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The Impact of the Magyar Nemzeti Bank's Funding for Growth Scheme on Firm Level Investment *

(A Magyar Nemzeti Bank NHP I. programjának hatása a vállalatok beruházására)

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Contents

Abstract

The Magyar Nemzeti Bank (the central bank of Hungary) introduced a "funding for lending" type loan program aimed at small and medium sized enterprises (SMEs) in mid-2013. We combine firms' balance sheet data with two loan data sets to study the program's impact on firm level investment in 2013. We start from a simple difference-in-differences (DID) estimator, but argue that the parallel trend assumption that underlies the method is likely violated. Therefore, we propose a correction based on the idea that the selection process involved in securing a market loan in a pre-program year is similar to the selection process into the program. Our results indicate that the program succeeded in generating extra investment in the SME sector that would not have taken place otherwise; specifically, we attribute to the program about 30% of the total investment undertaken by participating firms. Nevertheless, the effect is markedly heterogeneous with respect to firm size, being proportionally larger for smaller firms.

JEL: D04, G38, E58.

Keywords: funding for lending, program evaluation, difference-in-differences estimation, unconventional monetary policy.

Összefoglaló

Az MNB 2013 közepén vezette be a kis és közepes méretű vállalkozásokat célzó növekedési hitel programját. Vállalatok mérleg adatait két hitelezési adatbázissal összekapcsolva vizsgáljuk, hogy a program hogyan hatott a vállalati szintű beruházásokra 2013-ban. Egy egyszerű különbségek különbsége becsléssel indítunk. Mivel a módszer párhuzamos trend feltevése valószínűleg nem teljesül, bevezetünk egy korrekciót. Az alapötlet az, hogy a programba bekerülés szelekciós mechanizmusa hasonló ahhoz, ami a program bevezetés előtt a piaci hitel felvételnél működött. Eredményeink szerint a programnak sikerült olyan többlet beruházásokat generálnia, melyek a program hiányában nem valósultak volna meg; a résztvevő vállalatok beruházásainak mintegy 30%-át tulajdonítjuk a programnak. Ugyanakkor a program relatív hatása vállalat méret szerint erősen heterogén, a kisebb vállalatok esetében nagyobb.

1 Introduction

To facilitate recovery from the Great Recession, several central banks around the world have taken on a more direct role in firm financing in recent years. One of the better known such programs is the Bank of England's Funding for Lending Scheme. The Magyar Nemzeti Bank (MNB) introduced its own Funding for Growth Scheme (Növekedési Hitelprogram; henceforth, NHP) in June, 2013 with the purpose of reinvigorating the market for business loans and, as was hoped, promoting economic growth through increased investment. As post-program data starts becoming available, it is important to examine the extent to which these goals have been met. In this paper we focus on firm-level investment in 2013 as the outcome of interest, and attempt to identify how much new investment the program has been able to generate relative to an alternative universe without the program. This is a rather narrow perspective that represents just one of many inputs a broader cost-benefit analysis would require.

The central bank allocated approximately HUF 700 billion[1] to the first wave of the program. Commercial banks and other financial intermediaries were entrusted with lending out these funds to SMEs at an interest rate not exceeding 2.5% while bearing the risk of default.[2] The policymaker also restricted the use of the loan to four main purposes: (i) to refinance existing loans (HUF or foreign currency denominated); (ii) to finance working capital; (iii) to finance new long-term investment; (iv) pre-financing EU funds. The first wave of the program ended in September, 2014 after practically all funds were loaned out. The program was extended almost immediately, but the layout of funds under the second phase was negligible for the rest of 2013. Thus, the 2013 investment figures we construct from firm balance sheets reflect the effect of the first wave only. Some basic facts about the first phase of the program are summarized in Table 1.

The fundamental problem in evaluating the effect of the NHP program is that participating firms cannot be regarded as a random sample from the universe of firms. Comparing the average investment volume of participating firms with non-participating firms in 2013 reveals a large gap between the two groups: HUF 67.5 million for the former vs. 4.5 million for the latter. However, because firms partly self-select into the program, and are also screened by banks, it is not clear how much of this difference can be attributed to the program itself, and how much of it is due to systematic differences that would have led to different investment outcomes for NHP vs. non-NHP firms even without the program. Alternatively, making a before-after comparison, the mean real investment of participating firms, measured at 2013 prices, increased from HUF 47.0 million in 2012 to 67.5 million in 2013. It is still not clear how much of this change is due to the program and how much of it is due to changes in general economic conditions across the two years. From either perspective, identifying the program effect amounts to constructing the counterfactual investment path that participating firms would have been on in the absence of the program. To put it somewhat differently, what we want to know is how much of the 2013 investment volume by participating firms would have been realized anyway, and how much of it is truly new investment that would not have happened otherwise.

The simplest econometric method of program evaluation is to regress the outcome of interest on a treatment dummy variable and a set of pre-treatment covariates (or a flexible function of them) using variation across firms only. Endresz and Harasztosi (2014) follow essentially this strategy in evaluating the effect of foreign currency lending in Hungary on firms' investment outcomes. However, it remains doubtful that the set of observed covariates is rich enough to control for all selection effects. In this paper we propose an identification strategy capable of dealing with selection on unobservables using reasonable assumptions. In particular, we estimate the average treatment effect for the treated as the difference between two difference-in-differences (DID) estimators. While the proposed method is subject to a number of caveats, in our opinion it represents the best current attempt at identifying the impact of NHP from micro data.

Our analysis relies on three data sources. The firm panel of the National Tax and Customs Office (Nemzeti Adó- és Vámhivatal, NAV) contains balance sheet data for all Hungarian firms with double entry bookkeeping obligations since the early 1990s. We

[1] In 2013 the prevailing exchange rate was roughly 300 HUF/EUR, so this amount is on the order of EUR 2.3 billion or 2.3% of Hungary's GDP. To convert figures expressed in 2013 forints into 2013 euros, apply the same exchange rate throughout the paper.

[2] Eligibility for an NHP loan is tied to the official EU definition of an SME. The most important constraint is that participating firms cannot have more than 250 employees.

Table 1
Basic facts about the first phase of NHP

Firm size (employees)	Number of firms in 2013	Number of participants	Part. rate	Total NHP loans (millions, HUF)	Av. loan size (millions, HUF)
Micro (1-9)	318,574	2,894	0.9%	202,933	70.1
Small (10-49)	21,726	2,224	10.2%	198,069	89.1
Medium (50-249)	4,359	803	18.4%	213,331	265.7
N/A	55,736	203	0.4%	48,993	241.4
Total	400,395	6,124	1.5%	663,325	108.3

Note: To convert 2013 HUF into 2013 EUR, divide by 300.

use this data set to construct firm-level real investment and capital stock in 2013; see Appendix B.2 for a detailed description of the definition of these variables. Several other firm characteristics, such as number of employees, are also available in this data set. NHP loan data are supplied on a mandatory basis to the Magyar Nemzeti Bank by mediating banks. This information allows us to identify which firms in the NAV database are NHP participants, and what the officially stated purpose of each NHP loan is. Finally, we make use of the Central Credit Registry, which can also be matched up with the NAV firm panel, to gain information about other business loans held by firms. While the first data set contains public information, the other two are only accessible through the central bank. Hence, our results are not publicly replicable at present.

Qualitatively, our main finding is that the program "works", though its proportional effect is very heterogeneous with respect to firm size. On average, we attribute about 30% of the total investment undertaken by participating firms in 2013 to the program, but this ratio is much larger for micro firms (63%) and lower among upper medium-sized firms (practically zero). Overall, we estimate that the program is responsible for a 6.8% increase in investment in the SME sector.

Perhaps the most important caveat one needs to keep in mind when interpreting the numerical results is that what we record as new investment at the firm level does not necessarily correspond to an expansion of the capital stock in the aggregate economy. For example, if firm *A* has some old machinery with book value reduced to zero, but firm *B* buys it at a positive price, then we see an increase in fixed assets on firm *B*'s balance sheet, but there is no disinvestment recorded on firm *A*'s. Of course, this transaction merely reallocates existing capital, and does not add to the aggregate capital stock. (Nevertheless, reallocation alone might lead to increased efficiency in using existing capital.)

