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FERTILITY AND THE PERSONAL EXEMPTION: COMMENT

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ABSTRACT

One of the most commonly cited studies on the effect of child subsidies on fertility, Whittington, Alm and Peters (1990), claimed a large positive effect of child tax benefits on fertility using time series methods. We revisit this question in light of recent increases in child tax benefits by replicating this earlier study and extending the analysis. We do not find strong evidence to justify the model specification from the original paper. Moreover, even if the original specfication is appropriate, we show that the Whittington et al. results are not robust to more general measures of child tax benefits. While we do not find evidence that child tax benefits affect the level of fertility, we find some evidence of a short-run fertility response that occurs with a two-year lag.

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1 Introduction

Standard economic theory tells us that the demand for children is influenced by the cost of raising children. Holding other things constant, a decrease in the cost of raising children should lead to an increase in the demand for children. As shown in Figure 1, the average value of the U.S. child tax subsidy adjusted for inflation has increased from under $850 in 1980 to more than $2,000 in 2005.[1] The U.S.D.A. estimates that annual expenditures on children range from $7,580 to $16,970 depending on the age of the child and household income (Lino 2007); thus, the $1,150 real increase in child tax benefits can be thought of as a 7 to 15 percent discount on the cost of raising children. How much of an effect (if any) did this reduction in the cost of raising children have on fertility?

Whittington, Alm and Peters (1990) were the first to seriously estimate the responsiveness of fertility to child tax benefit changes. Their analysis of time series data from 1913 to 1984 suggests that the U.S. fertility rate is very responsive to child tax benefits. They estimate that a $100 increase (in 2005 dollars) in the tax value of the personal exemption would increase the general fertility rate by 2.1 to 4.2 births (a 3.2 to 6.5 percent increase).[2]

While the sign of the estimated effect is not unexpected, the strong and robust magnitude of the Whittington et al. (1990) estimate is surprising. If a $100 increase in annual child tax benefits could increase fertility by 3.2 to 6.5 percent, should we have expected a 32 to 65 percent increase in the U.S. fertility rate in response to the $1,000 Child Tax Credit, holding all other factors constant?[3]

Since Whittington et al. (1990), a handful of empirical studies have estimated a fertility response from changes in child tax benefits or other child subsidies. One set of papers uses

[1]The details regarding the calculation of the average per-child tax subsidy are given in the Appendix.

[2]Whittington et al. report their results in 1967 dollars. Their estimates of the effect of the value of the personal exemption in 1967 dollars on the general fertility rate range from 0.121 to 0.236. Converting the dollar amounts to 2005 dollars using the CPI-U, we find that their estimates range from 0.021 to 0.042.

[3]From 1997 (the year the Child Tax Credit was passed) to 2005, the general fertility rate in the United States increased by 4.9 percent. Note however that eligibility restrictions and interactions in the tax code make the $1,000 Child Tax Credit worth much less than this amount on average. From 1997 to 2005, the average child subsidy increased by approximately $550 in real terms.

similar aggregate time-series or pooled time-series methods to examine the long-run effect of child tax benefits on fertility (e.g. Georgellis and Wall (1992), Zhang, Quan and van Meerbergen (1994), Gauthier and Hatzius (1997), Huang (2002)). These studies generally find that fertility responds to tax benefits, though the estimated responses are smaller than that found by Whittington et al.

Another set of studies uses individual data and finds mixed results as to whether financial incentives influence fertility in the short run. While Whittington (1992) finds evidence in the PSID that tax benefits strongly influence family size in the United States, Baughman and Dickert-Conlin (2003) find that the largest estimated fertility response to Earned Income Tax Credit (EITC) expansions in the 1990s (for married non-white women) was less than half the magnitude reported in Whittington et al. and many subpopulations display no economically significant response. Similarly, Laroque and Salanie (2005) find evidence of only a small effect on fertility in France, despite the generosity of French child subsidies.

