estimates	-0.08** (0.04)	-0.08** (0.04)	-0.07* (0.04)	-0.07* (0.04)
	Marginal effects	-0.01** (0.006)	-0.01** (0.006)	-0.01* (0.01)	-0.01* (0.01)
	No controls	x			
	Filed Schedule C		x	x	x
	State of Residence			x	x
	Year fixed				x
Observations		670,411	670,411	670,411	670,411
R^2		0.009	0.010	0.028	0.028

Table 16b: Triple Differences estimates of change in number of 3+ dependent families not residing in prefer states in the post period

		(1)	(2)	(3)	(4)
3+ Child families	Parameter estimates	-0.14** (0.07)	-0.14** (0.07)	-0.11* (0.07)	-0.11* (0.07)
	Marginal effects	-0.02** (0.01)	-0.02** (0.01)	-0.02* (0.01)	-0.02* (0.01)
	No controls	x			
	Filed Schedule C		x	x	x
	State of Residence			x	x
	Year fixed				x
Observations		670,411	670,411	670,411	670,411
R^2		0.005	0.008	0.022	0.022

Statistically significant at the 99 percent (***), 95 percent (**), and 90 percent level (*)

CHAPTER 4: WORKER-LEVEL RESPONSES TO TRADE SHOCKS

I. Introduction

There has been a longstanding concern that while trade provides the domestic economy with lower prices, more diverse consumption baskets, productivity growth, and technological advancement, adjustment frictions prove costly to workers in import intensive industries ((Broda and Weinstein 2006); (Bloom, Draca and Van Reenan 2016); (Haltiwanger 2011)). Both theoretical and empirical work has found that increasing import competition can be unfavorable to domestic worker welfare. In addition, recent literature that exploits China's accession to the World Trade Organization as an import shock finds that, even when compared with a period of already increasing import competition, this sharp increase in imports had a disproportionately negative effect on worker welfare in associated industries (Autor, et al. 2013).

There is also evidence that import reductions benefit workers in import intensive industries. Theoretical models indicate that increasing trade protections—which serves as a de facto reduction in imports--benefits workers in the short term, and empirical work has confirmed theory (Gaston and Trefler 1994; Goldberg and Pavcnik 2005).

In keeping with the spirit of analyses that measure import competition and analyze its effect on worker welfare, I calculate import penetration for disaggregated manufacturing industries and then estimate import penetration's marginal effect on worker wages and unemployment on opposite sides of a trade activity shock. This analysis diverges from the traditional literature by quantifying marginal effects for three subsets of vulnerable workers once trade activity and manufacturing output have *fully recovered* from the shock. This distinction in approach is critical to potential policy remedies for supporting workers subsequent to periods of

economic shock. If the aftereffects of the shock persist when short term supports (such as unemployment) have ended, then longer-term solutions (such as education and retraining) may be needed. In addition, if the marginal effects for vulnerable worker subgroups diverge from the aggregate, then targeted interventions might be necessary to support those workers who are disproportionately affected.

The Great Trade Collapse of 2008-2009 and the subsequent recovery and stabilization of manufacturing output in 2010-2011 provide a fitting natural experiment to explore the effect of shocks and recovery on worker welfare because of the period's unprecedentedly sharp and internationally synchronized reduction in trade. The Great Trade Collapse, which was ensconced in the Great Recession, was an import reduction of unprecedented magnitude (Schott 2009). Over six months, the indexed volume of trade fell by the same margin as from November 1929 to January 1931 during the Great Depression; from July 2008 to March 2009, imports fell by 24 percent (O'Rourke 2009). By the beginning of 2010, however, manufacturing output regained its first quarter 2008 levels, and by 2011, output had surpassed its pre-Collapse levels. Though output rebounded, employment numbers remained depressed, as pictured in figure 11. This jobless growth indicates that productivity changed markedly post-Collapse, and might indicate movement on conventional relationships, such as the unfavorable association of import penetration and worker welfare.

In addition to being an unprecedentedly sharp reduction in trade, the Collapse was an internationally synchronized reduction in trade. All 104 data reporting World Trade Organization countries experienced a reduction in both exports and imports from mid 2008-mid 2009 (Baldwin 2009). This blanket reduction imbued the recovery-period welfare effect of import penetration with a sense of ambiguity, especially given two features: the globally integrated supply chain

governing production of manufactures, and the varied role across subsectors that intermediate inputs play in the production of final goods (Baldwin, 2009; Cravino and Sotelo 2019). Because a single product may be created using intermediate goods that cross multiple borders in the production process, a reduction in imports could have varied effects on domestic manufacturing workers.

For example, if a change in trade policy causes a reduction in imports of final demand goods due to a change in relative prices on the domestic market, then this could result in increased demand for competing domestic goods, which would increase demand for domestic workers. In such a case, a reduction in imports would be favorable for domestic workers. However, if the reduction is driven by a decrease in intermediate input imports used as components for domestic production and if a comparably priced domestic substitute is unavailable, then this reduction in imports could actually inhibit domestic output, reduce the demand for domestic labor, and eventually prove unfavorable for domestic worker welfare. Therefore, the synchronized, sharp decrease in imports that characterized the Great Trade Collapse could have amplified the unfavorable import penetration worker welfare relationship, changed the relationship favorably, or had no effect. Given this recovery to pre-Collapse output levels following a period of unprecedentedly marked, internationally synchronized decline, I investigate whether the worker welfare effects of import penetration also reverted to pre-Collapse levels.

I find mixed results. First, I find that in the recovery and stabilization era, a ten percent increase in import penetration resulted in a one percent reduction in wages, and a two percent increase in unemployment. Because I use an estimator that differences the response in the pre-Collapse period from the response in the recovery period, this result indicates that, generally

speaking, the unfavorable relationship between import penetration and worker wages continued and was amplified in the recovery and stabilization period.

Interestingly, I find that workers often considered the most vulnerable to import competition actually outperformed their peers in the recovery and stabilization period. To investigate heterogeneity in the marginal effect of import penetration on worker outcomes, I control for worker education, employment in a shrinking industry, and employment in a durable goods industry. Research has shown that low-skilled workers, workers in industries that are reducing their domestic workforce, and workers employed in durable goods industries are particularly vulnerable to increasing import competition (Benguria 2017; Bems, Johnson and Yi 2013). My estimates show that workers with low education levels and those employed in shrinking industries outperformed their peers in the recovery and stabilization period, boasting 0.5 percent and 1.3 percent wage premiums from ten percent increases in import penetration, and having no statistically significant adverse effect of increasing import share on unemployment. Among workers employed in durable goods industries, however, increases in import penetration were bad for both wages and unemployment.

The results for workers with low education and workers employed in shrinking industries are novel. No other study (to my knowledge) has quantified the effect of import penetration on these manufacturing worker subpopulations in the post-Collapse recovery and stabilization. In addition, the estimates demonstrate that, at least in the short to medium term, the effect of import penetration on worker welfare may change dramatically following a period of sharp import decline, even in an environment of increasing manufacturing output and trade activity.

The paper proceeds as follows: section 2 is a review of related literature; section 3 explains the empirical methodology; section 4 details the data sources; section 5 presents results and sensitivity checks, and section 6 concludes.

II. Related Literature

Economic theory affirms that import competition can prove problematic for domestic workers in high-income countries. Factor-price equalization states that an increase in trade in goods equalizes the rents of identical, mobile factors across country lines, reducing bilateral inequality. Thus, in the case of workers in different countries performing identical tasks, yet receiving disparate wages, an increase in trade between the countries would lower the wages of workers in the high wage country, and raise the wages of workers in lower wage countries (Samuelson 1948). Jones' (1971) specific factors model says that given industry specific human capital, trade lowers the real return to factors in import competing industries.

Numerous empirical studies have confirmed the theoretical predictions that, in general, import exposure and worker welfare move in opposite directions (Acemoglu, et al. 2016). Import penetration is associated with both higher unemployment and lower labor force participation (Autor, Dorn, & Hanson, 2013). Autor, et al. (2014), focusing only on workers in the manufacturing industry, find that individuals employed in industries that experienced high import growth garnered lower wages, enjoyed less job stability, and had greater reliance on public assistance. Using plant-level data, Alvarez and Opazio (2011) find that increased exposure to Chinese imports depresses wages of Chilean workers by as much as 25 percent. Even *anticipated* increases in import competition may be problematic. Pierce and Schott (2016) specifically link domestic employment declines to firms' anticipation of changing trade liberalization policies that would permanently reduce import tariffs on Chinese goods. They assert that anticipation of these

policies, which in turn facilitated the increase of both US imports from China and Chinese exports to the United States, resulted in employment contraction due both to increased job destruction and inhibited job creation.

There is nuance to the pejorative import competition story, however. First, duration matters: Görg and Görlich (2011) find that import competition results in short run job displacement, but that competition results in employment growth in the long run. Second, the income level of the importing country matters: Ebenstein (2009) finds that the wage effect of import competition via offshoring is industry dependent, and contingent on the income level of the destination. Offshoring to low-income countries depresses U.S. worker wages, while offshoring to high-income countries increases wages.

Third, occupational complexity matters: Ebenstein (2014) finds that import competition at the industry level has minimal effects on wages; rather, import competition via occupational offshoring leads to a decline in real wages for domestic workers who perform routine tasks. Trade induced occupational switching is associated with a 12.1 percent wage penalty. Cooke, Kemeny, and Rigby (2016) finds that offshoring to low-income countries depresses the wages of workers employed in routine occupations that perform non-complex tasks, but import competition raises the wages of workers performing highly complex or non-routine jobs. Occupational changes that result in exiting manufacturing could have significant income effects for workers, as hourly compensation in manufacturing boasts a thirteen percent premium over non-manufacturing employment (Langdon and Lehrman 2012). Indeed, of full-time manufacturing workers who lost their jobs between 2009 and 2011 and were re-employed in full-time work in January 2012, sixty-five percent were re-employed in positions paying less than their previous job (Manufacturers Alliance for Productivity and Innovation 2013).

While not as abundant as studies of increasing import penetration, analyses of decreasing import penetration generally find a symmetric result: decreasing imports often proves favorable for workers in associated industries. Investigations of decreasing import competition often focus on competition reductions via changes in domestic economic policies. Using a simulation model that incorporates adjustment dynamics and worker reallocation, Lechthaler and Mileva (2018) show that unskilled workers in import competing industries stand to gain in both the short and long term from increasing tariffs and decreasing imports. Macario (1999) finds that Chilean firms benefitted from reduced import competition induced by state-enforced exchange controls. Goldberg and Pavcnik (2005) show that Columbian workers employed in industries either that received trade protections or were subject to rising tariffs enjoyed a wage premium over workers employed in industries that were more liberalized. Thus, while increasing protections likely hurt the economy in the aggregate, and may result in a more costly, less diverse consumption basket, the accompanying reduction in import competition may benefit manufacturing workers.