Lending or refinancing programs focused on business credit have been implemented by other central banks, most notably by the Bank of England, but the ECB's (Targeted) Long Term Refinancing Operation ((T)LTRO) is also similar in spirit. While there has been some effort directed at evaluating these programs (e.g., Churm et. al. 2012, Darracq-Paries and De Santis 2013, Balog et al. 2014, MNB 2014), the analysis in these studies is based on aggregate data, and the focus is on credit market outcomes and (perhaps) inflation or output. We are not aware of other studies combining firm level micro data with rigorous econometric methodology to estimate the impact of these programs on real economic outcomes. Thus, both the results and the methodology of this paper might be of interest to researchers and policymakers outside of Hungary.

The rest of the paper is organized as follows. Section 2 describes the theoretical framework for program evaluation and lays out the identification strategy in detail. Section 3 presents the main numerical results along with some sensitivity analysis. Section 4 discusses limitations and concludes. There are several Appendices that give more information about the program, describe the data in more detail, and present results from variations on the baseline regression specifications.

2 The theoretical framework for evaluating NHP

We use the now standard potential outcome framework (see, e.g., Imbens and Wooldridge 2009) to describe the parameter of interest and the identification strategy.

2.1 BASIC DEFINITIONS AND PARAMETER OF INTEREST

Let $Y_{13}(1)$ denote a randomly chosen firm's investment outcome in 2013 if participation in NHP were imposed on it exogenously.[3] Similarly, let $Y_{13}(0)$ denote the firm's investment outcome if it were exogenously excluded from NHP (or the program did not exist at all). These two random variables represent *potential outcomes* out of which only one is observed for each firm. If a firm chooses to participate in the program, then its actually observed investment in 2013 is given by $Y_{13}(1)$, otherwise we observe $Y_{13}(0)$. More formally, let P be the indicator of program participation, i.e., $P = 1$ if a firm participates in the first phase of NHP and $P = 0$ if not. The relationship between the potential outcomes and the actually observed investment, denoted Y_{13}, is given by the equation

$$Y_{13} = PY_{13}(1) + (1 - P)Y_{13}(0). \tag{1}$$

Comparing various aspects of the potential outcome distributions leads to different measures of the effect of the program, often referred to as a "treatment effect". In case of voluntary participation, the average treatment effect for the actually treated subpopulation (ATT) is of particular interest from a policy standpoint. Formally, this parameter is defined as

$$ATT = E[Y_{13}(1) \mid P = 1] - E[Y_{13}(0) \mid P = 1].$$

One might also be interested in the average treatment effect in the entire population of firms (usually abbreviated as ATE), especially if a significant extension of the program is under consideration. This parameter is however more difficult to identify, so in this paper we restrict attention to ATT.

2.2 IDENTIFICATION STRATEGY

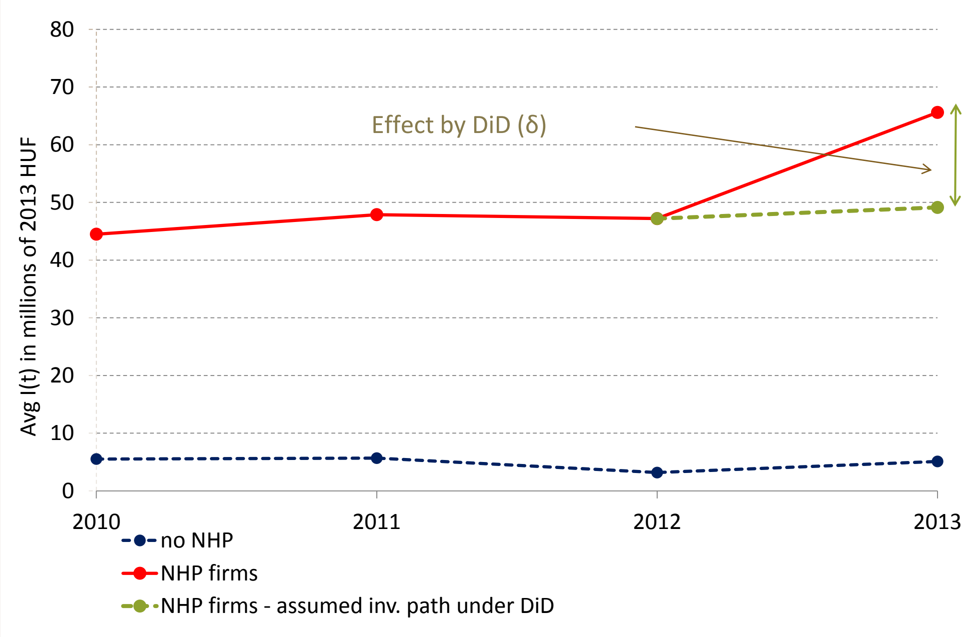
The definition of ATT contains the counterfactual expression $E[Y_{13}(0) \mid P = 1]$. This quantity describes the hypothetical average investment outcome of participating firms had the program not been implemented. Obviously, this baseline is not estimable without further identifying assumptions. To motivate these, let us decompose the actually observed difference between the average investment of participants vs. non-participants in the following way:

$$\begin{aligned} &E[Y_{13} \mid P = 1] - E[Y_{13} \mid P = 0] \\ &= ATT + \{E[Y_{13}(0) \mid P = 1] - E[Y_{13}(0) \mid P = 0]\}. \end{aligned} \tag{2}$$

Equation (2) follows directly from the definition of ATT and (1). As can be seen, the difference between the average investment of the two groups is due to two factors: the average effect of the program among participants and a selection effect described by the second term on the r.h.s. of (2). Specifically, if there are systematic differences between participating and non-participating firms, then the two groups would have likely had different investment outcomes in 2013 even in the absence of the program. For example, it is plausible to assume that mediating commercial banks tried to restrict NHP loans to better-quality firms (with already higher investment rates or volumes) as the markup they can charge is limited and they have to bear the risk of default. Furthermore, investment is related to firm size, and small to medium size enterprises are overrepresented among participants relative to micro firms.

[3] We use real investment volume (in millions of 2013 HUF) in our empirical models. In earlier versions of the paper we also considered models with investment rate as the dependent variable, but as the results are similar, we dropped them to simplify the presentation. Our models control for the capital stock directly.

Figure 1
The DID identification strategy: average investment volume by participants and non-participants



How can one estimate the selection component in equation (2)? As the program did not yet exist in 2012 (and arguably was not anticipated at all), the observed difference in the average investment outcomes of the two groups in 2012 is a natural candidate to proxy for the difference that would have materialized in 2013 in the absence of the program. Stated more formally, the underlying assumption is that

$$E[Y_{13}(0) \mid P = 1] - E[Y_{13}(0) \mid P = 0] = E[Y_{12}(0) \mid P = 1] - E[Y_{12}(0) \mid P = 0], \qquad (3)$$

where $Y_{12}(0)$ is a randomly chosen firm's potential investment in 2012 had NHP not been implemented a year later. In the absence of anticipation effects, it seems reasonable to further assume that $Y_{12}(0)$ coincides with the actually observed investment outcome Y_{12}. Combining the two assumptions, the ATT parameter is identified as

$$\delta \equiv \{E[Y_{13} \mid P = 1] - E[Y_{13} \mid P = 0]\} - \{E[Y_{12} \mid P = 1] - E[Y_{12} \mid P = 0]\}. \qquad (4)$$

The expression in (4) states that one can estimate ATT by comparing the average investment outcome of firms with and without NHP both before and after the program. This estimation strategy is known as "difference-in-differences" (DID) in the econometrics literature; see, e.g., Angrist and Pischke (2009, Ch. 5).

