Happily ever after? Domestic violence, women´s empowerment, and stress after CCTs

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Abstract[1*]

This paper analyzes the causal impact of the payment of the Colombian Conditional Cash Transfer (CCT) program, *Familias en Acción*, on domestic violence. Using the arguably exogenous variation in time and space of the payments, we find a reduction of 6% in the municipalities' domestic violence rate. This reduction is not driven by female empowerment, changes in marital status, nor changes in labor participation of the beneficiary women. Results associated with months of payments that did not occur, negative surprise payments, suggest that unexpected changes in the family budget can increase domestic violence.

JEL codes: D03, J12, J16
Key words: Domestic Violence, Conditional Cash Transfers, Female Empowerment, Colombia

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1. Introduction

Conditional Cash Transfer programs (CCTs) are one of the most popular public policies, up to date, used by governments to reduce poverty and inequality in low and middle income countries. Although the main objective of CCTs is to increase the school attendance and health status of children, its specific design may induce unexpected externalities. In particular, providing cash to the women in the household based upon the recently controverted idea that they can take better decisions (e.g. Akresh et al., 2015; Benhassine et al., 2015) might modify the bargaining power within the household and result in changes on the incidence of domestic violence. These is a relevant unexpected externality given that domestic violence, highly prevalent in the developing world, represents a clear violation of human rights. Recent estimates of the World Health Organization suggest that almost 35% of women are subject to this crime in the world (WHO, 2016). Meanwhile, in Colombia 37% of women have reported being victims of domestic violence (Profamilia, 2011). Importantly, research has shown this crime embraces both direct and indirect costs for their usual victims, women and children, making it a significant public health problem.[2]

Theoretically, the direction of the impact CCTs might bring on this crime is ambiguous. In a first scenario, domestic violence can be seen as the men's specific manifestation to maintain the decision making power at home. In these models, which are based on power games within the household, the financial resources of women often influence their empowerment and therefore can reduce the violence level to which they are exposed to (Anderson and Eswaran, 2009; Aizer, 2007; Farmer and Thiefenthalter, 1997; and Tauchen et al., 2001).[3] In contrast, CCTs might increase levels of domestic violence through what is referred to as "men backlash". As analyzed by Eswaran and Malhotra (2011), Tauchen et al. (1991), Bloch and Rao (2002) and Bobonis et al. (2013) more resources in women's hands could increase violence as men perceive their power threatened. Finally, as argued by Duflo (2012), under any of those models

[2] Some studies on the causal impact of domestic violence on women's' health include Ackerson and Subranmian (2008), Coker et al. (2002), Jewkes et al. (2010) y Ellsberg et al. (2008). The impact on children has also been analyzed by Aizer (2011), Karamagi et al. (2007), Koeing y Stephenson (2006) y Koenen et al. (2003), among others.

[3] CCT programs could also increase women's empowerment through complementary channels including the improvement of information and social networks through the invitation to participate in education and health related group conferences as well as the increased interaction with healthcare professionals and among themselves (Chiapa et al., 2012).

if households internalize that CCT programs are temporary and the transfer of resources will eventually come to an end, the impact on both women's empowerment and domestic violence should be null.

Not surprisingly thus, the available empirical evidence regarding the impact of CCTs on domestic violence is mixed. Studies for Peru (Perova, 2010) and Kenya (Haushofer y Shapiro, 2013) find a causal reduction on the incidence of physical violence against women. Yet most of the studies find that the impact is heterogeneous. For example, in the case of México's CCT program Angelucci (2008) finds that the transfers increased physical domestic violence against women when men adhere to strong traditional gender roles; while Bobonis et al. (2013) find emotional violence and threats for women in the rural sector increase. Similarly, Hidrobo and Fernald (2013) and Rodríguez (2015) find for Ecuador and Colombia, respectively, that even though on average the incidence of domestic violence decreases after the introduction of CCT programs, for less educated women and those residing in poorer municipalities, violence against them actually increase.

This paper contributes to the existing debate by analyzing the impact that the exogenous cash payments of *Familias en Acción*, the Colombian CCT program, have brought to the municipalities' domestic violence rate in the country. To do so, we take advantage of three characteristics of the payment process in the program. First, even though the program defines a specific amount of monthly transfers for each beneficiary woman, the receipt of these payments are scheduled to be every two months. Second, these payments occur in the same month for all women in a given municipality. Third, there exist an important variation in both time and space on this month of payment across the country. Not every municipality receives the transfers in the same month and in many cases the payments do not follow the expected bimonthly schedule. We link this exogenous variation on the month of payment at the municipality level with the rate of domestic violence and find that in the months when payments are received the rate decreases in 6%. In practical terms, in each month of payment 136 less women suffer from domestic violence in Colombian municipalities.

The findings in this paper complement the existent literature on different dimensions. First, unlike most of the aforementioned papers, we provide causal evidence on four different possible transmission channels and our results shed light on previously understudied questions. The first and arguably the most direct channel through which the transfers could affect domestic violence is an increase in women's empowerment given the additional resources the program transfer to them. Even though all of the above mentioned studies cite this as the plausible mechanism

none actually provide evidence on this regard. In fact, the empirical evidence on the impact of CCTs on women's empowerment is almost nonexistent. Although a handful of studies have analyzed the effect on this outcome from programs such as microcredit (Garikipati, 2008; Pitt et al., 2006; Hashemi et al., 1996) and access to formal financial products (Ashraf et al., 2010) no study has yet analyzed the causal impact CCTs may bring.[4] Using quasi-experimental information from the first impact evaluation of the program we do not find evidence that supports this hypothesis. In fact, the probability that men are the sole decision makers in the children's health and education decisions, precisely the outcomes that CCTs seek to improve, increases after the program implementation.