This work fits snugly into analyses of worker welfare in response to import shocks. Similar to Acemoglu, et al. (2016), I study a period that extends into the 2010-2011 recovery and stabilization period in manufacturing output. However, whereas Acemoglu, et al. (2016) investigates changes in aggregate national employment specifically in response to the Chinese import shock, I examine changes in worker-level employment and wages, and include all imports, regardless of country of origin. Kaplan, et al. (2011), examines worker level employment and wage responses to the Great Trade Collapse, but focuses only on the Collapse period itself (2007-2009) and limits analysis to the Mexican labor market. This work seeks to contribute to medium term microeconomic analyses of U.S. manufacturing workers by juxtaposing the effect of import penetration on manufacturer worker wages and unemployment in the years of output growth

before the Collapse to the effect of import penetration in the years of recovery and stabilization following the Collapse.

III. Empirical Strategy and Identification

III.i Empirical Strategy

Many papers have explored the relationship between trade and labor outcomes, but most analyze periods of economic expansion or minor recessions with quick recoveries. For example, Ebenstein, et al. (2014) estimates the effects of offshoring on worker wages over the years 1984 to 2002, and Autor, et al. (2013), Autor, et al. (2014), Autor, et al. (2016), and Benguria (2017) estimate the impact of import competition on a number of worker economic well-being measures, including wages and unemployment, over the period 1991-2007. While these periods both experienced recessions, the Great Trade Collapse's unprecedented drop in imports constitutes a unique opportunity to assess whether the reported adverse worker welfare effects of import penetration persist during output recovery and stabilization period following dramatic import decline.

To accomplish this analysis, I begin by quantifying the intensity of worker exposure to imports. Following the trade exposure calculation used in Autor, et al. (2014), I calculate the natural log of import penetration at the three-digit North American Industry Classification System (NAICS) subsector level in years 2004-2010, as given by:

$$IMPen_{jt} = ln\left(\frac{IM_{jt}}{TVS_{jt}+IM_{jt}-EX_{jt}}\right) \quad (1)$$

Where for industry j in year t, IM_{jt}indicates the value of imports, and $TVS_{jt} + IM_{jt} - EX_{jt}$ is initial absorption, measured as the total value of shipments TVS_{jt}, import value, and export value EX_{jt} in subsector *j* in year *t*.

I use a difference-in-differences specification to compare the effect of import penetration in the pre-Great Collapse period to the post-Great Collapse recovery and stabilization period. Specifically, I regress the given outcome (unemployment or natural log of wage) of worker i in industry j in year t--2005-2006 for the pre-Collapse era, and 2010-2011 for the post-Collapse era--on 3-digit NAICS subsector level lagged import penetration using trade measures from 2004-2005 for the pre-Collapse era and 2009-2010 for the post-Collapse era. The rationale for using the post-Collapse period, rather than the Collapse period itself, is to analyze periods when the manufacturing industry output levels were similar. By 2010, manufacturing output had regained its pre-Collapse level (see Figure 11).

This similarity in output permits me to examine whether the industry-level recovery "trickled down" to the individual worker. In addition, because Artuc, et al. (2010) notes that the labor market responds slowly to trade shocks, I use lagged measures of trade to accommodate labor market frictions. Assuming variation in import penetration constitutes an exogenous shock that will impact subsector-specific labor market outcomes, the lagged trade measure permits the labor market sufficient time to adjust. This gives an equation of the form:

$$W_{ijt} = \beta_0 + \beta_1 Z_{ijt} + \beta_2 Binary_{it} + \beta_3 Trade_{jt-1} * Col_t + \beta_4 Col_t + \beta_5 Trade_{jt-1} + \beta_6(\Delta\lambda_t - \Delta r_t) + \beta_7 \zeta_i + \varepsilon_{ijt} \quad (2)$$

Where W_{ijt} indicates worker i's well-being outcome (i.e. natural log of wages or unemployment[1]) for industry j in year t; Z_{ijt} is the vector of demographic covariates, $Binary_{it}$, are binary indicators for education, worker association with a shrinking industry, and age; $Trade_{jt-1}$ are

[1] Unemployment is a binary variable denoting employment status.

lagged measures of trade (import penetration $IMPen_{jt}$+ export share, $EXSh_{jt-1}$[2]); and ε_{ijt} is the error term. $Trade_{jt-1} * Col_t$ is the interaction between trade and the post-Collapse recovery and stabilization era, and β_3, the effect of trade in the recovery and stabilization era, is the coefficient of interest.

I control for sex, race, state of residence, veteran status, and citizenship status. Rather than using hedonic indices of age and education, I create age and education pools for workers following Riker (2015), classifying 45-65 year-olds as "old" and 18-44 year-olds as "young". I separate workers according to those with a high school diploma or less ("low-ed"), and those with some college or more ("high-ed"). I also control for association (current or previous employment) with a subsector that shrank by 20 percent or more between the pre- and post- period.

To separate the trade effect from business cycle effects, I include lagged change in gross domestic product (GDP) as a measure of national economic conditions during the period from 2005-2010. I calculate the GDP Gap the difference in real and potential GDP. The annual growth rate of U.S. real GDP, denoted as $\Delta\lambda_t$, is customarily used to predict unemployment rates and overall national economy health. The growth rate of potential GDP, denoted as Δr_t, is used to estimate the economy's maximum output level that will maintain constant inflation (OECD, 2019).

Next, I include measures related to firms' ability to tolerate credit shortages, such as those experienced during the Great Trade Collapse. Haltenhof, Lee, and Stebunovs (2014) find that industries dominated by firms that relied on bank credit were negatively affected by the Recession-era credit crunch.[3] In addition, they find that industries in which tangible assets were

[2] Export share is calculated as Exports/Total Value Shipments

[3] The Great Recession began in December 2007, and ended in June 2009 (The National Bureau of Economic Research 2019).

customarily used as collateral to secure loans were also more adversely affected. Thus, following the work of Rajan and Zingales (1998), Cetorelli and Strahan (2006), and Haltenhof, Lee, and Stebunovs (2014), I classify subsectors based upon three measures related to credit access $\zeta_{j,}$: dependence on external financing, availability of tangible assets, and production of durable goods.

Specifically, I separate subsectors into those with above/below the median dependence on bank credit, above/below the median ability to pledge collateral as security for commercial and industrial loans, and those identified as producing durable goods. As commercial and industrial bank credit became more difficult to access, certain industries had greater success at accessing external financing options, such as the bond market. These industries would be better poised to survive economic downturns. Conversely, industries that produced durable goods and that relied on pledgable assets would have been comparatively less able to survive downturns.

Because most durable goods (such as washing machines and cars) are financed, industries that produce such goods are sensitive to dramatic restrictions of consumer access to credit. Additionally, as the commercial and industrial loan credit standards tightened dramatically over the Great Recession period, industries that relied on pledgable assets to access bank credit were disproportionately adversely affected (Haltenhof, Lee and Stebunovs 2014). Using these measures of trade and these controls for business cycle and industry specific effects, I estimate the persistence of import competition's effect on two outcomes: wage and unemployment.

In a second specification, I investigate if there is heterogeneity in the marginal effect of import penetration across three subgroups: low-educated workers, workers associated with industries whose domestic workforce shrank 20 percent or more from the pre- to post- period, and workers employed in durable goods industries. Low educated workers often perform

routinized tasks that are automatable, and may not have sufficient skills to survive firm outsourcing and/or offshoring decisions (Brynjolfsson and McAfee 2012). A nontrivial fraction of workers associated with shrinking industries likely had to find employment in other industries or retire. These actions could have negatively affected their wages and employment status (Rutledge, Orlova, and Webb 2013). Durable goods industries were the hardest hit by the Collapse and Recession and accounted for a majority of factory job losses (Barker 2011). Therefore, I use triple differences to investigate the effect of import competition on wages and employment of these three subgroups.

Specifically, for low educated workers, shrinking industry workers, and durable goods workers, I estimate the following equation…

$$W_{ijt} = \beta_0 + \beta_1 Z_{ijt} + \beta_2 Binary_{it} + \beta_3 Trade_{jt-1} * Col_t + \beta_4 Col_t + \beta_5 Trade_{jt-1} +$$

$$+ \beta_6 Trade_{jt-1} * Subgroup_{it} + \beta_7 Col_t * Subgroup_{it} + \delta_1 Trade_{jt-1} * Subgroup_{it} * Col_t +$$

$$\beta_8(\Delta\lambda_t - \Delta r_t) + \beta_9 \zeta_i + \varepsilon_{ijt} \qquad (3)$$

Where $Trade_{jt-1} \cdot Col_t \cdot Subgroup_{it}$ is the interaction between trade, the post-Collapse era, and the relevant subgroup, and δ_1, the effect of trade competitiveness post-Collapse on workers in the subgroup, is the coefficient of interest. All other variables heretofore specified retain their denotations.

Under the log-log wage specification, the coefficient of interest (β_3 in equation 2, δ_1 in equation 3) estimates the post-Collapse period change in the partial elasticity of wage with respect to import penetration. A positive and statistically significant β_3, for example, would indicate that when import penetration increases by one percent, wage increases by a post-Collapse premium of β_3 percent, holding all else equal. Under the probit specification estimated with marginal effects, a positive and statistically significant δ_1, for example, would indicate that a

one percent increase in import penetration would result in an absolute increase in the unemployment level of δ_1, ceteris paribus.

III.ii Identification

To investigate whether the sharp reduction in imports during the Great Trade Collapse altered the relationship between import penetration and worker welfare, I exploit the cross-sectional variation in import penetration by subsector. Specifically, within the difference-in-differences specification, I examine whether changes in employment and wages in manufacturing subsectors with varying levels of import penetration (first difference) are larger after the recovery from the Great Trade Collapse (second difference).

Within the triple differences specification, I examine whether changes in employment and wages in manufacturing subsectors with varying levels of import penetration (first difference) are larger after the recovery from the Great Trade Collapse (second difference) for workers who are members of subgroups that have traditionally had a disproportionately unfavorable response to increased importing (third difference).

IV. Data

I use linked demographic, trade, macroeconomic, and industry specific data compiled from a number of sources to estimate the effect of trade activity in the post-Collapse era on worker outcomes.