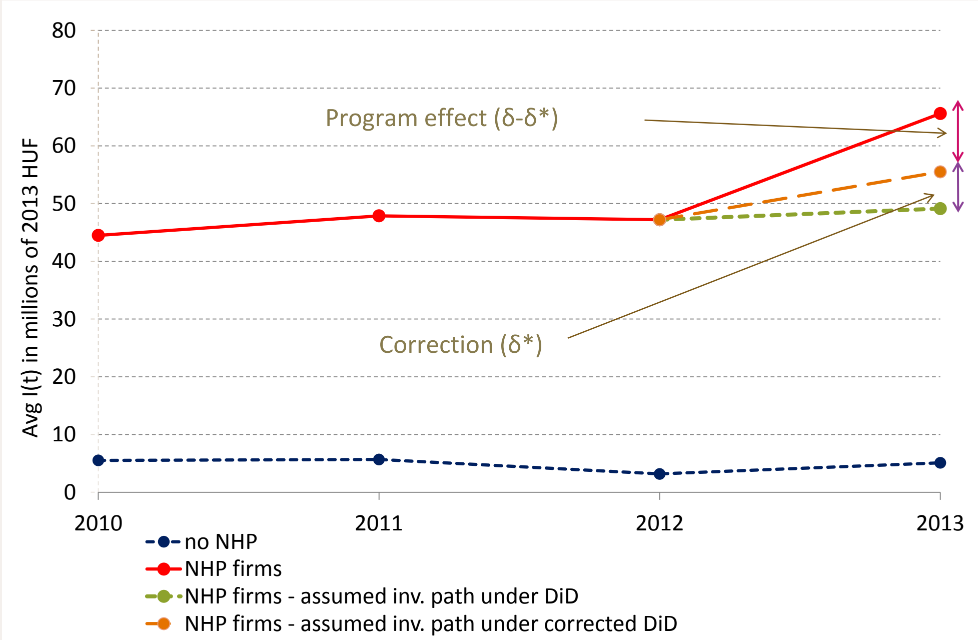
It is customary to examine the empirical plausibility of the DID identification assumption by plotting the sample analog of the difference

$$E[Y_t \mid P = 1] - E[Y_t \mid P = 0] \qquad (5)$$

for pre-program years. For example, Figure 1 shows investment volume in millions of 2013 HUF for NHP vs. non-NHP firms between 2010 and 2013. It is clear that participating firms, on average, invested significantly more even before the program, and the difference between the two groups appears stable in the years leading up to the program. The assumption underlying DID estimation is precisely that the same difference would have prevailed in 2013 in the absence of the program, as shown by the dashed extension of the average investment path for NHP firms.

In Appendix A we present more plots similar to Figure 1, broken down by firm size and the definition of the outcome variable (investment volume vs. investment rate). Although in most cases the historical difference (5) appears reasonably stable before the program, this observation alone does not fully validate the DID identification strategy in the present setting. This is due to

Figure 2
Correction to the DID identification strategy



the fact that the division of the population into a treatment vs. control group is completely endogenous, i.e., a choice made by firms and banks, rather than the result of a "natural experiment" as in the classic DID literature (e.g., Card and Krueger, 1994).

To see this point more clearly, rearrange equation (3) to obtain an alternative interpretation of the identification assumption behind DID:

$$E[Y_{13}(0) \mid P = 1] - E[Y_{12}(0) \mid P = 1] = E[Y_{13}(0) \mid P = 0] - E[Y_{12}(0) \mid P = 0]. \quad (6)$$

This condition states that the average change in investment from 2012 to 2013 in the absence of the program would have been the same for participants and non-participants alike. Unfortunately, it is very likely that this condition is violated. Every year, even in bad macroeconomic conditions, there are firms that plan to *increase* their investment due to some favorable idiosyncratic "shock" (e.g., a good business idea). It is realistic to assume that eligible firms for which $Y_{13}(0) - Y_{12}(0)$ is a larger positive quantity are more likely to be selected into treatment. Therefore, the l.h.s. of (6) is likely to be larger than the r.h.s., i.e., the counterfactual mean investment path of NHP firms in 2013 does not run parallel with that of non-NHP firms. Rather, NHP firms would have likely been on a steeper path even without the program as illustrated in Figure 2.

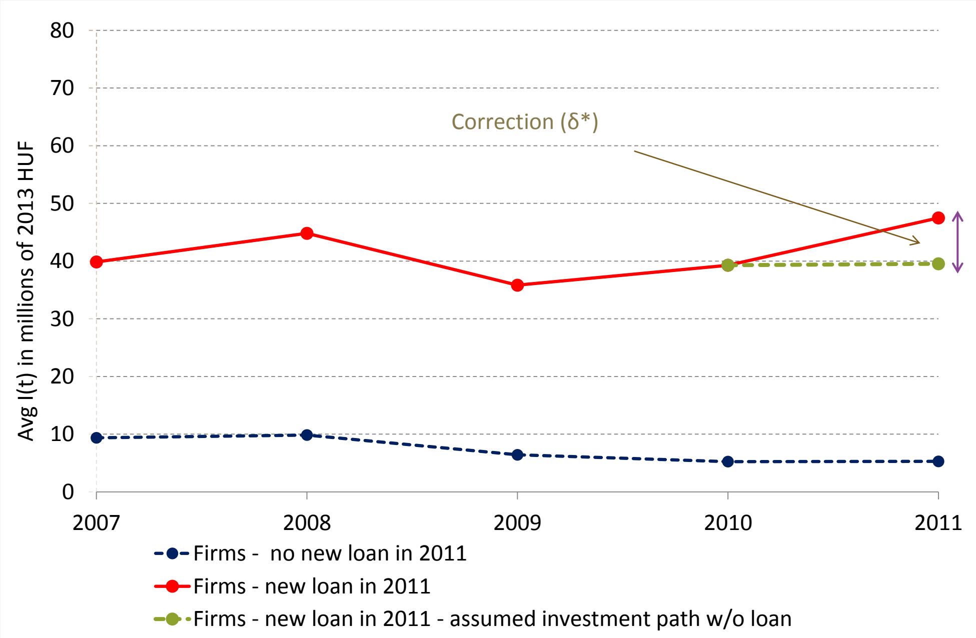
We propose a correction δ^* to the basic DID estimator that attempts to capture this additional selection effect. (This modification will implicitly produce the new counterfactual investment path shown in Figure 2.) By adding and subtracting the same terms, the decomposition given in equation (2) can be expanded to

$$\begin{aligned} E[Y_{13} \mid P = 1] - E[Y_{13} \mid P = 0] &= ATT + \{E[Y_{12}(0) \mid P = 1] - E[Y_{12}(0) \mid P = 0]\} \\ &+ \{E[\Delta Y_{13}(0) \mid P = 1] - E[\Delta Y_{13}(0) \mid P = 0]\}, \end{aligned} \quad (7)$$

where $\Delta Y_{13}(0) = Y_{13}(0) - Y_{12}(0)$. Equation (7) separates out two aspects of the selection process. First, there can be a pre-existing *level* difference in the average investment volume of participating and non-participating firms, given by the second term of equation (7). In addition, as discussed above, the two groups of firms might have *changed* their investment differently across the two years even in the absence of the program, as captured by the third term in equation (7).