Results using the same source of information also suggest that the decrease in the domestic violence rate is not driven either by changes in women's labor force participation or marital status, two questions that had not been previously analyzed either. Rather, using again information of the payments at the municipal level we find that the impact is probably driven by a decrease in the shortage and stress levels of the families that receive the transfers, a channel that was also found by Hidrobo et al. (2016) and Rodríguez (2015) and that is closely related to the results found by Camacho et al (2014).

The reminder of the paper is organized as follows. Section two describes the program, *Familias en Acción,* and the payment scheme of beneficiary mothers. Section three describes the data set used, while section four details our identification strategies. Section five presents our main results, and section six concludes.

2. *Familias en Acción* and its payment processes to the beneficiary mothers.

Familias en Acción (FeA), the Colombian CCT program, started in the year 2001 inspired on the Mexican CCT program PROGRESA. FeA has three main objectives: i) reduce poverty and inequality; ii) promote school assistance for children between 7 and 18-year-old, and; iii) strengthen health and nutrition care of the children under the age of 7. This program has expanded throughout the territory in the last 15 years, and today it serves nearly 2.6 million families in 1,102 municipalities. In this paper we study the first expansion of the program which

[4] The only exception is Adato et al. (2000) who analyze how the original Mexican CCT program influenced who in the beneficiary households took important decisions. However, given that no baseline information is available, the study cannot analyze changes in empowerment before and after the implementation of the program but only changes in patterns after the program was already in place with respect to the comparison group.

took place between 2001 and 2006 and targeted 845 municipalities with particular characteristics. These municipalities were mainly rural, had less than 100,000 inhabitants, could not be departmental capitals, had to have a banking institution to provide the payments, as well as enough health and education facilities to respond to the demand of such services.

The program gives two types of incentives. The first one is a health incentive that is provided per household with children under the age of 7. To receive the payment, the families must take all their children, under the age of 7, to growth and development checkups and they must follow all the protocols imposed by the Health and Social Protection Ministry. The second incentive is an educational incentive that is delivered per child. In order to receive the transfer, children should attend a minimum of 80% of the school days. The amount of money delivered in each of these incentives is important relative to the family's income, since these are the poorest families in Colombia. According to the 2015 data, the monthly health subsidy is between $63,500 COP (US$21) and $74,100 COP (US$24)[5]. The amount depends on the type of municipality where the family lives. The monthly educational subsidies vary from $21,175 COP (US$7) for children who are in first grade, to $58,225COP (US$19) for children who are in eleventh grade. For example, a family who does not live in a departmental capital and has two kids who are 6 and 12 years old would receive a monthly subsidy of $153,000COP (US$49.85). This amount represents one fourth of Colombia's monthly minimum legal wage, which is the median salary in the country.

The payment process of these transferences is a key factor that determines the identification strategy used in the current paper and thus a description of the process is necessary.[6] Although the amounts reported earlier are the transfers families are entitled to receive per month, the program states that the payment should be delivered to the families every two months. In order to minimize administrative costs caused by the delivery of the transfers, the payments are delivered simultaneously to all beneficiary mothers from a given municipality during three or four consecutive days.

Between 2001 and 2008, the payments were delivered through a non-banking operation called "*giro bancario*". These were direct cash transferences, delivered to each mother with the amount of the subsidy to a banking office where the mother could go and collect it in cash. In municipalities without a banking entity, there were two alternatives. The first one was an "extended box", in which the government and the bank in charge of the payment opened a

[5] These calculations are based on the TRM in May 31, 2016, which is that 1 dollar is equal to $3,069.17 COP

[6] For a more detailed account of the payment process and the changes in time please refer to Rodríguez (2015).

temporal office in the municipality to deliver the payments. The other alternative was to deliver the payment in the banking office of a nearby municipality and the mothers would receive the payment there. This alternative was implemented when the transportation costs allowed it. According to *Acción Social (*2010), 89% of the payments in these first years were made through the municipality's own bank offices. These payment alternative methods changed radically in 2009, when there was a massive financial inclusion process of the beneficiary mothers. Today all transfers of the program are directly deposited to the simplified savings bank accounts activated for the mothers in the program.

Despite the changes in the mechanisms through which the transfers are delivered to the beneficiary mothers, two important characteristics have remained constant since the onset of the program. First, information about the payment dates have always been through three mechanisms: i) radio and television announcements, ii) a report, directed to each municipality's mayor, that describes the incentives delivery and that announces the payment date, iii) announcements delivered in the Department of Social Prosperity (DPS by its acronym in Spanish) website. Second, transfer payments have always been supposed to be delivered every two months to each beneficiary mother. Despite this rule, the geographic and time distribution of these payments is not predictable; on the contrary, the payments are very random, which is key to the validity of our identification strategy, discussed in detail in the next sections.

Cash transfers are not the only mechanism through which FeA could impact both domestic violence and women's empowerment within the household. Networking, exposure to peers, and education by professionals could also change women's perceptions and social norms. Beneficiary women attend health check-ups and meetings with the community leaders, where they have social interactions with doctors, community leaders, and other beneficiary mothers and thus these interactions can change their perspective and tolerance towards several outcomes one of which could be domestic violence (Chiapa et al., 2012).