Milligan (2005) reports fertility response estimates of a similar magnitude as Whittington et al. (1990) using data from Quebec. However, it is likely this large fertility effect is in part due to the temporary nature of the Quebec subsidy program; Parent and Wang (2007) show that women may have had children earlier in order to claim the subsidy with no change in their completed fertility. Most recently, Cohen, Dehejia and Romanov (2007) find strong effects of financial incentives on fertility among low-income populations in Israel.

Despite the lack of agreement in the literature, Whittington et al. (1990) is cited by an increasing number of publications (many in non-economics journals) as evidence of a strong link between child tax benefits and fertility. In this paper, we revisit and extend the analysis in Whittington et al. along two dimensions. First, we update the data series with 21 additional years of data and broader measures of child tax benefits. While Whittington et al.'s analysis was limited to the real tax value of the personal exemption, we incorporate the child tax credit (CTC) and the earned income tax credit (EITC) in our measure of child subsidies. As illustrated in Figure 1, these additional components of child tax benefits grew in importance over the last two decades and account for much of the significant growth in the value of the average child tax subsidy; currently, they make up more than half of the total subsidy available to families with children. Extending and updating the data series allows us to develop more precise estimates of the relationship between fertility and child tax benefits and reexamine the relationship in light of recent increases in these subsidies.

Second, we also revisit the model specification and estimation procedure from the original paper. We find that the variables in the analysis are highly persistent which raises concerns about the potential for spurious regression results using the authors' original specification. Furthermore, we do not find strong evidence to justify the model specification from the original paper.

We also show that even if the original specification is correct, the results of Whittington et al. (1990) are specific only to the personal exemption series and are not robust to broader measures of tax subsidies. Because a tax subsidy in the form of a child tax credit should affect fertility in the same way as a tax subsidy from the personal exemption, this finding casts additional doubt on the results of Whittington et al.

Finally, we provide an illustrative analysis of the short-run effects of child tax benefits on the general fertility rate by estimating the models in first differences, under the assumption that the variables we found to be highly persistent are in fact unit roots. We find evidence that child tax benefits increase fertility with a two-year lag. However, the total short-run effect is not statistically different from zero. These results suggest that tax benefits do not affect the overall level of fertility, but are consistent with an effect on the timing of fertility.

The paper is organized as follows. Section 2 describes the estimation methods used to replicate the original Whittington et al. results. In Section 3 we update the data and report our new results. Section 4 concludes. Details on the data reconstruction are relegated to the Appendix.

2 1913-1984: Data and Replication

Whittington et al. (1990) regressed the general fertility rate from 1913 to 1984 on a set of explanatory variables that they argued would affect fertility: male and asset income, unemployment, infant mortality, immigration, female wage, and binary variables for World War II and the availability of the birth control pill. The dependent variable is the general fertility rate, defined as the number of births per thousand women age 15-44. While some of the series were reported in the appendix of the published paper, others have been lost since the paper's publication. We reconstructed the missing series using the footnotes and references in Whittington et al.

Table 1 reports summary statistics of the reconstructed series and those reported in Whittington et al. (1990). It is clear that there are small differences between the two datasets, even for some series that were copied directly from the Whittington et al. appendix. In fact, of those series for which we obtained original data (general fertility rate, personal exemption, male and asset income, and female wage), only the personal exemption series exactly matches the reported moments. The other series are either different than the series used to report the summary statistics or some error was made in computing the mean and standard deviation.[4] The unemployment, infant mortality, and immigration series that we constructed quite accurately match the reported moments.

The primary variable of interest for Whittington et al. (1990) is the real tax value of the personal exemption for dependents. Today, the personal exemption is only one of several child subsidy provisions in the federal tax code accounting for about one-third to one-half of the total child subsidy. However, for the 1913-1984 period considered in Whittington et al., the personal exemption was the primary source of the implicit child subsidy, never accounting for less than 90 percent of the total child subsidy. The statutory value of the

[4]Brigitte Madrian generously gave us access to a 1991 letter she received from Leslie Whittington in which the full male and asset income series used in Whittington et al. (1990) is reported. According to this letter, the average female wage index values for 1972 and 1919 were typos. However, correcting these typos leads to greater discrepancies between both the reported moments and the replication results, so we use the series as reported in Whittington et al. in the replication analysis.

personal exemption for dependents changed only nine times between 1913 and 1984; however, its real tax value fluctuates substantially due to changes in marginal tax rates and the price index.