Our idea is to consider the same investment decomposition for firms that borrowed on the market in a pre-program year vs. those that did not, and use parts of the selection process involved in market finance as a proxy for parts of the selection process into NHP. Specifically, let P_t^* indicate whether or not a randomly chosen firm took out a new market loan in a given year t before

Figure 3
Derivation of the correction term: investment by firms with and without a new market loan in 2011



the program and let $Y_t^*(p)$, $p = 1, 0$, denote the potential investment outcomes with and without market finance. Actual investment is again $Y_t = Y_t^*(1)P_t^* + Y_t^*(0)(1 - P_t^*)$. Mimicking equation (7), we can write

$$\begin{aligned} E[Y_t \mid P_t^* = 1] - E[Y_t \mid P_t^* = 0] &= ATT^* + \{E[Y_{t-1}^*(0) \mid P_t^* = 1] - E[Y_{t-1}^*(0) \mid P_t^* = 0]\} \\ &+ \{E[\Delta Y_t^*(0) \mid P_t^* = 1] - E[\Delta Y_t^*(0) \mid P_t^* = 0]\}, \end{aligned} \tag{8}$$

where ATT^* is the average effect of a market loan on the investment volume of borrowing firms. Identifying $Y_{t-1}^*(0)$ with the actually observed investment Y_{t-1}, equation (8) shows that the DID estimand

$$\delta^* \equiv \{E[Y_t \mid P_t^* = 1] - E[Y_t \mid P_t^* = 0]\} - \{E[Y_{t-1} \mid P_t^* = 1] - E[Y_{t-1} \mid P_t^* = 0]\} \tag{9}$$

is the sum of the selection effect related to the slope of the planned investment path under self-finance plus the average treatment effect of a market loan on borrowing firms, i.e.,

$$\delta^* = ATT^* + E[\Delta Y_t^*(0) \mid P_t^* = 1] - E[\Delta Y_t^*(0) \mid P_t^* = 0].$$

Hence, if we assume that firms that chose to participate in NHP, or at least the majority of them, would have borrowed on the market to finance some investment even in the absence of the program, we can take δ^* as a proxy for $E[\Delta Y_{13}(0) \mid P = 1] - E[\Delta Y_{13}(0) \mid P = 0]$ in equation (7). Therefore, we can estimate the effect of the NHP program as the difference between two DID estimators, namely (4) minus the correction term (9).

In estimating δ^*, one would ideally pick a year t with macroeconomic conditions similar to the program year 2013. Unfortunately, data availability constraints prevent us from using the closest pre-program year ($t = 2012$); the last year in which the central credit registry database and the tax authority's firm database can be matched up is 2011. Therefore we set $t = 2011$ in our baseline estimations, but examine other periods as a robustness check (the empirical results will be discussed in detail in Section 3).

To illustrate the correction mechanism graphically, Figure 3 depicts the sample analogs of the time series $E[Y_t \mid P_{11}^* = 1]$ and $E[Y_t \mid P_{11}^* = 0]$ for $t = 2007, \ldots, 2011$. Qualitatively, Figure 3 is remarkably similar to Figure 1: (i) firms that borrow on the market have a historically larger average investment volume; (ii) the investment differential between borrowing and non-borrowing firms is reasonably stable in the years leading up to the loan; and (iii) in the loan year borrowing firms exhibit a sizable uptick δ^* in investment volume in excess of this historical difference (as discussed above, we interpret this as part selection, part the treatment effect of the loan itself). Note, however, that the average pre-program investment difference between NHP and non-NHP firms is roughly HUF 45 million, while the corresponding difference is only HUF 35 million for firms with and without a new market loan. The proposed correction allows for such level discrepancy between NHP participants and market borrowers. What we do assume however is that in the absence of the program NHP firms would have borrowed on the market and exhibited the same investment-uptick δ^* relative to the investment path of non-NHP firms as did borrowing firms in 2011 relative to the investment path of non-borrowing firms (see Figures 2 and 3).

2.3 DISCUSSION OF THE CORRECTION TERM

The corrected DID estimator presented in the previous section is, we believe, a reasonable but admittedly somewhat ad-hoc attempt to account for unobserved heterogeneity in evaluating the investment impact of NHP. We will therefore discuss in detail a number of theoretical caveats the proposed correction is subject to and provide further empirical justification for it.

The correction to the basic DID estimator is predicated on the assumption that NHP firms would have borrowed on the market even in the absence of the program. This immediately rules out participating firms being credit constrained on the "extensive margin", i.e., in the sense of facing a completely vertical aggregate credit supply curve at their current level of capital. Nevertheless, the assumption that such firms do not participate in NHP is not far-fetched; after all, these are firms that no bank is willing to lend more to at *any* price, and it is not clear why this would change given that the interest rate on NHP loans is at most 2.5% and banks bear the default risk.

Of course, we allow for firms being credit constrained in the more general sense of facing an upward sloping aggregate loan supply curve (see Banerjee and Duflo 2014). Indeed, as argued by *ibid.*, for an unconstrained firm facing a horizontal credit supply curve at the market rate, a subsidized loan program such as NHP will prompt an increase in its capital stock only if the loan amount the bank is willing to grant through the program exceeds the firm's entire pre-existing capital stock.[4] Loans smaller than this size will be spent entirely on refinancing existing capital. In contrast, firms facing an upward sloping credit supply curve will generally split the cheap loan in some proportion between refinancing existing capital and expanding the capital stock. For example, if a firm faces a vertical credit supply curve, a smaller loan would be spent entirely on extra investment. More generally, the proportion of a marginal loan dollar going toward refinancing versus new investment is determined by the relative slopes of the credit demand and supply curves.

The selection story most consistent with the proposed correction is that participating firms' investment plans are driven primarily by positive shocks to the marginal product of capital, i.e., by outward shifts in the demand curve for capital. This ensures that they would want to borrow some (lesser) amount on the market even without the program. However, inasmuch as firms, constrained or not, face an interest rate higher than 2.5% on their marginal loan, they will have an incentive to apply for subsidized funds, which they may use in part to expand their capital stock even when they have a stagnant or slightly retreating capital demand curve. The problem is that, in general, these non-expanding firms would not have increased their borrowing otherwise, i.e., for them a new market loan is not the relevant counterfactual. If the proportion of such firms is actually high among program participants, the proposed correction is too large, meaning that the effect of the program is underestimated. The question is, to what extent firms of this type were allowed by banks into the program. As shown by Figure 1, NHP participants had persistently and substantially outperformed non-participants in terms of investment volume over an extended period before the program (the same is true within size categories as shown in Figure A.1 in Appendix A). It is hard to imagine that such an advantage is sustainable year after year without the capital demand schedule moving outward in a fairly steady fashion for the majority of NHP firms.

In fact, one might just as well be worried about the possibility that NHP participants are too much the "cream of the crop" even among creditworthy firms and thus are not representative of firms borrowing on the market as a whole. This could happen if the 2.5% interest rate cap is substantially less than the usual risk premium banks charge to finance most SMEs, so they cherry-pick participants as much as possible. If this select group of firms have capital demands expanding at a faster rate than that

[4] More precisely, when we talk about the "pre-existing capital stock" or "pre-program capital stock", we mean the capital stock that would have been financed from the market in the absence of the treatment. This may differ from the capital stock in the previous year if, say, the firm's demand curve for capital moves to the right in 2013.

of the average borrower's, the correction term might be too small, meaning that the effect of the program is overestimated. Nevertheless, there are also some counterarguments to this scenario that depend on the details of the loan approval process. For example, Banerjee and Duflo (2014) also point out that if individual loan officers are penalized for default by existing clients, there will be incentives to use the cheap money available through the program to bail out some bad firms.

Of further concern are the differences in macroeconomic conditions between 2011 (the baseline correction year) and 2013 (the program year). Most importantly, the central bank's policy rate was considerably lower and decreasing throughout 2013, presumably causing a downward shift in the aggregate loan supply curve over the year independently of the program. This also suggests that the correction could well be too small. In particular, suppose that the firms that normally borrow on the market to invest are indeed those that receive a positive shock to the marginal product of capital. If there is a simultaneous downward shift in the aggregate loan supply curve faced by these firms, they will want to borrow (and invest) still more. So even if the capital demand shocks are roughly the same across the two years, the counterfactual market loans that would have been taken out by NHP firms in 2013 would still be larger than the average market loan in 2011.