3. Data

We use four sources of data to answer our question of interest. The first one is the data from the National Institute of Legal Medicine and Forensic Science (INMLCF) of Colombia, the office in charge of delivering the forensic services that supports the country's justice administration. This Institute has 8 regional administrations, 25 sectionals, 103 basic units and 5 mobile units, with representation in 36 departments and districts in the country. The Institute's

doctors carry on the legal-medical activities that provide the system's information. In places where the Institute has no official doctors, the municipality's doctors must do the forensic investigation, under the Institute supervision. The violence indicators that we use in this paper include monthly municipal level measures on domestic violence, interpersonal violence, homicides, suicides, sexual violence, and traffic accidents from 2007 until 2010. The limitation of this data is the common one to any source of information on criminal activity; the data corresponds to those events that are reported in the health and justice systems, so it does not include all incidents. We discuss in detail the implications of this limitation in the results and conclusions sections.

The second source of information comes from the System of Information of *Familias en Acción* (SIFA) that reports the exact payment dates and amounts of the transfers delivered to beneficiary women in each municipality. We use aggregated data at the municipality level from May 2007 to December 2010 and create a dummy variable equal to one if the FeA payment was delivered in municipality *i,* in month *m,* in year *t.* Table 1 displays the proportion of municipalities in our sample that received monthly payment during the period of study, that is the municipalities that started to be served in the first national expansion of the program. There are two facts that are evident from this table: first, the payment date does not follow strictly the two months' payment rule, and second, the proportion of municipalities receiving payment by month varies importantly over time. For example, in 2007 and 2010 almost all of the municipalities in our sample received the FeA payment in December, while in this same month in 2008 only 19.9% of the municipalities received the payment, and finally, in 2009 this proportion was 2.7%. Similarly, while in June of 2007 and 2008 almost none of the municipalities received the payment, in June of 2009 and 2010 all of the municipalities received the payment. Also, between the same year's months, there is a wide variation. For example, although there are months in which all the municipalities in the sample receive the payments, there are also other months in which the proportion of municipalities that receive the payment varies between 30, 15 or even 1 percent.

Given this variation, it is not surprising that within the municipalities, the payment date does not follow the two months payment rule. Table 2 shows the payment dates within a randomly chosen municipality in the sample. This Table shows that the FeA payments in the municipality of Honda, are not always delivered every two months, sometimes, the payments are delivered

three months in a row, and other times the payments are not delivered in two consecutive months. Out of the 44 months where we observe data for this municipality, 14 months that appear in bold break the payments rule.

Table 1. Monthly proportion of Municipalities that received the FeA payments, 2007-2010

	2007	2008	2009	2010
January	N.D	1.3%	100.0%	22.3%
February	N.D	100.0%	4.6%	100.0%
March	N.D	13.1%	3.6%	2.0%
April	N.D	35.0%	99.9%	99.9%
May	55.5%	100.0%	92.0%	3.7%
June	0.0%	0.1%	100.0%	100.0%
July	98.6%	100.0%	7.2%	100.0%
August	34.6%	0.2%	100.0%	2.2%
September	99.2%	100.0%	2.1%	100.0%
October	14.5%	4.5%	100.0%	1.5%
November	83.6%	100.0%	100.0%	100.0%
December	99.3%	19.9%	2.7%	100.0%
Total	40.4%	47.8%	59.3%	60.9%

Source:SIFA

Table 2. Months in which FeA payments were or were not delivered in Honda, Tolima, 2007-2010

Year 2007	Payment	Year 2008	Payment	Year 2009	Payment	Year 2010	Payment
January	N.D.	January	0	January	1	January	0
February	N.D.	February	1	February	0	February	1
March	N.D.	March	**1**	March	**0**	March	0
April	N.D.	April	0	April	1	April	1
May	1	May	1	May	**1**	May	0
June	0	June	0	June	**1**	June	1
July	1	July	1	July	0	July	**1**
August	**1**	August	0	August	1	August	0
September	**1**	September	1	September	0	September	1
October	**1**	October	0	October	1	October	0
November	**1**	November	1	November	**1**	November	1
December	**1**	December	0	December	0	December	**1**

Soruce: SIFA

The third source of information, which allows us to analyze the impact of FeA on some of the plausible channels through which the program could impact domestic violence, comes from the FeA quasi-experimental panel data survey. Even though the program was not randomly assigned, the government made an effort to design and collect the necessary information to evaluate its impact. To do so, 57 and 65 stratified treatment and control municipalities were chosen in order to be as similar as possible. This is a representative sample of municipalities that entered the program in its first phase in 2001-2002. Households in the sample were randomly chosen to answer a standard multi-topic longitudinal household survey. The survey includes 6 modules with questions on demographics, household structure, education, health, consumption, employment, anthropometrics, housing conditions, assets, and access to education and health facilities. In this paper we use information from the baseline and the first follow up surveys. The baseline data was collected between June and October 2002, and the first follow-up survey revisited the same households between July and December 2003. Given the important efforts made in data collection, attrition between both rounds of surveys only accounts for nearly 6%.[7]

The fourth and final source of information comes from the CEDE panel. This is a panel at the municipality level that includes several variables that will be used as controls in our regressions. We will use data on: land inequality, per capita taxes collected by the municipality, per capita public spending in education and justice, average score on the standardized test at the end of high school (Saber 11), the rate of offensive actions per 10,000 people, the rate of interpersonal violence per 10,000 people and homicides rate per 10,000 people.

Table 3 shows some descriptive statistics for our main variables of interest from all the four data sources. In Panel A, we present information for the 838 municipalities included in our study. According to the data in 56% of the months of the year there is a FeA payment. This domestic violence rate is divided into intimate partner violence rate, violence against the minors, towards other people, and towards the elders. Intimate partner violence rate has the highest mean which is 1.21 per 10,000 people, while violence against the elders has a mean of only

[7] For greater detail on the program, its implementation, the data collection process, and the survey please refer to Attanasio et al (2005) and Attanasio, Battistin, and Mesnard (2012).