Following Whittington et al. (1990) we estimate the following reduced form equation for the period 1913 to 1984:

$$\begin{aligned}\text{General Fertility Rate}_t = \beta_0 &+ \beta_1 \,\text{Personal Exemption}_t + \beta_2 \,\text{Male and Asset Income}_t \\ &+ \beta_3 \,\text{Unemployment}_t + \beta_4 \,\text{Infant Mortality}_t + \beta_5 \,\text{Immigration}_t \\ &+ \beta_6 \,\text{Female Wage}_t + \beta_7 \,\text{Pill}_t + \beta_8 \,\text{WW2}_t + \beta_9 \,\text{Time Trend}_t + \epsilon_t.\end{aligned} \tag{1}$$

Whittington et al. (1990) estimate equation (1) by FGLS because of concerns about (first-order) serial correlation. Further details on the estimation approach are not included in the original paper. We report the original estimates of the primary specification as reported in Whittington et al. as Model (1) in Table 2. Next, we report the regular OLS estimates using the replicated data with Newey-West standard errors as Model (2) in Table 2. Finally, we report the results using Prais-Winsten FGLS (with a single iteration) and the replicated data as Model (3) in Table 2. Model (3) closely replicates the original Model (1) results.[5] The estimated coefficient on the tax value of the personal exemption is very close to the reported value in Whittington et al. In addition, the remaining coefficient estimates are also similar to Whittington et al.'s results.[6]

It is vital that Equation (1) is correctly specified, in the sense that it represents a long-run relationship between the primary variables of interest[7]. This issue is of paramount impor-

[5] At first glance, there appears to be a substantial discrepancy between Model (3) and Model (1), as measured by the R^2. In GLS estimation R^2 is not well defined, so it is unclear what definition was used by Whittington et al. Using the total sum of squares from the original OLS regression and the sum of squared residuals from Model (3) yields an R^2 of 0.919. While this technique does not give an accurate description of the fit of Model (3), it does represents a plausible method that may have been used to arrive at their reported R^2 of 0.916.

[6] We experimented with various estimation and iteration schemes and this provided the closest results. Slight differences in the data (including the series that were obtained from the paper itself) and potential differences in details of the estimation procedure likely explain deviations from the original results.

[7] We take as given that a single-equation analysis is appropriate. Discussion of the feasibility of this assumption is beyond the scope of this paper.

tance in the present application because these series are highly persistent. We conducted unit-root tests on the series in Equation (1) and found that the only series where we could reject the unit-root null hypothesis at a size of 10% was the unemployment rate and even this series exhibited a high degree of persistence.[8] We describe these results to emphasize the high degree of persistence in these series without taking a stand as to whether or not they have an exact unit root. If there does not exist a long-run relationship then a regression in levels, such as Equation (1), would be inappropriate and likely to produce spurious results.[9] In fact, Wooldridge (2009), a well-known undergraduate econometric textbook, uses Whittington et al. as an example of a spurious regression.

3 1913-2005: Updated Data and Results

3.1 Updated Data

We construct an updated dataset with 21 additional years (1985-2005) of data. In so doing, we examined each of the reconstructed (1913-1984) series to determine whether a better source was available. We found more up-to-date sources for several of the data series and use these rather than the reconstructed series in the updated data. Details regarding the data construction are provided in the Appendix.

We follow the Whittington et al. (1990) methodology in calculating the value of the personal exemption as described in the Appendix. We also construct a measure of the total value of child tax benefits in the federal income tax, as recent tax changes have increased the

[8]We conducted the unit-root tests of Harvey, Leybourne and Taylor (2009) and Carrion-i-Silvestre, Kim and Perron (2009) on the updated data. The tests of Harvey et al. (2009) are constructed to accommodate uncertainty over the nature of the initial condition or the presence of a linear time trend. The tests of Carrion-i-Silvestre et al. (2009) allow us to accommodate a structural break induced by the widespread availability of the birth-control pill. The autoregressive lag lengths were chosen by the variant of the modified Akaike information criterion (MAIC) described in Perron and Qu (2007).