In sum, there are a number of conflicting factors causing either upward or downward bias in the correction term, and it is hard to judge their overall impact. We therefore provide further pieces of empirical evidence in support of the proposed procedure. First, as we will show in Section 3, the baseline correction essentially eliminates the program effect among larger participating firms (in the 150 to 250 employee range). For this group it is possible to conduct an informal regression discontinuity analysis, presented in Appendix C.1, which independently confirms this finding with the help of additional data not used in the estimation. We consider the regression discontinuity results as a key piece of corroborating evidence showing that the baseline DID correction is well "calibrated". While the result is, strictly speaking, specific to the group of firms close to the eligibility cutoff, it also enhances one's confidence in the overall validity of the baseline correction. Second, in Appendix C.2 we compare some observed characteristics of firms that participated in NHP in 2013 with characteristics of firms that borrowed on the market in 2011. The two groups are reasonably alike, making it more credible that the selection process into the two "treatments" might be similar even with respect to unobserved investment-relevant factors. Finally, in Appendix D.1 we examine the sensitivity of our baseline estimation results presented in Section 3 to the choice of the time period over which the basic DID regression and the correction term is estimated. We find that the most important numerical conclusions are reasonably robust.

2.4 REGRESSION IMPLEMENTATION OF THE ESTIMATORS

The most direct implementation of the proposed estimator is based on sample analogs of the expressions given in equation (4) and (9). Nevertheless, there are significant advantages to embedding these estimators into a regression model. This can be done in a number of different ways. Our starting point is a two-period panel regression with separate firm and time (year) fixed effects:

$$Y_{it} = c_i + \lambda_t + \delta P_i D13_t + u_{it}, \quad t = 2013, 2012;\ i = 1, \dots, N. \tag{10}$$

Here Y_{it} denotes the value of the dependent variable for firm i in period t; $P_i = 1$ if the firm participates in the first phase of NHP and is zero otherwise; $D13_t$ is a dummy variable that takes on the value one in 2013 and is zero otherwise; c_i is a firm-specific fixed effect; λ_t is a year fixed effect, and u_{it} is an idiosyncratic error. One can estimate (10) by OLS either after first differencing in the time dimension or applying the within transformation (the latter procedure is usually referred to as "the" fixed effect estimator). In the two-period case the two methods give numerically identical results; let $\widehat{\delta}$ denote the value of this estimate. It is easy to show that under standard conditions $\widehat{\delta}$ converges in probability to (4) as $N \to \infty$; see Wooldridge (2002, Ch. 10).

The correction term given in (9) can be estimated by a similar regression.

$$Y_{it} = c_i + \lambda_t + \delta^* P_i^* D11_t + u_{it}, \quad t = 2011, 2010;\ i = 1, \dots, N^*, \tag{11}$$

where $P_i^* = 1$ if firm i has a new loan in 2011 and $D11_t$ is a year dummy for 2011. Denoting the fixed effect estimator of δ^* by $\widehat{\delta^*}$, the proposed ATT estimator is given by $\widehat{\delta} - \widehat{\delta^*}$.

We estimate robust standard errors for $\widehat{\delta}$ and $\widehat{\delta^*}$ clustered by firms, i.e., allow u_{it} to be serially correlated across different periods, and allow for heteroskedasticity across firms in a given period. (Serial correlation is a concern only for versions of the model estimated over more than two periods. Such models are used as a robustness check.)

2.5 FURTHER SPECIFICATION CHOICES

We consider a number of different versions and extensions of the basic model specification described above. In each case we match the specification of the corrective regression (11) to that of (10), so the former is not separately discussed.

Dependent variable: Let I_{it} denote firm i's real investment in year t, expressed in millions of 2013 HUF; see Appendix B.2 for a detailed description of how this variable was constructed. In all model specifications discussed in the paper we use I_{it} as the dependent variable ($Y_{it} = I_{it}$). A natural alternative is investment rate, i.e., investment relative to (pre-existing) capital stock. In earlier versions of the paper we considered such models as a robustness check, but as they led to similar results, we dropped them to simplify the presentation. As will be seen shortly, most of our specifications will include direct controls for capital.

Treatment definition: Our broader definition of the treatment group ($P_i = 1$) includes recipients of any type of NHP loan; the narrower definition considers as treated only holders of a "new investment" loan (and possibly other loans).[5] The broader definition permits the measurement of the indirect, and possibly more long-term, effect of loans formally not in the "new investment" category. The broader definition is also more attractive from the perspective of cost-benefit analysis.

Allowing for treatment effect heterogeneity: Firm size is an important covariate of both investment volume and investment rate. To the extent that size is persistent, firm fixed effects control for level differences in investment outcomes related to it, but it also turns out to be important to allow for differential *treatment effects* by size. We use the (real) capital stock (K) as well as number of employees (L) as measures of firm size. In particular, we include in (10) cubic polynomials of $K_{i,t-1}$ and/or $L_{i,t-1}$ both in levels and interacted with $P_i D13_t$.[6] We also examine some specifications that allow for treatment effect heterogeneity with respect to industry, but as the aggregate results are not materially affected, we only present these models in Appendix D.2 as part of our robustness checks.

Additional control variables: One can incorporate additional pre-treatment control variables $X_{i,t-1}$ with both time and across-firm variation into the regression models. Such controls can help account for differences in the investment paths of treated vs. non-treated firms in the absence of the program, thus enhancing the credibility of the identification scheme. A secondary benefit is a reduction in estimated standard errors. Specifically, we employ a number of financial statistics computable from a firm's balance sheet (e.g., the debt-equity ratio). The full set of controls is specified in Appendix B.2.

Final specifications: In light of the issues discussed above, we estimate the following regression specifications:

(S1) $I_{it} = c_i + \theta_t + \delta P_i D13_t + u_{it}$

(S2) $I_{it} = c_i + \theta_t + \delta P_i D13_t + \sum_{j=1}^{3}(\alpha_j K_{i,t-1}^j + \beta_j K_{i,t-1}^j P_i D13_t) + u_{it}$

(S3) $I_{it} = c_i + \theta_t + \delta P_i D13_t + \sum_{j=1}^{3}(\alpha_j K_{i,t-1}^j + \beta_j K_{i,t-1}^j P_i D13_t) + \sum_{j=1}^{3}(\kappa_j L_{i,t-1}^j + \lambda_j L_{i,t-1}^j P_i D13_t) + u_{it}$

(S4) $I_{it} = c_i + \theta_t + \delta P_i D13_t + \sum_{j=1}^{3}(\alpha_j K_{i,t-1}^j + \beta_j K_{i,t-1}^j P_i D13_t) + \sum_{j=1}^{3}(\kappa_j L_{i,t-1}^j + \lambda_j L_{i,t-1}^j P_i D13_t) + X_{i,t-1}'\gamma + u_{it}$

Estimation period: As presented in Section 2.4, our benchmark specification uses 2012-13 to estimate the basic DID regressions and 2010-11 to estimate the corrective regressions. We conduct robustness checks with respect to the choice of the estimation period in Appendix D.1.

Data cleaning: Models $S1$ through $S4$ are all estimated on samples smaller than the raw data shown in Table 1. This has two basic reasons. First, many firms have missing observations on the capital stock, employment or other variables used in the analysis over the sample periods examined. Second, the investment volume distribution contains outliers that exert undue influence on the results unless the corresponding firms are dropped from the sample. The outlier cleaning procedure, which is crucial for sensible results to obtain, is described in more detail in Appendix B.2. The final sample sizes used in estimating models S1 through S4, and the corresponding corrective regressions, are reported in Appendix B.3.

[5] In estimating (11) we attempt to match the definition of P^* to the definition of P. In particular, when P is broadly defined, we set $P^* = 1$ for those who take out any type of new market loan in 2011. When P corresponds to investment loans, we set $P^* = 1$ only for those firms that take out a new long term loan (12 months or over).

[6] Both capital stock and employment has enough time variation so that fixed effects estimation remains feasible. See again Appendix B.2 for the construction of the capital stock variable.