IV.i Demographic data

I source my demographic variables from the American Community Survey (ACS) years 2006, 2007, 2011, and 2012 using the IPUMS online microdata repository (Ruggles, et al. 2016).[4] Begun in 2005, the ACS provides annual microdata on questions previously only covered in the

[4] The survey year correspond to information gathered in the previous year. Thus, survey year 2006 covers responses gathered in 2005.

long form decennial Census (United States Census Bureau 2014). The survey is administered monthly and then aggregated yearly to give annual statistics for a cohort of approximately two hundred-fifty thousand United States households. Because the cohort changes monthly, estimates given will be for representative worker outcomes using the data as a repeated cross-sectional sample.

The ACS provides a rich set of variables that makes it feasible to estimate the impact of trade on labor outcomes. First, person-level data on wages and labor market attachment provide the opportunity to study wages, total personal and family income, and unemployment. Variables that capture education permit me to study whether the interactions of these variables with trade activity result in magnified effects that eclipse their additive effects. In addition, sex, "race", English proficiency, age, and veteran status provide additional identifiers to improve comparability between respondents.

Lastly, each respondent is linked to an industry using a NAICS code based on the goods produced in her stated establishment of employment (or, for those who are unemployed, the last establishment in which she was employed up to five years prior) (Ruggles, et al. 2016). Dependent on the information that a respondent gives, she may be able to receive as detailed as a six-digit NAICS classification or as generic as a two-digit classification. I retain individuals who are in the labor force, regardless of if they are employed. Additionally, I retain respondents who have an industry affiliation at the subsector (three-digit NAICS) or more detailed level.[5]

IV.ii Trade data

[5] I drop respondents who only have a two-digit NAICS industry affiliation, as the extensive heterogeneity within the three two-digit manufacturing sectors (31, 32, and 33) would make analysis of labor responses untenable.

I merge current year demographic to previous year trade data on the three-digit NAICS code. I use import, export, and domestic production manufacturing statistics for the lagged years 2004, 2005, 2009, and 2010 because I expect establishment labor force adjustments to changes in trade activity to be non-immediate. The import and export data is sourced from the US Census Bureau and provided by the United States International Trade Commission Interactive Tariff and Trade DataWeb, and the domestic production statistics are from the Annual Survey of Manufactures.

IV.iii Macroeconomic and Industry Specific data

To account for the level of monetary stimulus the U.S. economy would need to function optimally, I use the lagged output gap in the business cycle, calculated as the difference between percent change in real and potential GDP. Potential GDP is sourced from the Congressional Budget Office, and real GDP is sourced from the World Bank (Congressional Budget Office 2018; World Bank 2018).

The Great Trade Collapse was ensconced within the Great Recession, and the recession functioned as a "credit crunch"—a sudden and marked worsening of firm and consumer access to banking credit. Because of industry heterogeneity in ability to weather such credit crunches, I implement measures to account for industry resilience in the face of credit shortages (Haltenhof, Lee and Stebunovs 2014).. The twenty-two manufacturing industries and their categorization by credit crunch indicator can be found in Table 18.

IV.iv Descriptive Statistics

Descriptive statistics juxtaposing the pre- and post-Collapse periods (Table 17) indicate that pre-Collapse manufacturing workers were almost indistinguishable from post-Collapse workers over several demographic indicators, including English proficiency, citizenship status, and

"race". On average, however, pre-Collapse workers were less educated (4.7 percent) and more likely to identify as female (4.1 percent). Pre-Collapse workers were about a year younger (43.391 versus 44.662 years) and considerably (58.1 percent) less likely to be unemployed than workers in the post-Recession era (0.043 versus 0.068). These differences seem to indicate that older, more educated male workers were more likely to remain employed post-Collapse.

VI. Results & Sensitivity

VI.i Results

My baseline OLS regressions, presented in Tables 19 and 20, examine how increasing import penetration is associated with the wages and unemployment of workers in associated industries. Table 19 estimates that over the period under study, 2005-2011, increasing import penetration is associated with reduced wages. In addition, low education and employment in a shrinking industry have the expected negative correlations. Estimates from Table 20 indicate that unemployment also responded as expected; increases in import penetration resulted in higher unemployment, as did low education and employment in a shrinking industry. Conversely, durable goods association was estimated as favorable to worker wage and unemployment.

Under the difference-in-differences and triple differences specifications, I implement six levels of controls--with controls increasing in column number--to test the effect of changes in import penetration on wages and unemployment. My preferred control level for wage estimates is presented in column VI, which includes three subgroup categorical variables (youth, shrinking industry association, and low education), demographic variables, change in GDP, credit crunch variables, and state of employment.

Difference-in-differences estimates that juxtapose the pre-Collapse and post-Collapse period give a clearer representation of recovery and stabilization period worker conditions. As

specified in column VI of Table 21, log-log regression estimates indicate that increasing import penetration in the post period by ten percent results in approximately a one percent decrease in wages, indicating that overall, the Collapse further exacerbated the unfavorable relationship between import competition and wages. Triple differences estimates of the three potentially vulnerable subgroups, however, expose surprising heterogeneity. While workers employed in durable goods industries experienced both a wage and an employment penalty, post-Collapse, both workers with low education levels and workers employed in shrinking industries boasted increases (0.49 percent and 1.33 percent, respectively) in wage earnings given a ten percent increase in import penetration. These finding are even more surprising when taken in concert with unemployment estimates from Table 22. Though increasing import penetration generally resulted in slightly higher unemployment (a ten percent increase in competition results in a 0.20 percent increase in unemployment), estimates indicate that within the three vulnerable subgroups, workers with low education levels and those employed in shrinking industries again outperformed their peers. In both cases, triple differences estimates indicate that unemployment for these workers did not differ from the national average, and that they were no more likely to be unemployed as a result of increasing imports in the post-Collapse than they were pre-Collapse.

These wage and employment results are notable, given that the welfare of workers with low education levels and workers in shrinking domestic industries is traditionally found to suffer adversely when import trade activity increases. A possible mechanism for the favorable association is the changing landscape of the manufacturing workforce in firms entering post-Collapse versus those exiting post-Collapse. Relative to exiters, entering firms had a much higher rate of computer investment per employee, suggesting that these entering firms were using more technologically advanced—and in by corollary, productive—ways to accomplish output (Barth, et

al. 2017). Continuing firms within manufacturing--those older manufacturing firms that had been established prior to the Collapse and weathered the trade downturn--overwhelmingly engaged in labor hoarding, and point to a second possible mechanism for favorable results: Collapse-era firm adjustments to production location in order to cut costs. Barth, et al (2017) notes that some firms' highly global workforce might have permitted continuing establishments to offshore employment, leaving an experienced domestic workforce that would have made layoffs and the corresponding skill loss costly in the long run. Thus, the mechanisms for the favorable welfare effects of increasing import penetration during the recovery period likely varied by firm tenure, with entering firms engaging in technologically advanced production, and continuing firms increasing downstream production to cut costs and retain strong domestic workers.

VI.ii Further Analysis and Sensitivity

I subject the estimates to further analysis and three sensitivity tests to shed further light on the favorable results.

First, selective shrinking within manufacturing might have introduced sample bias that could lead to an overestimation of the benefit of import competition. It is possible that the wage returns to import competition for workers with low education levels and those employed in shrinking industries are higher during recovery and stabilization because some lower paid manufacturing workers took positions in sectors outside of manufacturing during the Collapse, and no longer claimed affiliation with their previous sector of employment. Indeed, the numbers of manufacturing workers in the recovery and stabilization period is roughly ten percent lower than in the pre-Collapse period (see Observations, Table 17). If a certain class of lower paid workers who were employed in a shrinking industry or who had low education—for example, those with less tenure, experience, or ability--were laid off from manufacturing and reabsorbed

into another industry by the time of the post-Collapse analysis, then this culling of workers could explain the favorable results.

The difficulty with assessing industry attrition in a repeated cross-sectional sample, however, is lack of data. ACS respondents may report multiple occupations on their survey, but the microdata sample only allows for one industry affiliation (IPUMS, 2019). If workers formerly employed in manufacturing transition to a lower paying position outside of manufacturing and report their non-manufacturing industry in their survey response, then their lower wages would be excluded from manufacturing wage estimates. Indeed, Autor, et al. (2013, p.2146) detail their difficulty with separating the impact of import shocks on wages from potentially confounding employment reductions by noting that ages with lower earning and ability are more likely to lose employment given an adverse trade shock. Thus, their recommendation is to interpret wage estimates with caution given potentially understated changes in workforce composition.

I address this potential sample bias by examining the distributional changes in age and wages among workers with low education levels and workers in shrinking industries in the pre- and post-Collapse period. The density graph in Figure 12 indicates that the age distribution shifted rightward during the recovery and stabilization period, and that the percentage of middle-aged workers dropped noticeably. Distributional analysis presented in Table 23a confirms the visual shift—at each quartile, recovery and stabilization era workers were older than pre-Collapse workers, sometimes by as much as 3 years (see third quartile of shrinking industry workers). On average, domestic manufacturing workers were at least 1.4 years older post-Collapse than pre-Collapse. I use locally weighted non-parametric smoothing (lowess) to assign a fitted value to each data point and graphically plot a smooth curve through a scatterplot of data with wage on the y-axis and age on the x-axis (Bartlein 2019). The lowess smoother that regresses wage on age

confirms that older age is correlated with higher wage (Figure 13). If we assume wage is correlated with experience, then it is possible that the workers who kept their jobs were older, more experienced, higher paid employees, and this culling of the younger, lower paid work force would be the source of the positive-signed returns to import penetration.

Distributional analysis of wages do not affirm this scenario, however. Table 23b indicates that workers in the post-Collapse recovery and stabilization period did earn more than those in the pre-Collapse period, but the differences are slight, and the wage distributions pre- and post- are similar. The differences in age between periods is stark (see Table 23a).

The kurtosis of the wage distribution adds additional insight. The sharp reduction in the kurtoses of the wage distributions during recovery and stabilization suggest that culling of the lowest and trimming of the highest earners might be one source of the reduction in number of workers.[6] It is possible that the lowest earners are largely comprised of the increasing number of manufacturing workers are temporary workers who might not enjoy the compensation premiums and job security of employees (Dey, Houseman, and Polivka 2012). Also, the highest earners might be retirement age workers facing unemployment, wage cuts, or reductions in work hours who might have decided to leave the labor force (Rutledge, Orlova, and Webb 2013). Regardless of the identity of the wage distribution outliers, is possible that a small number of very low or very high paid workers is driving the positive-signed estimates for low education and shrinking industry workers. To investigate this possibility, in the spirit of Choi (2009) and Sturm and de Haan(2005), I perform a sensitivity test by dropping observations that are lower than the 1st and higher than the 99th percentiles of the wage distribution. The updated estimates, found in Table 24, indicate

[6] The kurtosis is a measure of the propensity of the distribution to produce outliers (Westfall 2014), and is the fourth moment of a random variable.

that the positive-signed results are not driven by outliers. Indeed, though dropping outliers reduced the kurtosis of the wage distribution greatly, from 7.2 to 4.7, the magnitude of the wage point estimates remains largely unchanged. Thus, while the manufacturing industry underwent an approximately ten percent reduction over the time period under investigation, distributional analyses do not suggest that selective attrition biased the wage distribution caused the favorable wage estimates.