[9]Recall that the so-called "spurious regression" problem is not confined to unit-root processes. Similar effects may arise even when the series are stationary (see, for example, Granger (2003), Granger, Hyung and Jeon (2001), Su (2008)). In addition, it should be noted that autocorrelation correction may ameliorate spurious regression concerns.

relative importance of other child tax benefits. In addition to the tax value of the personal exemption, the total child subsidy series also includes the value of the child tax credit (CTC) and the earned income tax credit (EITC).

The child tax credit acts as a child subsidy in a similar manner as the personal exemption, providing tax benefits to parents with children. However, the EITC is a tax credit that both increases in value with the number of children and affects the after-tax wage of recipients. Therefore, the EITC could also affect fertility through its effect on the opportunity cost of time. However, theory and empirical evidence both suggest that the effect of the EITC on the opportunity cost of time is minimal.[10] Because the labor supply effect is weak in aggregate and the child tax benefits from the EITC are large, the EITC acts more like a child subsidy than a wage subsidy and we think it is appropriate to include the EITC in the measure of the total child subsidy. However, we also report results excluding the EITC from the total child subsidy series.

The average value of these credits is calculated by dividing the total federal tax expenditure on these credits by the number of children in the United States in each year. The summary statistics for the extended data are reported in Table 3.

3.2 Updated Results: Original Specification

Table 4 summarizes our first set of results. In Column (1), we report our replication of Whittington et al. (1990)'s main specification with one change – the typos in Whittington et al.'s series are corrected (see the discussion in footnote 4 and the Appendix). These results are reported in constant 1967 dollars and are calculated using data series from the years 1913-1984. For Columns (2) and later, we make an additional change: the value of the child

[10]Theory suggests that the effect of the EITC on female labor supply is ambiguous except for single women not in the labor force where there is an unambiguous increase in the likelihood of labor force participation. The empirical literature finds that the EITC does increase the labor force participation of single women mothers (Meyer and Rosenbaum 2001). However, the EITC appears to reduce the labor force participation of married women (Eissa and Hoynes 2004). The reduction in labor force participation by married women to some extent offsets the increase in labor force participation by single women. In terms of hours of work, the empirical literature finds no significant effect of the EITC on aggregate female labor supply (Eissa and Hoynes 2006).

tax subsidy, male income, and female wage are converted to constant 2005 dollars. The effect of changing the base year can be seen clearly in the coefficient on the tax subsidy: whereas our replication of Whittington et al. in Column (1) showed that \$100 in tax benefits (in 1967 dollars) are associated with an increase in the general fertility rate of 9.9 births, the results in Column (2) show that the comparable change in the general fertility rate for \$100 in tax benefits (in 2005 dollars) is 1.7 births. This value provides a benchmark against which results from our subsequent analyses can be measured.

Column (3) begins the analysis using our extended data series for 1913-2005. The results in Column (3) show that using updated data sources and extending the data through 2005 reduces but does not substantively change the key coefficient estimated in Whittington et al. (1990). However, the results are sensitive to the definition of tax benefits. In Column (4) we repeat the analysis including the child tax credit in the tax subsidy series. While the coefficient on the child tax subsidy variable has the same sign as in Column (2), it is less than half the size and no longer significant. In Column (5) we show that a similar conclusion holds when the EITC is added to the tax subsidy series. The main results of Whittington et al. are weaker but still present in the extended time horizon, but are not robust to more general measures of child tax benefits.

The specifications presented in all five columns of Table 4 are not valid if there is no long-run relationship.[11] We perform a variety of cointegration tests, both residual-based and systems-based, to determine if there is evidence for a long-run relationship. The indicator for the availability of the birth-control pill acts as a structural break (with known timing), so we perform tests that allow for this.