3 Estimation results

3.1 AVERAGE EFFECTS

Table 2 summarizes the full sample point estimates of δ, the basic DID estimand, and δ^*, our correction for self-selection related to the slope of the planned investment path, for both program definitions. The difference between these two parameters captures the average effect of the program on participants (in millions of 2013 HUF). More precisely, in models *S*2 through *S*4, the average effect depends on the capital stock and/or employment, and the reported figures correspond to the 2012 sample average among participants.[7] For each specification, the δ estimates are highly significant, while the δ^* estimates are at least borderline significant. The sum of the two standard errors provides an estimated upper bound for the standard error of $\widehat{\delta}-\widehat{\delta^*}$. Even this conservative approximation convincingly shows that the corrected estimate is statistically different from zero in all cases.

For ease of interpretation, the estimated effects on investment volume are also expressed relative to average 2012 capital stock among participants. In other words, the columns labeled "I/K impact" give the average increase in the investment rate for participants with an average level of pre-treatment capital and/or level of employment.

Generally speaking, the estimation results provide evidence that the first phase of the Funding for Growth Program did contribute to increased investment, i.e., firms undertook investment projects that they would not have undertaken in the absence of the program. Furthermore, we see that the additional correction to the basic DID estimator makes an economically significant difference. Given the somewhat *ad-hoc* nature of the correction term, the uncorrected estimates are useful as a generous upper bound for the program effect.

There is a fair amount of variation in the point estimates across models and treatment definitions. In terms of absolute impact, the estimated average effect of the broadly defined program on participants ranges from HUF 18 million to HUF 37 million in extra investment, i.e., model uncertainty changes the estimates by as much as a factor of two. Once controls are introduced, it is actually the basic DID estimator that is responsible for most of the variation; the correction is fairly stable at about HUF 11 to 13 million. As the model specification changes, the estimated values of δ and δ^* always change in the same direction, which is indirect evidence that selection into the program may indeed be similar to the selection process involved in borrowing on the market.

Looking at the proportional effects in more detail, the *S*1 model, which controls only for firm and year fixed effects, gives a 6.2% point average increase in the investment rate of participants. After allowing for heterogeneous effects as a function of capital (model *S*2), the program impact on the investment rate rises substantially to 12.7% points (at the average level of pre-treatment capital among participants). Adding employment in the same fashion reduces the effect to 9.2% (S3), and introducing other controls brings it further back down to 6.6% (S4).

Narrowing the treatment definition to loans whose declared purpose is to finance "new" investment, we see that the estimated program effects are substantially larger, both in absolute and relative terms, across all models; specifically, estimates of the proportional program effect range from about 17% to 24%, which translates to HUF 38 to 54 million per participating firm with a new investment loan. This increase is to be expected, as firms with such loans were required to undertake some investment whereas firms with only, say, a refinancing loan were not (and many of them did not). Thus, the likelihood of a positive direct effect is considerably larger in this narrower group. Changes in the model specification moves the estimated values of δ and δ^* much the same way as with the broader treatment.

That the pre-treatment capital level, and its powers and interactions with the program dummy, have a statistically and economically significant effect on the estimates is not surprising as a firm's investment need is expected to be correlated with capital.

[7] Technically, we measure capital stock and employment for each firm in the sample in deviations from the 2012 mean value. This ensures that $\widehat{\delta}$ and $\widehat{\delta^*}$ has the interpretation stated above.

Table 2
Estimated average program effects for participants

Model specification		Treatment=all NHP loans: Parameter estimates (millions of HUF) δ	δ^*	$\delta-\delta^*$	I/K impact (% point) $\delta/\bar{K}$	$\frac{(\delta-\delta^*)}{\bar{K}}$	Treatment=new investment loans: Parameter estimates (millions of HUF) δ	δ^*	$\delta-\delta^*$	I/K impact (% point) $\delta/\bar{K}$	$\frac{(\delta-\delta^*)}{\bar{K}}$
S1 (Inv)	firm FE	20.10	2.09	18.0	6.9%	6.2%	57.19	8.413	48.8	25.2%	21.5%
		[2.270]	[1.407]				[3.323]	[3.117]			
S2 (Inv)	FE+*f*(K)	47.63	10.81	36.8	16.4%	12.7%	77.99	24.05	53.9	34.3%	23.7%
		[3.861]	[5.770]				[5.496]	[4.442]			
S3 (Inv)	FE+*f*(K,L)	40.09	13.29	26.8	13.8%	9.2%	70.51	27.46	43.1	31.0%	18.9%
		[3.985]	[5.808]				[4.815]	[6.034]			
S4 (Inv)	FE+*f*(K,L,X)	30.19	11.12	19.1	10.4%	6.6%	60.59	22.94	37.7	26.6%	16.6%
		[3.827]	[4.658]				[4.857]	[4.604]			

Note: For *S*2, *S*3 and *S*4 the reported estimates correspond to the 2012 mean capital stock and/or employment among participants.

On the one hand, smaller firms with low capital tend to invest more relative to their existing capital.[8] On the other hand, investment is also determined by the need to replace depreciating capital. Moreover, the extent to which the low interest rate on NHP loans changes the optimal level of capital can depend on the existing capital stock in a complex, nonlinear way. The estimation results suggest that employment, our alternative measure of firm size, also influences the program effect in ways not captured by the capital stock. Furthermore, there seem to be other relevant time varying factors related to future investment outcomes captured by the extra controls. As all these model features are theoretically plausible, and are apparently empirically relevant, we designate the richest model specification *S*4 as our preferred one and use it in all further exercises.

We will now turn to exploring the heterogeneity of the program effect as a function of firm size; more specifically, as a function of number of employees. (For most purposes, employment gives information about firms size in a more intuitive way than the capital stock.) We consider the standard size categories shown in Table 1 but split the medium category into two subcategories (50 to 149 and 150 to 249). Given a size category c, we take the 2012 mean capital stock $\bar{K}_c$ and mean employment level $\bar{L}_c$ for program participants in that group and measure capital stock and employment for each firm in the full sample in deviations from $\bar{K}_c$ and $\bar{L}_c$. Reestimating model *S*4 this way, $\widehat{\delta}-\widehat{\delta^*}$ gives the program effect evaluated at $\bar{K}_c$ and $\bar{L}_c$, and the corresponding standard errors are computed automatically. The results are reported in Table 3.

The general message of Table 3 is that the impact of the program is proportionally larger for smaller firms. For micro and small firms participating in the broader program, the average effect on the investment rate is 16.8 and 5.9 percentage points, respectively, while the effect practically disappears in the upper medium size category after applying the correction. Size heterogeneity is similarly marked for the narrower treatment. The average program effect starts at about 43% points for micro firms, drops to 14.3% points for small firms and to about 9% points in the medium size categories. While we do not have exact standard errors for the difference, the individual standard errors for $\widehat{\delta}$ and $\widehat{\delta^*}$ suggest that the estimates in the largest size category may well be insignificant statistically, particularly for the broader treatment.

[8] Smaller firms with a lower capital stock have a larger average investment *rate*, but with a much larger dispersion, too.

Table 3
Estimated average program effects by size category

Size category	Treatment=all NHP loans					Treatment=new investment loans				
	Parameter estimates (millions of HUF)			I/K impact (% point)		Parameter estimates (millions of HUF)			I/K impact (% point)	
	δ	δ^*	$\delta-\delta^*$	$\delta/\bar{K}$	$\frac{(\delta-\delta^*)}{\bar{K}}$	δ	δ^*	$\delta-\delta^*$	$\delta/\bar{K}$	$\frac{(\delta-\delta^*)}{\bar{K}}$
Micro (1-9)	22.71	1.032	21.7	17.6%	16.8%	37.43	7.705	29.7	54.0%	42.9%
	[2.648]	[3.562]				[3.156]	[2.033]			
Small (10-49)	28.92	10.75	18.2	9.3%	5.9%	63.13	23.83	39.3	23.0%	14.3%
	[3.612]	[4.332]				[5.184]	[4.820]			
Medium (50-149)	79.85	41.87	38.0	8.2%	3.9%	145.4	63.19	82.2	15.4%	8.7%
	[10.88]	[12.03]				[15.60]	[17.93]			
Medium (150-249)	85.49	67.71	17.8	5.5%	1.1%	222.4	88.71	133.7	15.1%	9.0%
	[28.90]	[32.94]				[28.32]	[52.13]			

Notes: Based on model *S4*. Estimates correspond to the 2012 mean capital stock and employment among participants in each category.