A second possible reason that low education and shrinking industry workers may have performed comparatively well post-Collapse could be that the distribution of occupations has transitioned away from production employment and towards higher paying, "white-collar" positions. Indeed, Cooke, et al.(2016) finds that increased import penetration is favorable for domestic workers who perform highly complex, non-routine jobs. To investigate this possibility, I separate workers by occupation type—production or non-production--then regress wage on the import penetration variable of interest for each of the three subgroups under column VI. These results are presented in Table 25.

Estimates indicate that both low educated and shrinking industry production workers enjoyed higher wages during recover and stabilization, affirming that post-Collapse wage premiums are not due to a distributional shift towards higher paying, white-collar jobs. Compared to production workers from the pre- period, recovery and stabilization workers employed in shrinking industries still enjoyed an 11.2 percent higher wage; workers with low education levels boast a 9.4 percent premium over their pre-Collapse counterparts (table 9, column III).

I examine the import penetration distribution as a third possible reason that low education and shrinking industry workers have performed comparatively well during recovery and stabilization. Figure 14 demonstrates that the competition level fell in the recovery and

stabilization period, and that the median competition level was much lower in the post-period. While the median (mean) level of importing was 0.269 (0.276) in the pre-Collapse period, it fell to 0.137 (0.204) in the recovery and stabilization period. The divergence between the median and mean in the post-period demonstrates that certain industries had increased levels of penetration in the recovery period. Indeed, while the 75th percentile of import penetration in the pre- and post- periods were close (0.367and 0.334, respectively), the 95th percentiles were far apart (0.771 and 0.892). To check whether well-paid workers in high penetration industries were the source of the favorable wage estimates, I truncate the import penetration distribution by excluding workers employed in industries whose level of penetration fell below the 5th percentile or exceeded the 95th percentile. These estimates, presented in table 26, demonstrate that the edges of the import penetration distribution were not driving the favorable results. While import penetration's recovery and stabilization period effect remained negative for workers generally and workers in durable goods industries specifically, workers with low education levels and those employed in shrinking industries continued to enjoy wage premiums associated with increasing import penetration in the post-Collapse period.

VII. Conclusion

Though increased importing has numerous macroeconomic welfare benefits, import penetration has traditionally been considered unfavorable to domestic manufacturing worker welfare. Recent literature that exploits the effect of a shock that results in increased importing finds that in the post-shock period, workers fared dramatically worse than before. Here, I estimate whether, after a recovering from a period of sharp import decline associated with the Great Trade Collapse, import penetration continues to be unfavorable for worker welfare. Using a log-log difference-in-differences specification, I find that increasing import penetration by ten percent

decreases wages by one percent, and increases the likelihood of unemployment by 0.2 percent. Subgroup analysis of workers employed in industries that shrank twenty percent or more from the pre-Collapse period, and of workers with a high school education or less, however, show that increasing import competition is associated with higher wages and no adverse employment effect. These estimates are robust to three sensitivity checks, including an occupation specific analysis of workers' wages.

While distributional analysis of worker age indicates that those employed in the post-Collapse period are approximately a year and a half older--possibly pointing to a more able and experienced workforce--the wage distributions pre and post-Collapse are largely identical, suggesting that higher wages in the post period are not driving the effects. Indeed, the subgroup level positive-signed import penetration estimates are large and highly significant regardless of the exclusion of wage and competition outliers, or of narrowing the sample to production workers alone.

A possible mechanism for the favorable estimates could be changes in composition: both of firms post-Collapse, and of the function of imports post-Collapse. The Great Recession generally, and manufacturing specifically, was characterized by high firm volatility. Relative to firms that exited the market during the recession, firms that entered disproportionately engaged in technologically advanced production (Barth, et al. 2017). Firms that successfully weathered the trade downturn were likely able to do so because of decisions to cut costs by offshoring certain aspects of production in order to retain skilled domestic talent (Biddle, 2014; Barth, 2017; Brynjolfsson and McAfee, 2012). Indeed, analysis of Figure 15, which depicts the ratio of

intermediate imports[7] to total imports over the study period, demonstrates that there was a sharp increase in intermediate importing relative to total importing post-Collapse. Intermediate inputs constitute a growing share of U.S. exports, and exporting is good both for the domestic macroeconomy and for worker welfare (Ali and Dadush, 2011; Riker 2015, Tebaldi and Kim 2010). If Collapse inspired firm volatility resulted in a recovery period field of firms that disproportionately used offshored goods as intermediate inputs for domestically finished final goods, then the sharp growth of intermediates actually point to a reallocation away from import *competition* towards import *complementarity*, concomitantly enhancing domestic production and worker welfare.

A promising next research step would be to investigate the effect of increasing intermediate imports on worker welfare. Initially, such investigation could begin by performing further 3-digit NAICS subsector-specific analysis that considers worker welfare by intermediate import penetration. Ultimately, however, a firm level analysis would be ideal. Hakkala and Huttunen (2016) use Finnish matched worker-firm data to investigate the effect of imports from China and Russia on wages, likelihood of separation, and likelihood of employment. They find that though neither imports for final use nor intermediate inputs have a positive effect on worker welfare, the negative effects are much smaller for intermediate use and some estimates are positive for highly educated workers. Research into the U.S. workforce that focus on intermediate imports regardless of country of origin could be a promising next step for researching the nuanced microeconomic effect of imports on the domestic workforce.

[7]Intermediate imports are products imported as inputs for domestic production.

Table 17—General Descriptive Statistics

Variable	Pre-Collapse	Post-Collapse
Years[ǂ]	2005-2006	2010-2011
Import penetration (linear)	0.276	0.204
Import penetration (natural log)	-1.484	-2.009
Age	43.491	44.662
Unemployment	0.043	0.068
Not an English speaker	0.055	0.054
Young	0.499	0.451
Wage (natural log)*	10.397	10.452
Low education	0.574	0.547
Noncitizen	0.082	0.080
Not a veteran	0.884	0.906
Female	0.316	0.303
Black	0.084	0.090
Native American	0.006	0.007
Asian	0.051	0.059
Other race	0.001	0.001
Number of Observations	375,886 *336,566	339,559 *294,641

Note: *Statistics are for the arithmetic mean of the given variable in the pre-Collapse or post-Collapse time period. Data sourced from American Community Survey years 2005-2006 (pre-Collapse) and 2010-2011 (post-Collapse), with the exception of data marked with ǂ, which denotes lagged trade data from the American Survey of Manufactures for years 2004-2005 and 2009-2010.*

Table 18: Employment by Industry Subsector and Time Period

Industry Subsector	NAICS code	Pre-Collapse Emp.	Post –Collapse Emp.	% Change	DG	TA	EF
Food	311	33,823	37,615	11%		√	
Beverage and Tobacco	312	5,092	5,180	2%		√	
Textile Mills	313	4,975	3,727	-25%		√	√
Textile Product Mills	314	4,170	3,169	-24%		√	√
Apparel	315	7,185	6,264	-13%			
Leather and Allied Product	316	1,322	1,245	-6%			
Wood product	321	14,215	10,456	-26%	√	√	√
Paper	322	11,177	9,728	-13%		√	
Printing and Related Support	323	16,306	13,353	-18%		√	
Petroleum and Coal Products	324	4,388	4,379	0%		√	√
Chemical	325	28,485	28,354	0%			√
Plastics	326	16,443	11,517	-30%		√	√
Nonmetallic Minerals	327	12,562	10,065	-20%	√	√	√
Primary Metal	331	13,037	13,000	0%	√	√	√
Fabricated Metal	332	31,912	26,993	-15%	√		
Machinery	333	30,551	28,596	-6%	√		
Computer and Electronic Product	334	32,791	27,721	-15%	√		√
Electrical Equipment, Component, and Appliance	335	11,737	10,133	-14%	√		√
Transportation and Equipment	336	54,914	49,140	-11%	√		
Furniture and Related Product	337	13,949	10,281	-26%	√		
Miscellaneous	339	26,852	28,283	5%	√		
Total	NA	375,886	339,199	-10%			
Legend:	≥20 % decrease	1-19 % decrease	0% decrease	≥1% increase			

Note: *Abbreviations: NAICS, North American Industry Classification System; DG, durable goods; TA, tangible assets; EF, external funding.*

Data sourced from American Community Survey years 2005-2006 (pre-Collapse) and 2010-2011 (post-Collapse). Industries that shrank by 20 percent or greater are considered "shrinking industries", one of three subgroup specifications under the triple differences model. Industries that relied on tangible assets for collateral or external funding for credit, and/or that produced durable goods are denoted with checkmarks (√) in the corresponding column.

Table 19—OLS
Outcome Variable: Natural Log Wage
Mean=10.423

	I. No controls	II. State fixed effects
Import penetration	-0.067***	-0.062***
	(0.003)	(0.003)
Female	-0.364***	-0.339***
	(0.002)	(0.002)
Low education	-0.493***	-0.446***
	(0.002)	(0.002)
Young	-0.358***	-0.330***
	(0.002)	(0.002)
Shrinking industry	-0.185***	-0.165***
	(0.004)	(0.004)
Natural log export share	0.069***	0.064***
	(0.003)	(0.003)
Post Collapse	0.005***	-0.010***
	(0.003)	(0.002)
GDP	0.010***	0.019***
	(0.004)	(0.004)
Tangible assets	-0.133***	-0.113***
	(0.004)	(0.003)
Durable goods	0.033***	0.032***
	(0.003)	(0.003)
External financing	0.204***	0.184***
	(0.003)	(0.003)
Production worker	-0.247***	-0.220***
	(0.002)	(0.002)
Observations	631,207	631,207
R^2	0.221	0.323

Note: *Import penetration calculated at the three digit NAICS level in the years 2004, 2005, 2009, and 2010 as* $ln\left(\frac{Imports}{Total\,Value\,of\,Shipments\,+Imports-Exports}\right)$. *Low education< high school diploma or less. Young age<45 years. Industries that shrank by 20 percent or greater are considered "shrinking industries", one of three subgroup specifications under the triple differences model. Export share is calculated as* $\frac{Exports}{Total\,Value\,of\,Shipments}$. *GDP denotes change in gross domestic product, calculated as real GDP-potential GDP. Data on real GDP is sourced from the World Bank, and data on potential GDP is from the Congressional Budget Office. Tangible assets, durable goods, and external financing industry denotation sourced from Haltenhof, Lee, and Stebunovs (2014), and listed in Table 2. Demographic data is from the American Community Survey. Trade data is from the American Survey of Manufactures. Statistically Significant at p≤0.1 , *; at p≤0.05 , **; at p≤0.01 , ***.*