On balance the tests are suggestive that no cointegrating relationship occurs. However, the results are at times sensitive to the exact specification. In the residual-based test of Westerlund and Edgerton (2006), which has a null hypothesis of no cointegration, we find no evidence to reject. However, using the residual-based test of Arai and Kurozumi (2007),

[11] We also consider more general, dynamic models in the Appendix.

which has a null hypothesis of cointegration, the test results are sensitive to the specification. For a lag length of less than three, the null hypothesis of cointegration is rejected for a test with nominal size of 5 percent. However, with a lag length of three, we fail to reject the null hypothesis for some specifications at this size. Meanwhile, the systems-based test of Saikkonen and Lütkepohl (2000), for most specifications, suggests either no cointegration or a cointegrating relationship only between those variables outside the variables of interest (i.e., excluding the general fertility rate and any subsidy variable). The test results and further discussion may be found in the Appendix.

We view the cointegration tests as largely suggestive that no cointegrating relationship exists. They do not completely rule out the claim that the original specification is appropriate. For example, it is well known that the performance of cointegration tests can be sensitive to the exact form of persistence in the variables. However, the key point is that even if we assume that the original specification is appropriate, which means the results from Table 4 are not spurious, Columns (4) and (5) and the results in the Appendix show that there is no statistically significant evidence of an effect of tax subsidies on the general fertility rate once the data are updated and we include more comprehensive measures of tax subsidies.

3.3 Short-Run Effects

If there is not a long-run relationship between child tax benefits and fertility then the Whittington et al. (1990) results are driven by the high persistence of the variables in the model rather than a meaningful relationship between these variables. In this section we will consider a unit-root specification as illustrative of a model with a high degree of persistence. The spurious regression problem can apply to any regression involving persistent variables, not only those with unit roots. However, the unit-root specification is convenient because it allows us to estimate the short-run relationship by simply differencing the variables, which may exist even if there is no long-run relationship.

To produce estimates of the short-run effect, we consider a regression similar to Equation (1), except using differenced variables. Table 5 summarizes the results from these regressions. Column (1) displays the results for differenced variables over the time period originally considered in Whittington et al. (1990) using the replication dataset converted to 2005 dollars. Surprisingly, the coefficient on the tax subsidy flips sign and decreases in magnitude. In Column (2), we run the same specification but utilize the extended data series. Columns (3) and (4) show the results for the other two child tax subsidy measures. Across all four models, the estimated short-run effect is negative.

As pointed out in Whittington et al. (1990), there are several reasons to believe that a fertility response from changes in covariates may occur with a lag. The birth of a child will lag the decision to have a child by at least nine months and frequently longer, and therefore the relevant variable in analyzing fertility in year t may be the covariate's value in year $t-1$. Covariates in time t may have little influence on fertility in year t.[12] Moreover, there is a reason to believe that the fertility response from changes in child tax benefits may be even more delayed. While a fertility response would not likely be observed until at least one year after a change to child tax benefits, it takes some time for taxpayers to learn that a tax change has taken place. Changes to the tax code are often made while the tax year is well underway. Individuals are not likely to learn about tax changes until they do their taxes (by April of the following year). While this may have an immediate effect on the decision to have a child, the actual birth is then realized with a delay. Therefore, while a single lag may be appropriate for the other regressors, the real value of child tax benefits should enter the fertility equation with at least two lags. That is, we posit that a tax policy change in year t may not affect the decision to have children until at least year $t+1$ and thus would not affect the total fertility rate until at least year $t+2$.

Thus, we explore whether the short-run effect changes when additional lags of the child tax subsidy are included. Table 6 reports the results from a regression of the differenced

[12]Immigration by women of childbearing age is an exception since some women may be pregnant at the time of immigration.

total fertility rate on varying number of lags of the child tax subsidy. The child tax subsidy variable specified here includes all three components of the child tax subsidy: the personal exemption, the child tax credit and the EITC. The current and lagged values of all other controls are included in the estimations although the estimated coefficients are not reported. Table 6 also reports the measure of the estimated total short-run effect of tax benefits, equal to the sum of the coefficients of all lagged child tax subsidy variables, with standard errors.