We provide an even more detailed picture of the proportional program effect as a function of size in Figure 4, where the size categories are defined in much finer increments (note that at higher employment levels the bins are wider). For both program definitions we see a monotone decreasing impact on the investment rate. Initially, the program effect falls at a fast but decreasing rate, and stabilizes at around 30 employees, on the 5-6% points level for the broad treatment, and the 10-15% points level for the narrow treatment (note that these values correspond fairly well to the overall averages). We then see a further drop to zero for the largest firms; here the estimated effects are not statistically different from zero (in particular, the uptick at the very large end for the narrow program definition is due to just a small number of firms). As mentioned already in Section 2.3, in Appendix C we present an informal regression discontinuity analysis to offer additional evidence on the vanishing treatment effect in the upper medium size category.

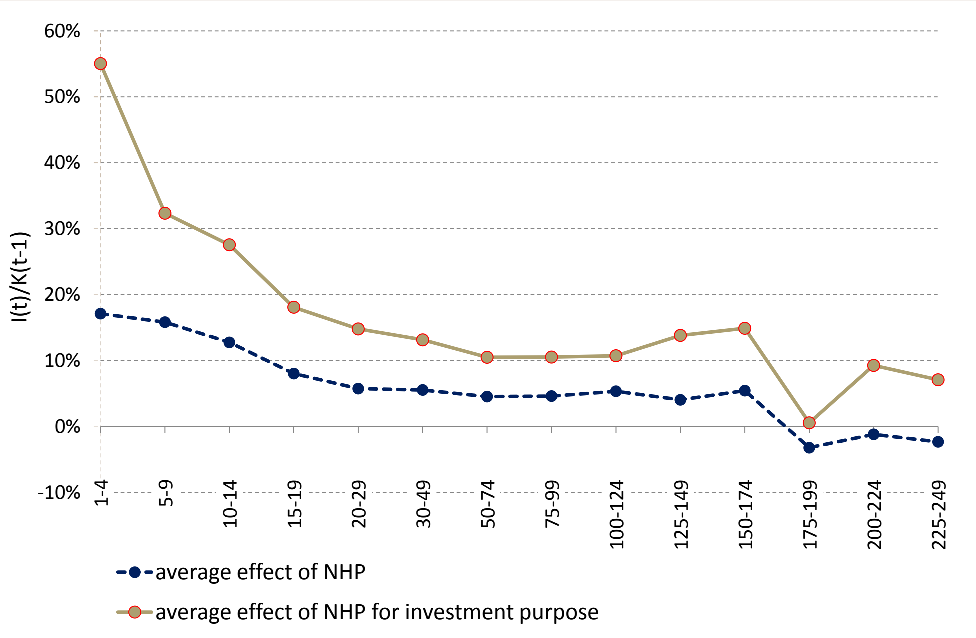
We conduct detailed robustness checks with respect to the time periods over which our models are estimated. These additional results are presented in Appendix D.1.

3.2 INTERPRETATION OF THE ESTIMATION RESULT

We will now interpret our estimation results in the context of the theoretical framework outlined in Banerjee and Duflo (2014, Section 3) and with the help of auxiliary data on borrowing patterns in the program. The exercise offers some insights into the state of small business finance in Hungary in 2013.

That the program has no effect on the largest SMEs suggests that these firms were not originally credit constrained and were likely rationed with respect to NHP loans. In other words, they used the program to (re)finance part of the *same* level of capital they would have financed from the market in full without the program. Thus, for them the program is effectively a transfer, at least in the short run (in the long run there may be indirect investment effects through balance sheet channels). Table 4 provides supplementary evidence for this story. In particular, columns (2) and (3) show the total value of all NHP loans taken out by participants as a fraction of their 2012 capital. In the two largest size categories the mean ratio across firms is 70% and 56%, while the median is 25% and 26%. This suggests that larger participants could not borrow enough under the program to

Figure 4
The effect of NHP on investment rate as a function of employment



equalize the marginal return on capital with the 2.5% cost (another, less likely, possibility is that they had limited sources of even cheaper finance which they did not want to substitute for).[9]

There are a couple of mechanisms that potentially contribute to the positive but decreasing proportional treatment effect for micro and small firms. Our first point, already mentioned in Section 2.3, is that firms that are constrained on the market will generally split a cheap program loan between refinancing existing capital and genuinely new investment, i.e., an expansion of the capital stock that would not have happened otherwise (a positive treatment effect). The flatter the credit supply curve a firm faces on the market, the smaller portion of a marginal program loan will initially go toward new investment. Thus, even if the NHP loan size to capital ratio were constant and less than unity, the decreasing treatment effect on the investment rate could be explained by larger firms being less credit constrained (i.e., the credit supply curve faced by a firm becoming flatter with size). The supplementary evidence in column (1) of Table 4 is consistent with this prediction. The ratio of NHP loans with the "new investment" label to loans with the "refinancing" label is as large as 128% for micro firms but falls below 50% in the medium category.

Nevertheless, columns (2) and (3) of Table 4 also show that the NHP loan to capital ratio does change with size, being much larger for smaller firms, many of which could apparently borrow well in excess of their pre-program capital stock.[10] Theoretically speaking, it seems that the smaller firms are sufficiently small so that they perceive the credit supply curve at the subsidized rate as completely flat. Such firms will refinance their entire pre-program capital stock and borrow extra funds up to the point where the marginal product of capital equals the cost. Once the pre-program capital is fully refinanced, all extra borrowing goes toward new investment. If the firm's demand for capital is sufficiently elastic, they may end up borrowing well in excess of

[9] The flat credit supply curve also implies that the nominal purpose of the NHP loan held by these firms is not important. Suppose that the total amount the firm can borrow under the program is HUF 1 million, a small amount relative to the firm's capital stock. With a flat supply curve it does not matter whether the firm uses the loan to refinance the "first" 1 million forint's worth of capital or the "last" 1 million, which is, say, due to a right-shift in capital demand in 2013, i.e., is nominally new investment. Figure 4 shows that for the largest participating firms the estimated treatment effect is indeed zero regardless of the definition of the treatment.

[10] The large difference between the mean and a median are caused by a few firms that borrowed very large amounts relative to their capital.

Table 4
Types of NHP loans by firm size

Firm size	New investment loans to refinancing loans (%)	Total NHP loans to capital (%)	
	Average	Average	Median
	(1)	(2)	(3)
Micro (1-9)	128	2691	60
Small (10-49)	82	1593	31
Medium (50-149)	49	70	25
Medium (150-249)	41	56	26

their pre-existing capital. Therefore, even if small firms were also unconstrained on the market, they would still exhibit a higher proportional treatment effect as they borrowed more relative to, and in excess of, their pre-program capital.

The fact that smaller firms were allowed by banks to borrow more relative to their capital is somewhat puzzling given that failure probabilities, and hence risk premia, are generally negatively correlated with firm size, and the 2.5% interest rate cap limits the risk premium banks can charge. It is possible therefore that smaller applicants are screened more selectively to fit this reduced margin. A small firm with a small default probability is then allowed to borrow well in excess of its original capital because the absolute loan size and the expected loss faced by the bank is still small.

In sum, the size related heterogeneity of the estimation results is consistent with the idea that smaller firms faced more severe constraints on the credit market which the program helped to eliminate. Note, however, that the second mechanism that operates through program loan to capital ratios in excess of unity is still at play even if all firms originally faced a completely flat credit supply curve on the market. Hence, our results are not entirely conclusive about the existence of pre-program credit constraints. More definitive statements could be made if we could observe how market borrowing by participating firms changes in 2013. Unfortunately, as of now it is not possible to link the Central Credit Registry with the NHP database in this year.