0.03 per 10,000 people. In this table, we also include the means of other types of violence such as interpersonal rate whose mean is 1.13 per 10,000 people, the homicides rate has a mean of 0.29 per 10,000 people, and the rate of offensive actions per 10,000 people has a mean of 2.91. Finally, we include some of the municipalities' socioeconomic characteristics. On average, the municipalities collected 833,163 pesos of per capita taxes; while the mean per capita investment in education is 214.37 thousands of pesos, far greater than the mean per capita investment in justice, which is only 22.16 thousands of pesos. Municipalities' land Gini is 0.69 reflecting a high inequality in land ownership; while as a proxy for poverty, on average, 32% of the municipalities' families are FeA beneficiary families.

Panel B presents summary statistics from the FeA panel survey. To test the empowerment channel, we use the survey's fourth module, answered by the female head of the household or spouse which includes questions regarding the household decision making process that will be described in detail below. Specifically, the survey asks each woman five questions regarding decision making: i) who decides when to take a child to the doctor, if sick; ii) who decides if the child goes to school; iii) who decides whether or not to buy children's clothes and shoes; iv) who decides how much is spent on food, and finally; v) who decides if certain extra spending is done (e.g. fix something in the household or buy appliances). For each of these questions, there were four possible answers: father decides alone, mother decides alone, joint decision between mother and father and finally other members in the household decide. The table shows the percentage of the fathers that decide on such issues limiting the sample to those that women who have information from baseline and from the first follow up. As can be observed, 7% of the fathers decide doctor issues, 9.8% of them decide school issues, 23% take decisions regarding the children's clothes, 39% of the fathers decide on how to spend resources in food, and finally, 36% decide on extra spending subjects.

To test the marital and working status channel we use traditional questions included in these surveys on these outcomes. At baseline, 30.47% of women were married, 49.97% lived with their couple, 4% were single, 5.14% were widows and 10.43% were divorced or separated. Regarding their labor participation, 41.64% of women report to have a job at baseline. Panel B also reveals that the average, 54% of the women in the sample are FeA beneficiaries, they are 38 years old at baseline and have on average 3.36 years of education, compared to men who have 2.98 years. Finally, the number of children indicates that at baseline and in average the families have 1 child under 7 years old, 1.26 children between 7 and 12 years old, and they have on average less than 1 child between 13 and 17 years old.

Table 3. Descriptive statistics. Colombia May 2007- December 2010.

	Number of Observations	Mean	SD	Min	Max
Panel A: Municipality level data					
Month of the FeA payment	35,872	0.568	0.50	0	1
Domestic Violence rate per 10.000 people	35,872	1.1	2.12	0	69.3
Rate of violence towards the minors per 10,000 people	21,840	0.43	2.03	0	92.45
Rate of intimate partner violence per 10,000 people	21,840	1.22	6.22	0	249.1
Rate of violence towards the elders per 10,000 people	21,840	0.03	0.27	0	12.96
Rate of violence towards other family members per 10,000 people	21,840	0.41	1.82	0	82.18
Rate of interpersonal violence per 10,000 people	35,872	1.11	2.77	0	191.03
Homicide rate per 10.000 people	35,872	0.28	3.38	0	333.41
Per capita taxes in thousands of pesos (IPC2008)	35,872	815,527	920,852	14,467.09	15,700,000
Land Gini	35,872	0.70	0.1	0	0.98
Education Investment per capita	35,872	221.79	4,010.77	0	228,984.6
Justice Investment per capita	35,872	22.59	361.7	0	15,572.83
Average score on Saber 11 test	35,872	-0.14	0.19	-0.69	0.98
Proportion of beneficiary families	35,872	32.17	14.91	0.81	79.9
Rate of offensive actions per 10,000 people	35,872	2.96	6.44	0	127.99
Panel B: FeA Panel Survey					
Father's decision regarding					
Doctor check ups	4,042	0.08	0.27	0	1
School attendance	4,042	0.10	0.30	0	1
Clothes purchases	4,042	0.26	0.44	0	1
Food purchases	4,042	0.40	0.49	0	1
Extra spending allocations	4,042	0.38	0.49	0	1
Mother single or divorced	5,316	0.14	0.34	0	1
Mother works	4,042	0.35	0.48	0	1
Received FeA	4,042	0.54	0.49	0	1
Subsidy relative to woman income	3,757	0.44	0.47	0	1
Mother´s age	4,042	37.96	9.96	16	85
Mother's participation in political/social groups	4,042	0.32	0.47	0	1
Father's age	4,042	42.85	11.48	14	94
Father's years of education	4,042	3.13	2.89	0	19
Mother's years of education	4,042	3.49	2.94	0	19
Father works	4,042	0.92	0.26	0	1

Difference in education between mother and father	4,042	1.28	1.35	0.08	12
Children under 7 years old	4,042	1.31	1.04	0	6
Children between 7 and 12 years old	4,042	1.31	1.04	0	6
Children between 13 and 17 years old	4,042	0.81	0.90	0	5

Source: SIFA, Forensics, Panel CEDE, and FeA Panel Survey. Notes: In Panel A, the unit of observation is municipality per month. Notice that the observations of the domestic violence rate are greater than the observations of the different types of domestic violence, according to the victim. This occurs because in many cases, the Legal Medicine Institute does not have a record of the type of victim, it only includes if there is a domestic violence event. All the variables shown in Panel B represent the data at baseline. The doctor, school, clothes, food and extra spending decisions displayed in this table are the ones taken by the father.