Table 20—Probit
Outcome Variable: Unemployment
Mean=0.055
Marginal Effects

	I. No controls	II. State fixed effects
Import Penetration	0.006*** (0.001)	0.005* (0.003)
Female	0.009*** (0.001)	-0.045*** (0.003)
Low education	0.021*** (0.001)	0.007*** (0.003)
Young	0.014*** (0.001)	0.099*** (0.002)
Shrinking industry	0.010*** (0.001)	0.024*** (0.004)
Export share	-0.001* (0.001)	0.007** (0.003)
Post Collapse	0.031*** (0.001)	0.126*** (0.003)
GDP	0.006*** (0.001)	0.013*** (0.005)
Tangible assets	0.005*** (0.001)	0.004 (0.004)
Durable goods	-0.002*** (0.001)	0.005 (0.003)
External financing	-0.006*** (0.001)	-0.004 (0.003)
Production worker	0.009*** (0.001)	0.003 (0.002)
Observations	715,445	142,000
R2	0.028	0.033

Note: *Import penetration calculated at the three digit NAICS level in the years 2004, 2005, 2009, and 2010 as* $ln\left(\frac{Imports}{Total\ Value\ of\ Shipments + Imports - Exports}\right)$. *Low education< high school diploma or less. Young age<45 years. Industries that shrank by 20 percent or greater are considered "shrinking industries", one of three subgroup specifications under the triple differences model. Export share is calculated as* $ln\left(\frac{Exports}{Total\ Value\ of\ Shipments}\right)$. *Note: GDP denotes change in gross domestic product, calculated as real GDP-potential GDP. Data on real GDP is sourced from the World Bank, and data on potential GDP is from the Congressional Budget Office. Tangible assets, durable goods, and external financing industry denotation sourced from Haltenhof, Lee, and Stebunovs (2014), and listed in Table 2. Demographic data is from the American Community Survey. Trade data is from the American Survey of Manufactures. Statistically Significant at p≤ 0.1 , *; at p≤0.05 , **; at p≤0.01 , ***.*

Table 21—Difference-in-Differences & Triple Differences
Outcome Variable: Natural Log Wage
Mean=10.423

	(I) No Controls	(II) Young age, low education, shrinking industry indicators	(III) Indicators + Demo-graphics	(IV) Indicators + Demo. + GDP	(V) Indicators + Demo.+ GDP + Credit Crunch	(VI) Indicators + Demo.+ GDP + Credit Crunch+ State FE
Import penetration * Post-Collapse	-0.007 (0.006)	-0.021*** (0.006)	-0.007 (0.005)	-0.007 (0.005)	0.005 (0.005)	-0.010** (0.005)
Import pen.* Post-Collapse* Shrinking	0.380*** (0.020)	0.357*** (0.020)	0.184*** (0.019)	0.185*** (0.019)	0.154*** ()0.019	0.134*** (0.018)
Import pen.* Post-Collapse* Low Education	0.097*** ()0.008	0.111*** (0.008)	0.063*** (0.008)	0.062*** (0.008)	0.063*** (0.008)	0.049*** (0.007)
Import pen.* Post-Collapse* Durable Goods	- 0.079*** (0.14)	-0.034*** (0.013)	-0.064*** (0.013)	-0.062*** (0.13)	-0.029** (0.013)	-0.024** (0.012)
Observations	631,207	631,207	631,207	631,207	631,207	631,207
R^2	0.022 0.29 0.121 0.028	0.152 0.156 0.157 0.156	0.198 0.201 0.202 0.200	0.198 0.201 0.202 0.201	0.205 0.209 0.210 0.208	0.313 0.313 0.314 0.313

Note: *Import penetration calculated at the three digit NAICS level in the years 2004, 2005, 2009, and 2010 as* $ln\left(\frac{Imports}{Total\ Value\ of\ Shipments + Imports - Exports}\right)$*. Low education< high school diploma or less. Young age<45 years. Industries that shrank by 20 percent or greater are considered "shrinking industries", one of three subgroup specifications under the triple differences model. Demographic controls include sex, citizenship status, English proficiency,veteran status, and the social construct "race".*
*GDP denotes change in gross domestic product, calculated as real GDP-potential GDP. Data on real GDP is sourced from the World Bank, and data on potential GDP is from the Congressional Budget Office. "Credit crunch" variables are tangible assets, durable goods, and external financing industry, as defined in Haltenhof, Lee, and Stebunovs (2014), and listed in Table 2. Demographic data is from the American Community Survey. Trade data is from the American Survey of Manufactures. Statistically Significant at p≤ 0.1 , *; at p≤0.05 , **; at p≤0.01 , ***.*

Table 22—Difference-in-Differences & Triple Differences
Outcome Variable: Unemployment
Mean=0.055

	(I) No Controls	(II) Young age, low education, shrinking industry indicators	(III) Indicat. + Demographics	(IV) Indicat. + Demo. + GDP	(V) Indicat. + Demo.+ GDP + Credit Crunch	(VI) Indicat. + Demo.+ GDP + Credit Crunch+ State FE
Import penetration* Post-Collapse	0.004***	0.004***	0.004***	0.004***	0.003***	0.020***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.005)
Import penetration* Post-Collapse* Shrinking	-0.010**	-0.009**	-0.001	-0.001	-0.000	0.025
	(0.004)	(0.004)	(0.004)	(0.004)	0.000	(0.018)
Import penetration* Post-Collapse* Low Education	-0.003	-0.005***	-0.002	-0.002	-0.002	0.004
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.006)
Import penetration* Post-Collapse* Durable Goods	0.014***	0.013***	0.014***	0.013***	0.013***	0.040***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.012)
Observations	715,445	715,445	715,445	715,445	715,445	142,000
R^2	0.008	0.018	0.026	0.026	0.027	0.033
	0.008	0.018	0.026	0.026	0.027	0.033
	0.009	0.019	0.027	0.027	0.027	0.033
	0.016	0.019	0.027	0.027	0.027	0.033

Note: *Import penetration calculated at the three digit NAICS level in the years 2004, 2005, 2009, and 2010 as* $ln\left(\frac{Imports}{Total\ Value\ of\ Shipments + Imports - Exports}\right)$. *Low education< high school diploma or less. Young age<45 years. Industries that shrank by 20 percent or greater are considered "shrinking industries", one of three subgroup specifications under the triple differences model. Demographic controls include sex, citizenship status, English proficiency, veteran status, and the social construct "race".*
GDP denotes change in gross domestic product, calculated as real GDP-potential GDP. Data on real GDP is sourced from the World Bank, and data on potential GDP is from the Congressional Budget Office.
*Tangible assets, durable goods, and external financing industry denotation sourced from Haltenhof, Lee, and Stebunovs (2014), and listed in Table 2. Demographic data is from the American Community Survey. Trade data is from the American Survey of Manufactures. Difficulties in estimation of the probit regression with state fixed effects lead to column VI being "N.E.", or not estimable. Statistically Significant at p≤ 0.1 , *; at p≤0.05 , **; at p≤0.01 , ***.*

Table 23a—Distributional Analysis of Worker Subgroups

Age						
	Low-ed		Shrinking		Durable Goods	
	Pre	Post	Pre	Post	Pre	Post
Mean	43.4	44.7	42.7	44.6	43.6	45.0
First Quartile	34	35	33	35	35	36
Median	45	47	44	46	45	47
Third Quartile	53	55	52	55	53	55
Skew	-0.3	-0.4	-0.2	-0.3	-0.2	-0.3
Kurtosis	2.1	2.1	2.1	2.1	2.2	2.1

Table 23b—Distributional Analysis of Worker Subgroups

Wage						
	Low-ed		Shrinking		Durable Goods	
	Pre	Post	Pre	Post	Pre	Post
Mean	10.1	10.1	10.2	10.2	10.4	10.5
First Quartile	9.8	9.9	9.9	9.9	10.1	10.1
Median	10.3	10.3	10.3	10.3	10.5	10.6
Third Quartile	10.6	10.7	10.7	10.8	11.0	11.1
Skew	-1.9	-1.7	-1.6	-1.4	-1.4	-1.3
Kurtosis	9.3	7.9	9.0	7.3	8.2	7.0

Age and wage variable distributions for the pre-Collapse period (2005-2006) and the post-Collapse period (2010-2011). The skewness is the third moment of a variable; the increasingly negative skew of the age distribution from the pre-Collapse period to the post-Collapse period indicates that the mass of the distribution is concentrated on higher ages. The decreasingly negative skew of the wage distribution post-Collapse means that in the wage distribution became more centered about the mean.
The kurtosis is the fourth moment of a random variable. As a reference, a normally distributed random variable will have a kurtosis of 3. The decreasing kurtosis from the pre-Collapse to post-Collapse period indicates that the wage distribution post-Collapse was less "tailed", and had fewer extreme values (Westfall, 2014). The steady kurtosis of the age distribution indicates that there was no appreciable increase in the number of extreme values for age (very young or very old workers), and suggests that the entire distribution shifted rightward about higher mean post-Collapse.
Data sourced from the American Community Survey.