The results in Table 6 suggest that there is a statistically significant short-run effect of changes in child tax benefits on changes in fertility with two lags. However, the estimated *total* short-run effect across the four specifications are small and statistically insignificant, ranging from -0.004 to 0.010. The point estimates suggest that a $100 increase in the real value of child tax benefits in 2005 dollars is associated with an increase of approximately 0 to 1 birth. The magnitude of this total effect is much smaller than the magnitude of the Whittington et al. (1990) estimate of 1.7 births as calculated in Table 4, Column (2), and is statistically insignificant across all specifications.

These results provide weak evidence of an overall short-run response of fertility to tax benefits for this particular specification, under the assumption that the variables found to be highly persistent are in fact unit roots. Our estimates of the total effect are small and generally positive, but statistically insignificant.

4 Conclusion

The effect of tax policy on fertility rates is often neglected in the literature on federal tax policy, even though child tax benefits are large and have recently grown in importance. One of the most cited studies on this topic, Whittington et al. (1990), estimates a very large fertility rate response to the tax value of the dependent exemption. We have updated their analysis by incorporating 21 additional years of data along with more general measures of tax benefits for having children. We also revisited their original specification and do not find strong evidence that their original specification is appropriate. However, even if the original specification is appropriate we find in our updated analysis that the results of Whittington et al. are not robust to more general measures of child tax benefits.

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Figure 1: General Fertility Rate and Real Average Per Child Tax Subsidy

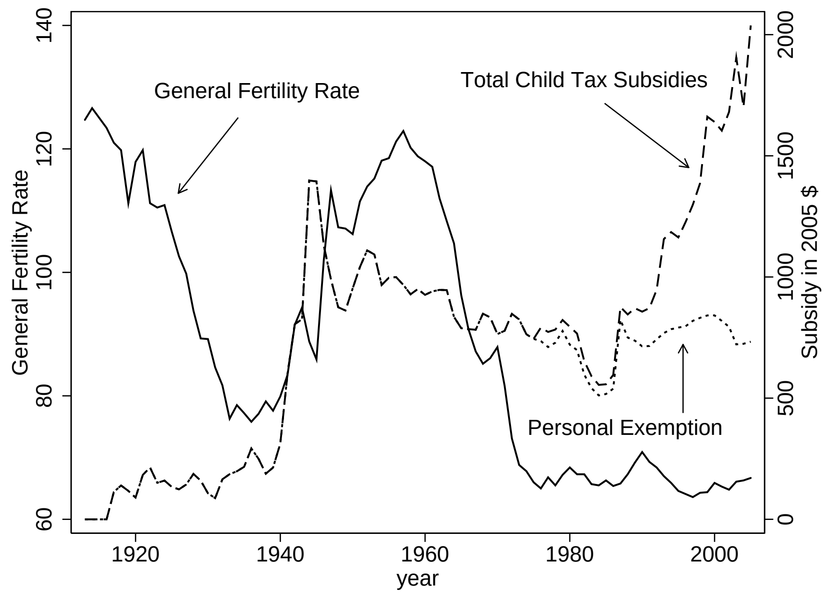

Table 1: Summary Statistics, 1913–1984

		Replicated Data		Whittington et al.	
Variable	Obs.	Mean	Std. Dev.	Mean	Std. Dev.
General Fertility Rate	72	95.6	19.81	95.5	19.64
Personal Exemption	72	100.4	65.88	100.4	65.88
Male and Asset Income	72	7,467.38	2,926.06	7,466.37	2,982.78
Unemployment	72	0.071	0.054	0.071	0.053
Infant Mortality	72	43.02	26.84	43.02	26.84
Immigration	72	0.003	0.0036	0.003	0.0035
Female Wage	72	1.35	0.585	1.22	0.532
Pill	72	0.306	0.464	0.305	0.464
WW II	72	0.069	0.256	0.069	0.256
Time Trend	72	36.5	20.93	36.5	20.92

Variables expressed in constant 1967 dollars.