4. Empirical strategy

a. FeA transfer payments and their impact on domestic violence

In order to identify the causal impact of FeA's payments on the domestic violence rate, we use the exogenous variation of the program's transfer payments at the municipality level. As explained before, according to the program design, the families should receive the transfer payment every two months on an established date. Yet, data shows that the payment is not delivered on the same month in all the country's municipalities. Moreover, within each municipality the payment is not delivered every two months either. There is sufficient exogenous variation such that the beneficiary families do not know exactly when the money is delivered. Moreover, it is hard to imagine how the payment dates, established by the central government, could depend on the domestic violence levels in each municipality.

Thus, we take into account the payment dates exogeneity and construct an unbalanced panel data at the municipality level that will identify the transfers' impact on the domestic violence for those municipalities that entered the program in its first expansion phase. The main empirical specification can be summarized as follows:

$$VioInt_{i,m,t} = \beta_0 + \beta_1 P_{i,m,t} + X_{i,m,t}\gamma + \vartheta_i + \sigma_m + \tau_t + \theta_{i,m,t} + \epsilon_{i,m,t} \quad (1)$$

Where $VioInt_{i,m,t}$ measures the domestic violence rate per 10,000 people in the municipality *i*, in the month *m,* and in the year *t.* $P_{i,m,t}$ Is a dummy variable that is equal to one if a payment was delivered in the municipality *i*, in the month *m* and in the year *t;* and takes the value of zero otherwise. The $X_{i,m,t}$ is a matrix that includes municipalities' characteristics such as the poverty

level, the inequality, the public spending in education and health, the taxes collected by the municipality and the quality of the education proxied by the average of the standardized test score at the municipality. These characteristics control time varying information that is associated with the domestic. The regression also includes three fixed effects: the first one is ϑ_i which captures the municipalities' fixed effect, and can thus control for differences that are time invariant in each municipality and that may influence domestic violence. The second fixed effect is σ_m, which represents the month fixed effect that controls for any seasonality that might be present in the domestic violence events. The third fixed effect is τ_t which represents year fixed effects. To further control for variations within the same municipality, we include in the regression $\theta_{i,m,t}$ which is a time trend at the municipality level. Finally, $\epsilon_{i,m,t}$ represent the error term.

Under this specification, β_1 is our coefficient of interest that estimates the average causal impact that the FeA payments have on municipal domestic violence rate. This identification strategy, similar to the one used by Camacho et al (2014) and Rodríguez (2015), relies on two assumptions: continuity and exogeneity. The first assumption requires the payment and non-payment dates to be similar and comparable. As explained in the last section, the payment months within a municipality vary from one year to another across the country and within each municipality. Additionally, with the inclusion of the month fixed effects, we will compare violence rates within the same month with and without payment. The second assumption, exogeneity, is to our judgment a credible one given that is highly unlikely that the payment dates established by the central government would depend on the domestic violence rate in each municipality. These assumptions guarantee that β_1 measures the causal and unbiased impact of the receipt of the FeA transfers on the municipal domestic violence rates. Moreover, it can easily be adapted to estimate heterogeneous impacts and robustness checks with other measures of violence incidence at the municipality.

b. Transmission channels

This paper analyzes three possible transmission channels through which FeA payments may have impacted domestic violence rates in Colombia's municipalities. The first one is the empowerment channel were we use questions about who in the household takes certain decisions regarding children's schooling, children's health, household expenditure on children's

clothes and shoes, household consumption of food and extra spending decisions. The answers to these questions, although subjective, are commonly used for these purposes by several studies in the literature such as Adato et al. (2000), Pitt et al. (2006), Garikipati (2008), Anderson and Eswaran (2009) and Ashraf et al. (2010) among others. We construct a dummy variable equal to one if the father is the sole responsible to take such decision and zero otherwise. Given the structure of the data and the question we want to answer is feasible to estimate a Seemingly Unrelated Regressions (SUR) model, in which we control for women's fixed effects. In this SUR model we are able to estimate six equations simultaneously, one for each decision, with a gain in efficiency [8].

$$Fdecides_{ij} = \beta_0 + \beta_1 FeA_{i,j} + \beta_2 Post_{i,j} + \beta_3 Post_{i,j} * FeA_{i,j} + \sigma Pscore_{i,j} + \gamma X_{i,j} + \vartheta_i + \epsilon_{i,j} \quad (2)$$

Due to the quasi-experimental structure of the data one could estimate the impact of the program using a simple difference in difference model, conditional on household and municipality characteristics, where the treatment variable is a dummy equal to one for beneficiary mothers and zero otherwise. Nonetheless, we address differences in the quasi-experimental design by going three steps forward. First, the regression is estimated with a Propensity Score, including similar controls as used previously by Attanasio et al. (2005), and the sample is restricted to women who fall within the common support[9]. Second, in a similar way to Aizer (2011), we also check whether the relative amount of the transfer with respect to women's income is the driving mechanism.[10]

Other papers have previously established that working increases women bargaining power and could therefore reduce violence against them (Aizer, 2007; Anderson and Eswaran, 2009). Evidently, not having a partner would also reduce the violence women are exposed to.

[8] We also ran regressions under multinomial Logit, pooled OLS with women fixed effect, and a SUR model with the dependent variable equal to one if the mother decides and zero otherwise. Our results are robust to the ones presented in this paper and are available upon request.

[9] The controls we use in the regressions are the head of the household's characteristics and not the father's characteristics, such as his age, or education.