Table 24—Difference-in-Differences & Triple Differences Sensitivity Analysis
Outcome Variable: Natural Log Wage
Retained only wage observations above the 1st and below the 99th percentile of the original distribution
Mean=10.440

	(I)	(II) Young age, low education, shrinking industry indicators	(III) Indicators + Demographics	(IV) Indicators + Demo.+ GDP	(V) Indicators + Demo. + GDP + Credit Crunch	(VI) Indicators + Demo. + GDP + Credit Crunch+ State FE
Import penetration* Post-Collapse	-0.005	-0.018***	-0.004	-0.004	0.007	-0.004
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)
Import penetration* Post-Collapse* Shrinking	0.368***	0.347***	0.186***	0.187***	0.161***	0.137***
	(0.018)	(0.017)	(0.017)	(0.017)	(0.017)	(0.016)
Import penetration* Post-Collapse* Low Education	0.095***	0.111***	0.064***	0.063***	0.065***	0.052***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)
Import penetration* Post-Collapse* Durable Goods	-0.068***	-0.027**	-0.058***	-0.055***	-0.026**	-0.022**
	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)	(0.010)
Observations	619,163	619,163	619,163	619,163	619,163	619,163
R^2	0.024	0.156	0.206	0.206	0.214	0.307
	0.031	0.161	0.209	0.209	0.217	0.310
	0.127	0.162	0.210	0.211	0.219	0.311
	0.030	0.160	0.209	0.209	0.217	0.310

Import penetration calculated at the three digit NAICS level in the years 2004, 2005, 2009, and 2010 as $ln\left(\frac{Imports}{Total\ Value\ of\ Shipments + Imports - Exports}\right)$. *Low education< high school diploma or less. Young age<45 years. Industries that shrank by 20 percent or greater are considered "shrinking industries", one of three subgroup specifications under the triple differences model. Demographic controls include sex, citizenship status, English proficiency,veteran status, and the social construct "race"..*
GDP denotes change in gross domestic product, calculated as real GDP-potential GDP. Data on real GDP is sourced from the World Bank, and data on potential GDP is from the Congressional Budget Office.
*Tangible assets, durable goods, and external financing industry denotation sourced from Haltenhof, Lee, and Stebunovs (2014), and listed in Table 2. Demographic data is from the American Community Survey. Trade data is from the American Survey of Manufactures. Statistically Significant at p≤ 0.1 , *; at p≤0.05 , **; at p≤0.01 , ***.*

Table 25—Difference-in-Differences & Triple Differences Analysis by Occupation
Outcome Variable: Natural Log Wage
Mean for Non-production=10.586
Mean for Production=10.130

	(I) Entire Sample Indicators + Demographics+ GDP + Credit Crunch+ State FE	(II) Non-production workers Indicators + Demographics+ GDP + Credit Crunch+ State FE	(III) Production workers Indicators + Demographics + GDP + Credit Crunch+ State FE
Import penetration* Post-Collapse	-0.010*	-0.015**	0.005
	(0.005)	(0.006)	(0.007)
Import penetration* Post-Collapse* Shrinking	0.134***	0.116***	0.112***
	(0.018)	(0.025)	(0.026)
Import penetration* Post-Collapse* Low Education	0.049***	0.014	0.094***
	(0.007)	(0.010)	(0.011)
Import penetration* Post-Collapse* Durable Goods	-0.024**	-0.010	-0.006
	(0.012)	(0.16)	(0.018)
Observations	631,207	405,865	225,342
R^2	0.313	0.324	0.247
	0.313	0.325	0.247
	0.314	0.326	0.247
	0.313	0.325	0.247

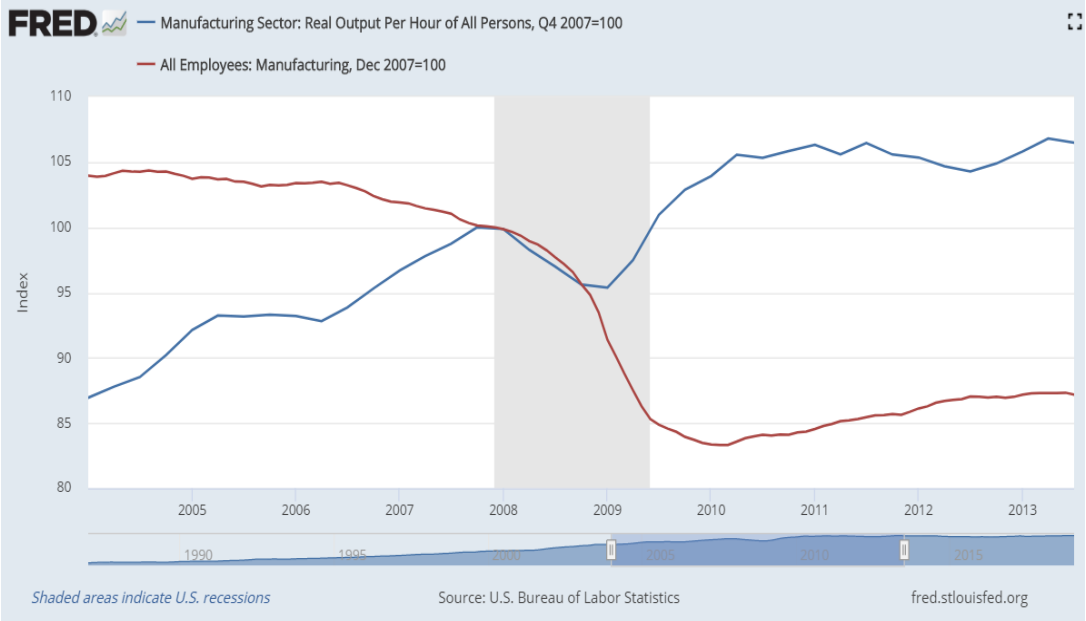
Import penetration calculated at the three digit NAICS level in the years 2004, 2005, 2009, and 2010 as $ln\left(\frac{Imports}{Total\,Value\,of\,Shipments\,+Imports-Exports}\right)$. *All results in table 9 are estimated with the set of full controls, including indicators for youth, low education, and shrinking industry, demographic controls (sex, citizenship status, English proficiency,veteran status, and the social construct "race"). Low education< high school diploma or less. Young age<45 years. Industries that shrank by 20 percent or greater are considered "shrinking industries", one of three subgroup specifications under the triple differences model. GDP denotes change in gross domestic product, calculated as real GDP-potential GDP. Data on real GDP is sourced from the World Bank, and data on potential GDP is from the Congressional Budget Office. Tangible assets, durable goods, and external financing industry denotation sourced from Haltenhof, Lee, and Stebunovs (2014), and listed in Table 2. Demographic data is from the American Community Survey. Trade data is from the American Survey of Manufactures. Statistically Significant at p≤ 0.1 , *; at p≤0.05 , **; at p≤0.01 , ***.*

Table 26—Difference-in-Differences & Triple Differences Sensitivity Analysis
Outcome Variable: Natural Log Wage
Retained only import penetration observations above the 5th and below the 95th percentile of the original distribution
Mean=10.423

	(I)	(II) Young age, low education, big red indicators	(III) Indicat.+ Demographics	(IV) Indicat. + Demo.+ GDP	(V) Indicat. + Demo.+ GDP + Credit Crunch	(VI) Indicat.+ Demo.+ GDP + Credit Crunch+ State FE
Import penetration* Post-Collapse	-0.079***	-0.050***	-0.048***	-0.023***	-0.023***	-0.030***
	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Import penetration* Post-Collapse* Shrinking	0.383***	0.374***	0.204***	0.203***	0.239***	0.209***
	(0.021)	(0.020)	(0.020)	(0.020)	(0.020)	(0.019)
Import penetration* Post-Collapse* Low Education	0.042***	0.051***	0.045***	0.043***	0.063***	0.035***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.009)
Import penetration* Post-Collapse* Durable Goods	-0.123***	-0.049**	-0.086***	-0.085***	-0.041**	-0.032**
	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)	(0.013)
Observations	575,544	575,544	575,544	575,544	575,544	575,544
R^2	0.024	0.156	0.206	0.206	0.214	0.307
	0.032	0.156	0.197	0.197	0.203	0.308
	0.117	0.155	0.197	0.197	0.203	0.307
	0.150	0.193	0.193	0.199	0.199	0.304

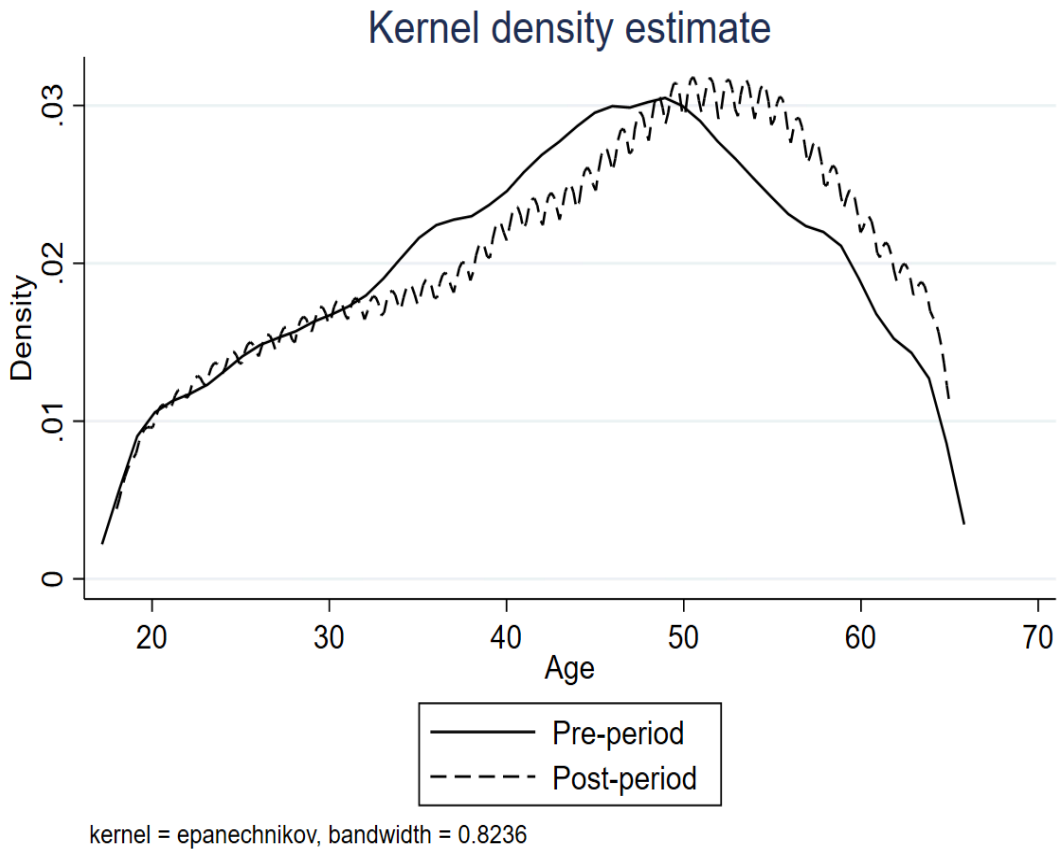
Import penetration calculated at the three digit NAICS level in the years 2004, 2005, 2009, and 2010 as $ln\left(\frac{Imports}{Total\ Value\ of\ Shipments + Imports - Exports}\right)$*. . All results in table 9 are estimated with the set of full controls, including indicators for youth, low education, and shrinking industry, demographic controls (sex, citizenship status, English proficiency,veteran status, and the social construct "race"). Low education< high school diploma or less. Young age<45 years. Industries that shrank by 20 percent or greater are considered "shrinking industries", one of three subgroup specifications under the triple differences model. GDP denotes change in gross domestic product, calculated as real GDP-potential GDP. Data on real GDP is sourced from the World Bank, and data on potential GDP is from the Congressional Budget Office. Tangible assets, durable goods, and external financing industry denotation sourced from Haltenhof, Lee, and Stebunovs (2014), and listed in Table 2. Demographic data is from the American Community Survey. Trade data is from the American Survey of Manufactures. Statistically Significant at p≤ 0.1 , *; at p≤0.05 , **; at p≤0.01 , ***.*