Table 2: Comparison of Estimation Results

Variable	(1) Whittington et al.	(2) OLS	(3) Prais-Winsten
Personal Exemption	0.121	0.178	0.116
	(0.0446)**	(0.0977)	(0.0449)**
Male and Asset Income	-0.0004	0.0035	0.0007
	(0.0027)	(0.0031)	(0.0025)
Unemployment	-73.43	-68.12	-68.19
	(34.20)**	(25.818)*	(34.004)**
Infant Mortality	0.083	0.393	0.0351
	(0.255)	(0.321)	(0.251)
Immigration	774.24	964.13	760.71
	(311.31)**	(329.44)**	(304.98)**
Female Wage	5.647	15.427	5.629
	(15.686)	(5.286)**	(5.036)
Pill	-10.856	-25.383	-12.014
	(6.126)*	(11.961)*	(6.028)*
WW II	-17.223	-29.419	-17.863
	(4.989)**	(8.057)**	(4.854)**
Time Trend	-0.539	-0.843	-0.741
	(0.538)	(0.543)	(0.510)
Intercept	102.979	55.944	104.130
	(24.666)**	(25.831)*	(23.368)**
Observations	72	72	72
R^2	0.916	0.829	0.749

Standard errors in parentheses.

Variables expressed in constant 1967 dollars.

* significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level

Model (1) reports the regression results from the Whittington et al. paper.

Model (2) OLS estimates with Newey-West standard errors.

Model (3) Prais-Winsten FGLS estimation with a single iteration.

Table 3: Summary Statistics, 1913–2005

Variable	Obs.	Mean	Std. Dev.	Min	Max
General Fertility Rate	93	88.9	21.4	63.6	126.6
Personal Exemption	93	625.9	347.9	0	1398
Personal Exemption + CTC	93	661.1	384.8	0	1501
Personal Exemption + CTC + EITC	93	741.7	479.1	0	2038
Male & Asset Income	93	31,287	11,681	17,043	50,169
Unemployment	93	0.068	0.048	0.012	0.249
Infant Mortality	93	35.15	27.77	6.7	101
Immigration	93	0.00351	0.00257	0.00028	0.01505
Female Wage	93	7.59	3.34	2.14	12.93
Pill	93	0.462	0.501	0	1
WW II	93	0.054	0.227	0	1

Variables expressed in constant 2005 dollars.

Table 4: Comparison of Estimation Results in Levels

Variable	(1)	(2)	(3)	(4)	(5)
Personal Exemption	0.099	0.017	0.011		
	(0.044)**	(0.008)**	(0.006)*		
Personal Exemption + CTC				0.007	
				(0.005)	
Personal Exemption + CTC + EITC					0.005
					(0.004)
Male and Asset Income	-0.0003	-0.00005	-0.001	-0.001	-0.001
	(0.003)	(0.0004)	(0.0005)***	(0.0005)***	(0.0005)**
Unemployment	-68.019	-68.019	-86.711	-80.939	-84.576
	(33.684)**	(33.684)**	(25.079)***	(24.068)***	(24.254)***
Infant Mortality	-0.013	-0.013	0.057	-0.041	-0.086
	(0.247)	(0.247)	(0.157)	(0.141)	(0.139)
Immigration	698.917	698.917	1,079.458	989.809	979.596
	(299.761)**	(299.761)**	(297.470)***	(285.178)***	(288.937)***
Female Wage	16.545	2.829	4.137	3.847	4.257
	(14.129)	(2.416)	(2.349)*	(2.240)*	(2.240)*
Pill	-10.937	-10.937	-6.080	-5.332	-5.436
	(5.902)*	(5.902)*	(4.697)	(4.562)	(4.631)
WW II	-16.269	-16.269	-13.736	-11.689	-11.371
	(4.772)***	(4.772)***	(3.865)***	(3.653)***	(3.669)***
Time Trend	-0.969	-0.969	-0.527	-0.625	-0.718
	(0.590)	(0.590)	(0.348)	(0.346)*	(0.365)*
Constant	108.208	108.208	119.724	128.591	132.707
	(23.052)***	(23.052)***	(15.527)***	(13.919)***	(13.510)***
Observations	72	72	93	93	93
R^2	0.745	0.745	0.804	0.793	0.792

Standard errors in parentheses.

* significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level

Model (1): Replication of Whittington et al. (1990) with typos corrected (see text).

Model (2): Model (1) with variables expressed in constant 2005 dollars.

Model (3): Model (2) with extended data series for sample period 1913-2005.

Model (4): Model (3) with child tax benefits defined by personal exemption and child tax credit.

Model (5): Model (3) with child tax benefits defined by personal exemption, child tax credit, and EITC.

Table 5: Comparison of Estimation Results in First Differences

Variable	(1)	(2)	(3)	(4)
Personal Exemption	-0.014	-0.013		
	(0.006)**	(0.005)***		
Personal Exemption + CTC			-0.008	
			(0.004)*	
Personal Exemption + CTC + EITC				-0.007
				(0.004)*
Male and Asset Income	-0.001	-0.001	-0.001	-0.001
	(0.000)*	(0.000)	(0.000)	(0.000)
Unemployment	-20.985	-10.041	-8.391	-9.063
	(25.647)	(21.515)	(22.030)	(22.130)
Infant Mortality	-0.042	-0.072	-0.055	-0.053
	(0.178)	(0.157)	(0.159)	(0.159)
Immigration	68.878	198.098	191.007	195.459
	(182.199)	(195.021)	(200.214)	(201.176)
Female Wage	1.278	2.127	1.950	1.934
	(1.563)	(1.834)	(1.871)	(1.876)
Pill	-1.910	-0.688	-0.524	-0.447
	(1.113)*	(0.897)	(0.924)	(0.931)
WW II	5.138	4.703	3.629	3.483
	(2.441)**	(2.241)**	(2.229)	(2.227)
Constant	-0.618	-1.272	-1.177	-1.176
	(0.951)	(0.914)	(0.936)	(0.940)
Observations	71	92	92	92
R^2	0.203	0.145	0.108	0.104

Standard errors in parentheses.
Variables expressed in constant 2005 dollars.
* significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level

Model (1): Replication of Whittington et al. (1990) performed in first differences.
Model (2): Model (1) with extended data series for sample period 1913-2005.
Model (3): Model (2) with child tax benefits defined by personal exemption and child tax credit.
Model (4): Model (2) with child tax benefits defined by personal exemption, child tax credit, and EITC.

Table 6: Short Run Effects of Child Tax Benefits on Fertility, 1913–2005

Variable	(1)	(2)	(3)	(4)
Δ Total Child Tax Subsidy	-0.004	-0.003	-0.003	-0.004
	(0.004)	(0.004)	(0.004)	(0.004)
Δ Total Child Tax Subsidy$_{t-1}$	0.001	-0.0002	0.0002	0.0002
	(0.004)	(0.004)	(0.004)	(0.004)
Δ Total Child Tax Subsidy$_{t-2}$		0.012	0.012	0.011
		(0.004)***	(0.004)***	(0.004)***
Δ Total Child Tax Subsidy$_{t-3}$			0.002	0.002
			(0.004)	(0.004)
Δ Total Child Tax Subsidy$_{t-4}$				-0.003
				(0.004)
Measure of Total Effect	-0.004	0.008	0.010	0.007
	(0.007)	(0.008)	(0.010)	(0.011)
Observations	88	88	88	88
R^2	0.264	0.349	0.350	0.355

Standard errors in parentheses.
Variables expressed in constant 2005 dollars.
* significant at the 10% level; ** significant at the 5% level; *** significant at the 1% level

All specifications include current and lagged values of all independent variables on the right-hand side. Only current values of Pill and World War II included. All analysis was done with the updated data series. Total Child Tax Subsidy defined by personal exemption, child tax credit, and EITC. The column number signifies the number of lags of the child subsidy measure included in the model.