[10] Although not shown, all empirical exercises regarding women's empowerment were also carried out using a multinomial model specification instead. We also took into account that, as detailed by Attanasio et al. (2005), FeA started before the baseline survey was completed in almost half of the treatment municipalities. Robustness checks confirm that using women residing only in municipalities without payment before the baseline survey, and their corresponding controls, also give very similar results. All these are available upon request.

To test the work and marital status channels, we evaluate if the program increases the women's probability of working or being married using a probit model as described below[11]:

$$works_{i,j} = \beta_0 + \beta_1 FeA_{i,j} + \beta_2 Post_{i,j} + \beta_3 Post_{i,j} * FeA_{i,j} + \sigma Pscore_{i,j} + \gamma X_{i,j} + \mathfrak{a}_i + \epsilon_{i,j} \quad (3)$$

Finally, CCT programs may affect domestic violence through the shortage and stress channel. To test this fourth channel, we first evaluate if the transfers' effect depends on the expectation of the payment. The idea is to check if the effect varies when the family expects a payment but does not receive it, and also if it varies when the family does not expect the payment but they in fact do. The available information allows us to evaluate if these moments of euphoria or frustration are key factors to explain the effect on the domestic violence as for example Card and Dahl (2011) evaluate. Second, we analyze the heterogeneous impact of the payment according to the municipality's poverty level. We take the proportion of beneficiary families in each municipality as a proxy for such levels and include it together with its interaction with the payment month.

5. Results

a. Domestic Violence

In Table 4 we show the estimations of equation (1) where the dependent variable is the domestic violence rate per 10,000 people in each municipality in the first three columns and the measure of intimate partner, minor and other victims' violence rate within the household in the last three columns respectively. The first model, the simplest one, only includes the dummy variable of the month when the CCT payment is delivered in each municipality. As observed, the coefficient is negative and highly significant implying that precisely those months in which the exogenous payment is delivered, the municipality's domestic violence rate decreases. The second and third columns include fixed effects and municipal controls to reduce endogeneity problems related with omitted variables. It is important to notice that once we control for the municipality's fixed effects and time fixed effects, the coefficient in the last two models is almost identical. This reflects that the month in which the payment is delivered is in fact exogenous

[11] The results do not change when the model is a Linear Probability Model (LPM) or a Logit model.

from such characteristics, and this in turn indicates that we do not have an omitted variable problem that could invalidate our results. The controls we include are: the taxes per capita collected by the municipality, the land inequality, the amount of public spending on education and justice, the average score on the standardized test (Saber 11), the rate of offensive actions, and the homicide rate per every 10,000 people. When the measure of domestic violence is disaggregated according to who is the reported victim, results from the last three columns show that all the effect comes from a reduction in intimate partner violence. Interestingly, the transfers do not have any impact on violence against minors, old age individuals or other members of the household.

Table 4. Familias en Acción payment's average impact on the domestic violence rate.

	(1)	(2)	(3)	(4)	(5)	(6)
	Domestic violence rate	Domestic violence rate	Domestic violence rate	Intimate Partner violence rate	Violence against the minors rate	Violence against other family members rate
FeA Payment Month	-0.0843***	-0.0647***	-0.0644***	-0.0578***	0.00636	-0.0201
	(0.0203)	(0.0201)	(0.0201)	(0.0202)	(0.0193)	(0.0282)
Fixed Effects						
Municipalities	No	Yes	Yes	Yes	Yes	Yes
Year	No	Yes	Yes	Yes	Yes	Yes
Month	No	Yes	Yes	Yes	Yes	Yes
Time tendency	No	Yes	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	Yes	Yes
Observations	35,872	35,872	35,872	21,840	21,840	21,840
R-squared	0.000	0.002	0.006	0.004	0.004	0.003
Number of municipalities		865	865	838	838	838

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

The results from the third and fourth model in Table 4 indicate that in the months in which the municipalities receive the payment there is a reduction in the domestic violence and

intimate partner violence rate of 0.06 and 0.058 events per 10,000 people respectively[12]. The domestic violence average in the years studied in these municipalities is 1.1 events per 10,000 people, thus in the payment months the program reduces domestic violence in nearly 5.45% from average value. In turn, given that intimate partner average violence rate is 1.21, the receipt of transfer payments decreases it by 4.78%

This is a significant impact for two reasons. First, the domestic violence reduction is not a specific objective of FeA, so this unexpected result is encouraging and has positive implications on the beneficiary families' welfare. Second, our analysis measures the program's average impact on domestic violence measures in each municipality and it does not only measure the effect on the beneficiary families. Therefore, it is very likely that the effect on the beneficiary women is higher than the one we report in this work. Moreover, this impact is consistent with the results found in the studies analyzed in the introduction.

Our analysis is different from the literature since we use the domestic violence reports from the administrative records of an official institution and not those reported in a households' survey. Moreover, we measure the CCT payment impact on serious violence events that need medical attention or at least that are severe enough to be reported. On the contrary, less intense physical violence events, psychological or verbal threats are probably not reported to the Legal Medicine Institute, and thus are not considered in our estimations. Results should be analyzed taken this into account, especially given the recent debate in the literature that argues CCT's may have different impact depending on the type of violence analyzed. For instance, Bobonis et al. (2013) find that while severe physical violence declines with the payment reception, the emotional violence against women rises. On the contrary, Perova (2010) and Hidrobo and Fernald (2013) find that both physical and emotional violence diminishes.

b. Channels through which FeA reduces domestic violence

As we argued earlier, the possible channels through which FeA reduces domestic violence and we are able to test include women's empowerment, higher divorce rate, labor force participation, contact with FeA leaders and/or through a decrease in stress levels associated with the shortage of resources these families may be subject to. We present the main results obtained regarding each particular channel.