Figure 11: Manufacturing Output, Years 2004-2013



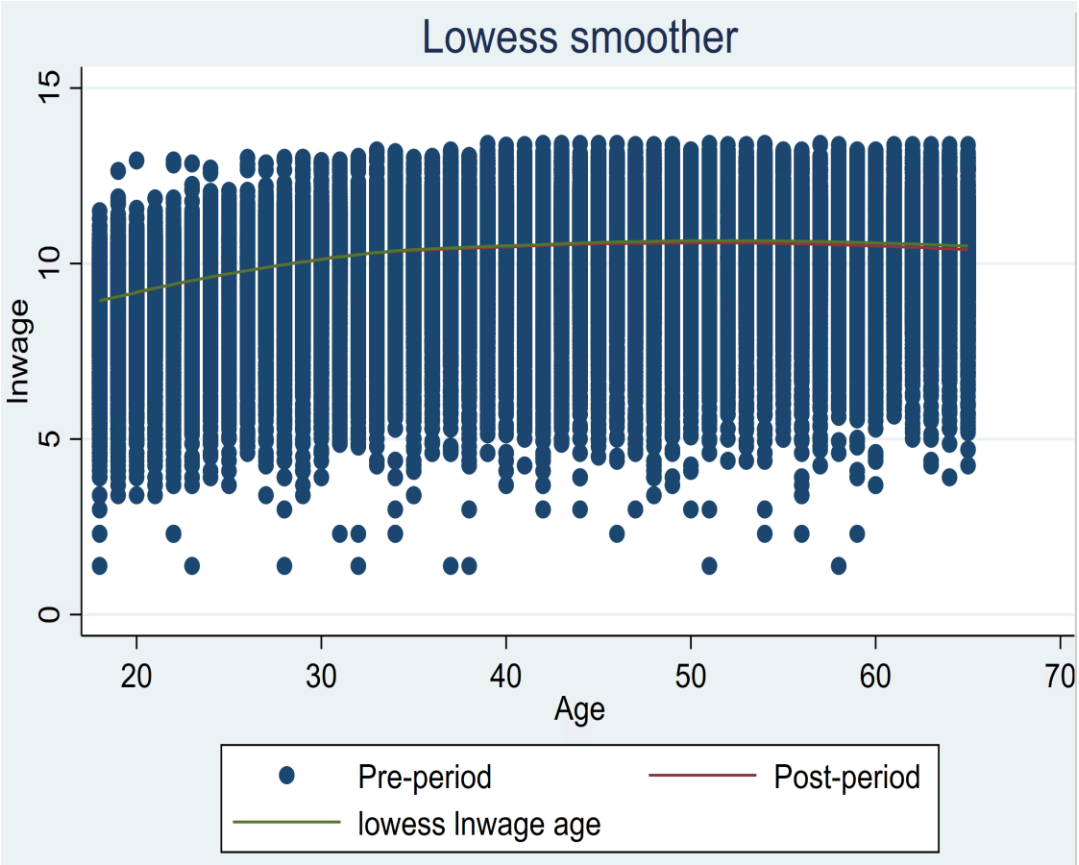
Note: *Source: (U.S. Bureau of Economic Analysis 2017). The Great Recession period is shaded in gray. The red line depicts manufacturing employment, which declined sharply during the Great Recession period, and has been slowly inching upwards since 2010. The blue line, depicting manufacturing output, shows the sharp reduction in output during the Recession period, but then an equally as sharp rebound in output before the completion of Recession, and at the end of the Collapse period, starting in 3rd quarter 2019.*

Figure 12: Age Distribution by Time Period



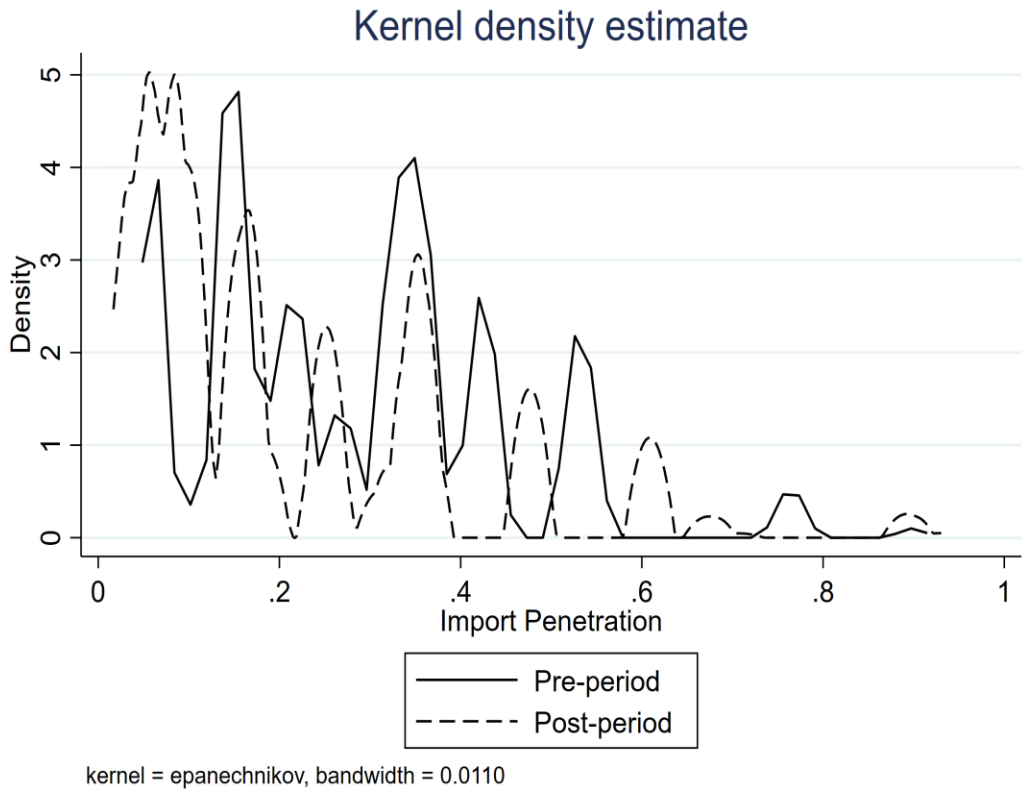
Note: *The kernel density graph depicts the age sample distribution of the population of 18-65 year old manufacturing employees. The pre-Collapse (2005-2006) period is plotted in solid line, whereas the post-Collapse period (2010-2011) is plotted in dashed line. The juxtaposition of distributions demonstrates that post-Collapse, the proportion of middle aged workers (35-50 year olds) fell, and the proportion of older employees (50-65 year-olds) rose. Data sourced from the American Community Survey.*

Figure 13: Relationship Between Wage and Age in the Pre- versus Post-Collapse era



Note: *The locally weighted non-parametric smoother (hereafter "lowess smoother") assigns a fitted value to each data point of the scatterplot, and graphically plots a smooth curve through the scatterplot. The pre-period is plotted as a green line, and the post period is plotted as a red line. The lowess smoother demonstrates wage is increasing in age, and that this relationship is virtually unchanged by time period. Data sourced from the American Community Survey.*

Figure 14: Import Penetration by Time Period

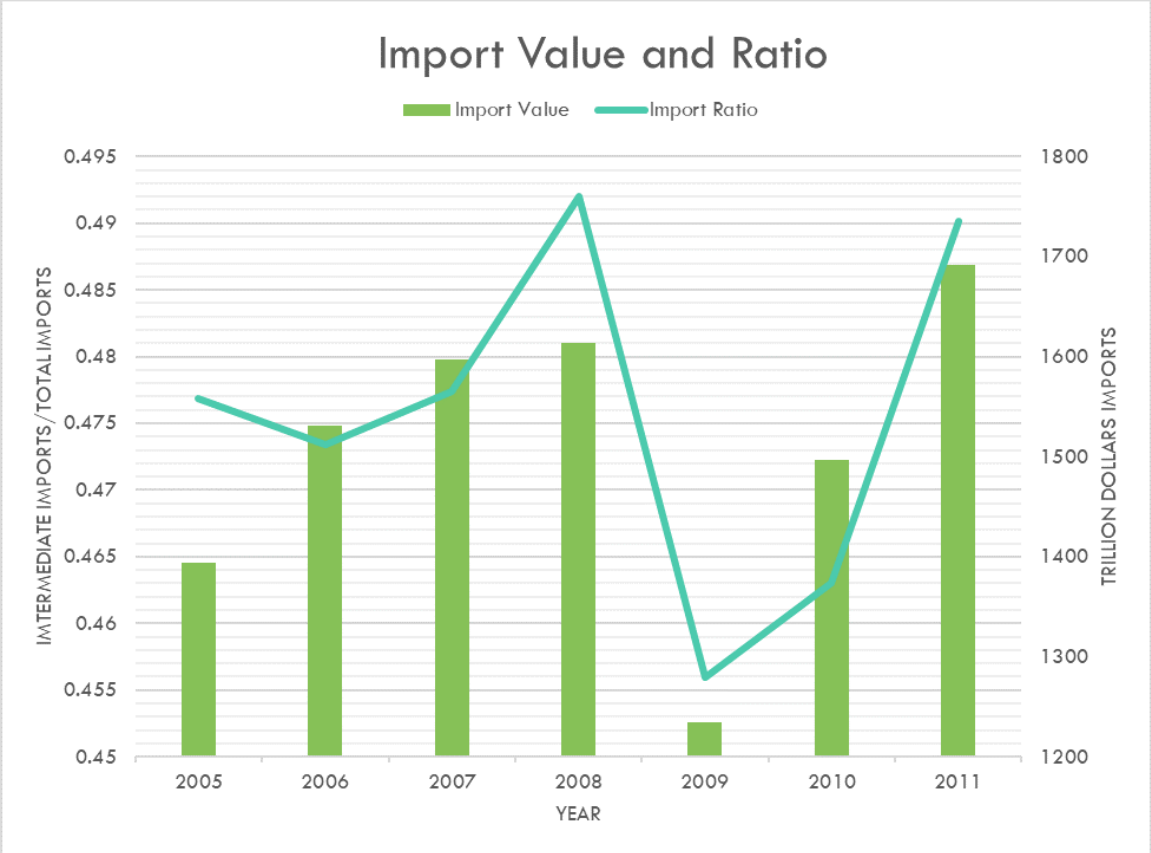


Import Competition by Period

	Pre-Collapse	**Post-Collapse**
Mean	0.276	0.204
Median	0.269	0.137
75th percentile	0.367	0.334
95th percentile	0.771	0.892