[12] The results remain unchanged when we run the first three columns with the same sample as the regressions from columns 4 to 6.

Women's empowerment

Table 5 presents the main results of the impact that FeA has on women's empowerment, measured by who is the decision maker at home and restricting the sample to those women with an intimate partner. We run a SUR model including individual mother fixed effects in the common support and controlling for their probability of being benefited by the program as suggested by Attanassio et al. (2005). We present results using two independent variables of interest: the dummy variable indicating if the woman is a FeA beneficiary and a continuous variable indicating the proportion of their monthly income that FeA transfers represent. The table provides evidence that do not support the traditional hypothesis under which the extra income provided by the program's transfers gives additional bargaining power to the recipient, in this case to the mother, within the household. None of the two measures support the idea that FeA has empowered beneficiary women in any of the five dimensions evaluated. On the contrary, for schooling and doctor visits decisions, after the implementation of the program, the fathers from beneficiary households have a significantly higher probability of being the sole decision taker on those regards. The coefficients from the models using the dummy of FeA participation as main dependent variable imply an increase of 2 and 4 percentage points the probability that fathers' are the sole decision makers regarding doctor and schooling decisions of the children, decreasing thus women's empowerment at home. This is an important effect and accounts to an increase of 23.53% percent and 43% percent in the rate of doctor and schooling decisions taken by the father respectively.

Table 5. Women Empowerment

	(1) doctor decision	(2) school decision	(3) cloth decision	(4) food decision	(5) extra spending decision
Effects of receiving FeA					
Received FeA*First follow-up	0.02**	0.043***	-0.006	-0.003	0.012
	[0.0099]	[0.0127]	[0.017]	[0.0184]	[0.0184]
Controls	Yes	Yes	Yes	Yes	Yes
Observations	8,152	8,152	8,152	8,152	8,152
Effects of subsidy relative to woman income					
CCT relative to woman income*First follow-up	0.021*	0.038***	-0.012	-0.001	0.014

	[0.011]	[0.0127]	[0.0184]	[0.0198]	[0.0212]
Controls	Yes	Yes	Yes	Yes	Yes
Observations	7,714	7,714	7,714	7,714	7,714

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.
Note: Controls include woman participation in groups, women's age, head of household's age, women and head of household's years of education, woman works, head of households works, educational gap between woman and head of household, if the family has children between 0 and 6 years old, between 7 and 12 years old and children between 13 and 17 years old.

Labor participation and marital status

A second possible transmission channel is that the program, could have changed women's decisions regarding labor participation; which in turn could change bargaining power within the household and a change the incidence of domestic violence. To test this hypothesis, we estimate a probit model were the dependent variable is a dummy equal to one if the woman works and zero otherwise. As in the previous case, we used only the sample of women with an intimate partner, in the common support and we controlled for their probability of being benefited by the program as suggested by Attanassio et al (2005). Our results, presented in the first column of Table 6, show that receiving the payment has not affected women's probability of working. Although it thus not serve as a possible transmission channel it's an interesting result given that few studies have analyzed the impact of CCTs on women's labor participation and none of them, to our knowledge, has analyzed this for Colombia. For instance, Fernandez and Saldarriaga (2014) find that in Peru, the cash transfer diminishes labor participation by 6-10 hours, while Alzua et al (2013) find almost nonexistent reductions in labor force participation for Nicaragua, Mexico and Honduras.

Table 6. Marital Status and Labor participation

	(1) Woman works	(2) Divorced or Single
Received FeA*First Follow-up	-0.006	-0.001425
	[0.021]	[0.013]
Controls	Yes	Yes

Observations	8,152	10,665

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1
Controls include: woman participation in groups, woman's age and years of education, and if the family has children between 0 and 6 years old, between 7 and 12 years old and children between 13 and 17 years old. In the Divorced/single regression we include if the woman works as a control. In the woman works regression, we include head of household's age and years of education, if head of household works and educational gap between woman and head of household.

Alternatively, the resources given by the program could have allowed otherwise dependent women to change their marital status and leave violent partners. We test this hypothesis by estimating a probit model where the dependent variable is equal to one if the woman is married or cohabiting and zero otherwise. Unlike previous models though, we use information from all women in the survey, and not only those with a partner in both periods, to understand if FeA changed such status. As it is evident from the second column on Table 6, the coefficient of interest is not significant suggesting that the program has not changed marital and cohabitation decisions of beneficiary women either. This in turn suggest that using only women with a partner in both the baseline and follow-up survey does not introduce bias correlated with the program.

Resource shortages and stress reduction

The last channel tested in this paper which could explain the reduction in domestic violence rates is a reduction in stress levels among household that can be explained by a relief that the transfer gives to these families that experience severe liquidity constraints. We test this channel in two ways using again the administrative information of the program at the municipal level. First, we analyze if the impact varies according to the expectations families have about the payments. It is possible to argue that unexpected payments produce euphoria and unexpected non-payments generate stress. To test this hypothesis we create two dummy variables following Rodríguez (2015). The first dummy is equal to one if in a given month municipality *m* receives an unexpected payment, and equal to zero otherwise. This occurs because, given that payments should occur once every two months, beneficiary families do not expect a payment in two consecutive months. Therefore, the payment in the second consecutive month is a positive surprise for the families. The second dummy identifies the

negative surprises, which occur when the beneficiary families expect a payment but the payment is not delivered in two consecutive months or more. In this case, the dummy is equal to one in the second consecutive month in which the payment is not received, and zero otherwise.

Column 1 of Table 7 replicate the basic regression including the same controls as the ones in Table 4. The second column presents the coefficient of the positive surprise and in the third column we show the regression with the negative surprise dummy. A positive surprise, which means an unexpected payment, has no statistically significant effect over domestic violence, but it has the expected negative sign[13]. On the contrary the negative surprise, which is not receiving an expected payment, increases domestic violence. On the fourth column we include the payment dummy and both surprises dummies. As can be observed, the increase in domestic violence associated with an expected payment that did not occur is maintained and is higher than the reduction caused by the actual payment.

Table 7. Shortage and stress

	(1)	(2)	(3)	(4)	(5)
	Domestic violence rate	Domestic violence rate	Domestic violence rate	Domestic violence rate	Domestic violence rate
FeA Payment Month	-0.0644***			-0.0418*	-0.178***
	(0.0201)			(0.0218)	(0.0457)
Positive Surprise		-0.0405		-0.0109	
		(0.0251)		(0.0271)	
Negative Surprise			0.132***	0.105**	
			(0.0443)	(0.0453)	
Proportion of beneficiary families					0.00590
					(0.00451)
Payment Month* Proportion of beneficiary families					0.00351***
					(0.00109)
Constant	1.263***	1.240***	1.178***	1.204***	1.112***
	(0.333)	(0.333)	(0.335)	(0.336)	(0.348)
Observations	35,872	35,872	35,872	35,872	35,872

[13] When we run the regression for all the implementation stages of FeA, we get that the positive surprise is significant and negative. Also, when we run the regressions with the sample of 21,840 observations from columns 4-6 from Table 4, we get that the positive surprise has a negative and significant coefficient.

R-squared	0.006	0.006	0.006	0.006	0.007
Number of municipalities	865	865	865	865	865

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

These results reveal that the increase shortage and stress, caused by the negative surprise, increases domestic violence, while the euphoria and the reduced shortage, caused by the positive surprise, does not affect domestic violence. An alternative through which the shortage and stress channel can be tested is with the interaction of our main variable of interest and the poverty levels in the municipality. We use the number of beneficiary families as a proportion of the total number of families in each municipality as a poverty proxy[14]. It's expected that municipalities with a higher proportion of beneficiary families are the poorest municipalities, given that FeA attempts to serve the poorest individuals[15]. In the fifth column of Table 7 we show the regression with the interaction between the month of payment receipt dummy and the proportion of beneficiary families. As can be observed, the payment dummy continues to be negative and significant suggesting that the receipt of the transfers decreases domestic violence in the municipality. The interaction term however is positive suggesting that in municipalities with a higher proportion of beneficiary families the impact of the transfer on domestic violence is lower. That is, even though all specifications suggest the robust impact of the transfers on the reduction of intimate partner violence, this effect is lower in poorer municipalities.

Results from the euphoria and frustration regressions suggest that the channel through which this reduction in domestic violence occurs could be the shortage channel. The transfers from FeA alleviate the family's budget constraint and thus reduce stress and domestic violence. There are two papers in the literature that reinforce and coincide with this finding. First, Attanasio et al (2012) find that *Familias en Acción* increases food consumption, especially meat, milk, eggs and cereal. In addition, they also find that FeA increases consumption of children's clothes and education. Even though the authors suggest that this effect is coming through the

[14]To construct the total number of families in each municipality we assumed the families had four members on average.

[15] We also did this exercise with different measures of the proportion of beneficiary families in the municipality, with the Unsatisfied Basic Needs index and the SISBEN score (the Colombian census of the poor) and obtained similar results available upon request.

empowerment channel[16] the results presented in this paper suggest they may stem from the alleviation of the budget constraint. A second paper by Camacho et al (2014) reinforce this evidence as they find a reduction in crime in neighborhoods with higher proportion of program beneficiaries just after the FeA payments take place.

6. Conclusions

This paper investigates the casual impact that the cash transfers given by *Familias en Acción*, the Colombian CCT program, has brought to women's wellbeing in the country measured by the incidence in domestic violence. We find that the program has significantly reduced the domestic violence rate at the municipality level by 6% in the months when the payments are received. This is an important effect given that it measures average domestic violence in a municipality a measure that thus includes both the incidence of such events in both beneficiary and non-beneficiary households.

We test four possible transmission channels through which this effect could be taking place: female empowerment, labor market participation, changes in marital status and reduction of shortage and stress. We find that the program does not empower women. On the contrary, results suggest the probability that men are the sole decision makers in aspects related to children's education and health is increasing in beneficiary households. We do not find evidence of changes in labor market or marital status decisions either. The channel that seems to be driving the results corresponds to a reduction in stress where the transfer payments are probably alleviating the budget constraints. We use surprise payment and no payment months to test euphoria and frustration effects respectively and find a differential response of households to each of them. While the incidence of domestic violence is not affected when there are consecutive unexpected payments; it significantly increases when there are consecutive months were no payments are received. This is consistent with the idea that the impact of the transfer is stronger (weaker) when the shortage is more (less) salient.

[16] The authors state that "This result is not inconsistent with the hypothesis that the program could increase the bargaining power of women, inducing a more than proportional increase in food consumption" (Attanasio et al, 2012)

Nonetheless, we also find that in the poorest municipalities the program is increasing domestic violence.

Our results show that CCT programs generate positive unexpected impacts in the whole society and not only in the beneficiary population. However, they also suggest that the reduction in domestic violence generated by the payments is fragile and temporal, since domestic violence rises when families do not receive an expected payment. Thus, although CCT programs may help reduce domestic violence in the short run, countries need to create specific programs in order to abolish this crime and strengthen women's empowerment within the household.

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