Note: *The kernel density graph depicts the import penetration sample distribution of the 3 digit NAICS subsector industries in the pre- and post-Collapse periods. The pre-Collapse (2004-2005) period is plotted in solid line, whereas the post-Collapse period (2009-2010) is plotted in dashed line. (Note that these lagged import penetration values by year are matched to worker microdata for years 2005-2006 and 2010-2011.)The juxtaposition of distributions demonstrates that post-Collapse, a greater proportion of industries imported a small amount (notice the high and wide dashed peak at penetration level 0 – 0.2 penetration, as opposed to the sharper peaks at 0.1-0.2 and 0.3-0.4 penetration level in the pre-Collapse period). Data sourced from the American Survey of Manufactures.*

Figure 15: Ratio Between Intermediate Imports and Total Imports



***Note**: This graph depicts the ratio of intermediate import to total imports over the Collapse window on the left axis, and the volume of imports on the right axis. Note that intermediate imports as a proportion of total imports fell sharply during the Collapse, then climbed steadily from 2009-2011. As intermediate imports feed directly into domestic production and export sales, it is possible that this increasing post-Collapse ratio of intermediate imports to total imports served as a proxy for a more profitable domestic manufacturing environment. Source: Bureau of Economic Analysis Import Matrices Before Redefinitions, 2018*

Appendix

Table A1: Fertility Rates by Country of Origin

Niger	7.738	Madagascar	5.548
Somalia	7.61	Central African Republic	5.446
Afghanistan	7.496	Sudan	5.444
Chad	7.354	Togo	5.41
Timor-Leste	7.112	**---end of 3rd quartile---**	
Burundi	7.057	Mauritania	5.403
Congo, Dem. Rep.	7.053	West Bank and Gaza	5.39
Angola	6.905	Comoros	5.384
Mali	6.897	Eritrea	5.328
Uganda	6.865	Sao Tome and Principe	5.278
Burkina Faso	6.592	Congo, Rep.	5.109
Ethiopia	6.529	Kenya	5.012
Yemen, Rep.	6.335	Iraq	4.888
Sierra Leone	6.257	Solomon Islands	4.72
Malawi	6.25	Ghana	4.67
South Sudan	6.223	Pakistan	4.58
Nigeria	6.106	Gabon	4.539
Zambia	6.097	Papua New Guinea	4.525
Guinea	6.082	Samoa	4.503
Benin	5.975	Djibouti	4.472
Gambia, The	5.917	Guatemala	4.434
Liberia	5.88	Vanuatu	4.368
Cote d'Ivoire	5.861	Lao PDR	4.304
Guinea-Bissau	5.818	Haiti	4.302
Mozambique	5.816	Micronesia, Fed. Sts.	4.3
Equatorial Guinea	5.773	Tonga	4.25
Tanzania	5.689	Swaziland	4.209
Rwanda	5.64	Lesotho	4.089
Cameroon	5.621	Zimbabwe	4.065
Senegal	5.552	Kiribati	4.058

Jordan	4.056
Bolivia	4.055
Nepal	4.03
Namibia	4.018
Honduras	3.993
Saudi Arabia	3.976
Tajikistan	3.969
Syrian Arab Republic	3.958
Philippines	3.814
---end of 2nd quartile---	
Cambodia	3.805
Oman	3.719
Cabo Verde	3.666
Bhutan	3.604
Belize	3.6
Paraguay	3.553
Botswana	3.413
India	3.311
Qatar	3.24
Egypt, Arab Rep.	3.233
Bangladesh	3.169
Fiji	3.09
Nicaragua	3.083
Ecuador	3.027
Guyana	3.022
Kosovo	2.95
Israel	2.95
Maldives	2.945
Libya	2.931
Peru	2.929
Myanmar	2.903
El Salvador	2.898
Dominican Republic	2.892
South Africa	2.866
Kuwait	2.847
Turkmenistan	2.837
Malaysia	2.825
Guam	2.824
Venezuela, RB	2.822
Suriname	2.81
Bahrain	2.765
Mexico	2.747
Panama	2.705
Morocco	2.691
---end of 1st quartile---	
United Arab Emirates	2.644
Faroe Islands	2.6
New Caledonia	2.59
Grenada	2.582
Uzbekistan	2.58
Jamaica	2.577
Argentina	2.561
Algeria	2.514
Indonesia	2.483
Turkey	2.479
French Polynesia	2.463
Kyrgyz Republic	2.4
Colombia	2.389
St. Vincent and the Grenadines	2.379
Costa Rica	2.373
Brazil	2.364
Greenland	2.329
Antigua and Barbuda	2.316
St. Lucia	2.313
Brunei Darussalam	2.278
Uruguay	2.242
Sri Lanka	2.241
Lebanon	2.225
Iran, Islamic Rep.	2.211

Albania	2.16
Mongolia	2.143
Tunisia	2.142
Iceland	2.08
Seychelles	2.08
Chile	2.073
Bahamas, The	2.071
Virgin Islands (U.S.)	2.06
United States	**2.056**
Puerto Rico	2.0485
Vietnam	2.01
Azerbaijan	2
Korea, Dem. People's Rep.	1.991
Mauritius	1.99
New Zealand	1.98
France	1.89
Ireland	1.89
Montenegro	1.875
Aruba	1.874
Norway	1.85
St. Martin (French part)	1.83
Kazakhstan	1.8
Denmark	1.77
Luxembourg	1.76
Australia	1.756
Trinidad and Tobago	1.753
Barbados	1.744
Bermuda	1.74
Finland	1.73
Macedonia, FYR	1.723
Netherlands	1.72
Cyprus	1.714
Malta	1.7
Armenia	1.688
Thailand	1.671
Belgium	1.67
Cuba	1.642
United Kingdom	1.64
Georgia	1.609
Singapore	1.6
Liechtenstein	1.57
Portugal	1.55
Sweden	1.54
Switzerland	1.5
Canada	1.49
Serbia	1.48
Korea, Rep.	1.467
China	1.447
Moldova	1.435
Bosnia and Herzegovina	1.413
Channel Islands	1.402
Croatia	1.39
Lithuania	1.39
Germany	1.38
Poland	1.37
Austria	1.36
Estonia	1.36
Japan	1.359
Hungary	1.32
Belarus	1.317
Romania	1.31
Slovak Republic	1.3
Bulgaria	1.26
Italy	1.26
Slovenia	1.26
Greece	1.25
Latvia	1.25
Spain	1.22

Russian Federation	1.195
Czech Republic	1.15
Ukraine	1.11
Hong Kong SAR, China	1.035
Macao SAR, China	0.939

Table A2: Pre-Period (before 1997) Summary Statistics

Variable	Prefer	Non-Prefer	Difference (Pref.-Non-Pref.)	P-value Ho: diff = 0 Ha: diff != 0
Family's total inc.	45263.18	43937.18	-1325.992	Pr(|T| > |t|) = 0.00
Married	.577865	.5912655	.0134006	Pr(|T| > |t|) = 0.00
Number of Children	1.073252	.9073302	-.165922	Pr(|T| > |t|) = 0.00
Less than high school	.2206455	.1666829	-.0539626	Pr(|T| > |t|) = 0.00
Age	42.08021	42.28894	.20873	Pr(|T| > |t|) = 0.0231
Number of children <1	.037423	.0353682	-.0020548	Pr(|T| > |t|) = 0.0677
Observations	32,627	164,498	-173,867	NA

Panel A: Prefer Women versus Non-Prefer Women

Variable	High fertility immigrant	Low fertility immigrant	Difference (High-Low)	P-value Ho: diff = 0 Ha: diff != 0
Family's total inc.	28406.21	39901.96	-11495.75	Pr(|T| > |t|) = 0.00
Married	.6174103	.6427472	-.0253369	Pr(|T| > |t|) = 0.00
Number of Children	1.484777	.9024625	.5823144	Pr(|T| > |t|) = 0.00
Less than high school	.5612423	.2310504	.3301919	Pr(|T| > |t|) = 0.00
Age	36.49878	41.36937	-4.870598	Pr(|T| > |t|) = 0.00
Number of children <1	.0741032	.0394382	.034665	Pr(|T| > |t|) = 0.00
Observations	11,430	5,198	6,232	NA

Panel B: High Fertility (HF) COO Immigrant Women versus Low Fertility COO Immigrant Women

Table A2: Pre-Period (before 1997) Summary Statistics (continued)

Variable	Religious	Non-Religious	Difference (Relig.-Non-Relig.)	P-value Ho: diff = 0 Ha: diff != 0
Family's total inc.	47267.87	43629.58	3638.29	Pr(|T| > |t|) = 0.00
Married	.5698778	.5922951	-.0224173	Pr(|T| > |t|) = 0.00
Number of Children	.9820009	.9267951	.0552058	Pr(|T| > |t|) = 0.00
Less than high school	.1661939	.1772104	-.0110165	Pr(|T| > |t|) = 0.00
Age	42.24975	42.25518	.0054328	Pr(|T| > |t|) = 0.96
Number of children <1	.0363484	.0355999	.0007485	Pr(|T| > |t|) = 0.53
Observations	28,557	168,568	-140,011	NA

Panel C: HF Religious State Women versus Non-HF Religious State Women

Note: *Women ages 15-44 are represented in both the authorized and unauthorized populations in the years 1993-1995 (CPS years 1994-1996). Because of the disparity between year of immigration (1981 or earlier) and year of birth, women age 15 are omitted from the 1997 (CPS year 1998) sample, women ages 15-16 are omitted from the 1998 (CPS 1999), and women age 15-17 are omitted from the 1999 (CPS 2000).*

Table A3: Refugee and Asylee Countries of Origin

Africa

- Ethiopia
- Somalia
- Liberia
- Sudan

Europe

- Hungary
- Poland
- Romania
- Yugoslavia

Americas

- Nicaragua
- Cuba
- Haiti

Asia

- Syria
- Iraq
- Iran
- Kazakhstan
- Uzbekistan
- Azerbaijan
- Afghanistan
- Vietnam
- Thailand
- Laos
- Cambodia
- China
- Ukraine
- Belarus
- Moldova
- USSR/ Russia
- Estonia
- Latvia
- Lithuania

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BIOGRAPHY

Stephanie Fortune-Taylor is an economist for the U.S. federal government. Her research areas include labor and public finance, and focus on how lower-income Americans navigate the financial complexities of everyday life. A Southern Regional Education Board Doctoral Scholar, Fortune-Taylor has presented her research at both academic institutions and governmental organizations. She is married to Eric, a mathematician, and is mother to David, age five, and Erica, age two.