3.3 AGGREGATE EFFECTS: DECOMPOSING ACTUAL INVESTMENT

To gauge the aggregate macroeconomic impact of the program, we perform the following exercise. Using model *S*4, we construct an individual treatment effect estimate for each participating firm *i* in a given estimation sample by taking the difference between

$$\widehat{\delta} + \widehat{\beta}_1 K_i + \widehat{\beta}_2 K_i^2 + \widehat{\beta}_3 K_i^3 + \widehat{\lambda}_1 L_i + \widehat{\lambda}_2 L_i^2 + \widehat{\lambda}_3 L_i^3 \qquad (12)$$

and the corresponding terms in the corrective regression (here K_i and L_i are firm *i*'s 2012 capital and employment levels, respectively). Thus, we decompose the actual 2013 investment of NHP -firms into two parts: investment attributed directly to the program and residual investment, which would have been undertaken even in the absence of NHP.

The results of this exercise are collected in Table 5. As an upper bound for the aggregate program effect, we also report the decomposition based on the uncorrected DID estimate, i.e., (12) alone, without subtracting the correction terms. The corrected estimate of the absolute program effect is highlighted in bold. In the last two columns of Table 5 we compare this figure to the total investment volume of program participants and the SME sector (for the size breakdowns the corresponding subset is taken as the comparison group).

Taking the program as a whole, we find that about HUF 120 billion out of the HUF 610 billion in NHP loans allocated to the firms in the estimation sample was spent on genuinely new investment. This amounts to over 30% of all investment undertaken by participating firms, or a 6.8% increase in the investment volume of the SME sector as a whole. (As a rule of thumb, the SME sector in Hungary is responsible for about half of the total private investment in a year.) Size related treatment effect heterogeneity is again apparent. The proportion of program-induced investment is over 60% for micro sized firms (HUF 54 billion out of 85 billion), while it drops to 20% in the lower-medium category (HUF 24 billion out of 116 billion). As estimates

Table 5
Decomposition of the actual investment of participating firms

		Investment (billions of HUF)				Relative impact of NHP	
		DID		DID*		as % of all investment by:	
	actual	w/o NHP	due to NHP	w/o NHP	due to NHP	participants	SME sector
PANEL A: Treatment = All NHP							
All firms	387.8	204.5	183.3	268.3	**119.5**	30.8%	6.8%
Micro (1-9)	84.7	28.2	56.6	31.1	**53.7**	63.3%	3.1%
Small (10-49)	155.8	86.9	68.9	112.4	**43.4**	27.8%	2.5%
Medium (50-149)	115.6	63.3	52.3	91.5	**24.1**	20.8%	1.4%
Medium (150-249)	30.1	24.2	5.9	31.3	**-1.2**	-4.1%	-0.1%
PANEL B: Treatment = NHP for investment							
All firms	225.0	82.3	142.7	133.0	**92.0**	40.9%	5.3%
Micro (1-9)	57.4	13.8	43.6	22.9	**34.5**	60.1%	2.0%
Small (10-50)	88.4	32.2	56.3	54.0	**34.5**	39.0%	2.0%
Medium (50-149)	59.7	26.5	33.2	42.2	**17.5**	29.3%	1.0%
Medium (150-249)	17.4	7.7	9.7	11.6	**5.7**	33.1%	0.3%

Notes: Based on model S4. All absolute numbers are in billions of 2013 HUF. DID denotes the decomposition results based on the uncorrected DID estimator, giving an upper bound on the program effect. DID* denotes the decomposition results for the corrected estimator.

of the average effect have already shown, the program does not meaningfully stimulate the investment activity of the largest participants. (The small negative point estimates are unlikely to be statistically significant.)

Loans for nominally new investment projects amounted to about HUF 175 billion in our sample. Narrowing the treatment definition to firms with such loans (panel B of Table 5), we see that the volume of program induced investment is about HUF 92 billion, which is about 41% of all investment undertaken by these firms (HUF 225 billion in total). The same share is again much larger for micro firms (60%), and it drops by firm size, but does not disappear even in the largest size category. Nevertheless, the roughly 33% program-induced investment share for these firms is based on rather small absolute numbers, which could be noisy.

The results also demonstrate that the majority of new investment attributed to the program, HUF 92 billion out of 120 billion, is undertaken by firms that took out investment loans. This suggests that the indirect investment impact of loans with a different nominal purpose is rather weak, at least in the short run. Of course, with the narrower definition the program's contribution to the overall investment activity of the SME sector is somewhat smaller (5.3% overall).

These figures allow us to give a back-of-the-envelope estimate of the direct, "accounting" impact on GDP of the first phase of NHP whilst ignoring any (or most) multiplier effects.[11] In particular, given the estimated $6.8/2 = 3.4$ percent positive effect on total private investment, the investment share of GDP, and the fact that about half of total investment is imports, we estimate that the program gave a 0.2% boost to GDP. This impact took place already in 2013, over no more than a six month horizon. On the other hand, these calculations still assume that all positive investment appearing on firms' balance sheets is an addition to the aggregate capital stock, which is rather optimistic.

[11] In principle, the estimated program effect might already contain some indirect multiplier effects.

4 Discussion and conclusions

By and large, our numerical results show that the Magyar Nemzeti Bank's Funding for Growth Scheme succeeded in what was presumably one of its main goals—it generated investment that would not have happened otherwise. A particularly robust feature of our findings is that the program effect is heterogeneous with respect to firm size with a larger proportional effect for smaller firms. On average, we attribute about 30% of the total investment undertaken by participating firms in 2013 to the program, but this ratio is much larger for micro firms (60%) and is practically zero among upper medium-sized firms. From a slightly different perspective, additional investment induced by the program is estimated to boost the average investment rate by about 17% points among micro firms, 6% points among small firms, 4% points among lower medium-sized firms and 1% points among upper medium-sized firms (technically, this last estimate is not significantly different from zero). It seems that the largest participating firms simply executed their already existing investment plans for 2013—only cheaper, thanks to the program. This suggests that they were facing an essentially flat aggregate credit supply curve on the market (i.e., were not credit constrained), but were rationed with respect to the cheaper program loans.

More generally, the size-related heterogeneity of the proportional treatment effect is consistent with the idea that smaller firms *were* credit constrained in the sense of facing a steeper, upward-sloping aggregate credit supply curve, and that the program helped eliminate these constraints. However, there are alternative explanations for the observed pattern that do not say anything about this issue. Thus, no definite conclusions on this question follow from the evidence presented in this paper. Linking the 2012-2013 Central Credit Registry to the NHP database would be particularly informative in this regard, but it is not legally feasible at the moment.

On the aggregate level, we estimate that the first phase of NHP generated about 6.8% extra investment in the SME sector, and 3.4% in the private sector over a six month horizon. With some caveats, this translates into a direct GDP effect of 0.2% already in 2013.

While our results suggest that the first phase of the Funding for Growth Scheme produced 'external' benefits through increased investment, a careful cost-benefit analysis is definitely beyond the scope of the paper. Nevertheless, a few considerations that such an analysis would require are helpful for putting the results further into perspective.

First, as explained in the introduction, positive investment at the firm level does not necessarily correspond to the creation of new capital in the aggregate economy—it can also be generated by a reallocation of the existing capital stock. (Such reallocation may still have an effect on aggregate productivity.) Second, we do not have good qualitative information about the type of investment projects undertaken; e.g., purchasing new machinery versus repainting the office. Third, we do not yet know how the default rate on program loans is going to compare to default rates on market loans. Fourth, this study has no implications about the opportunity cost of the program. Fifth, analysis of the benefit side could also be extended to include, say, employment effects.

In light of these caveats, there are several relevant directions for further research in the evaluation of the program. Using the methodology of this paper, one could also estimate the program effect on outcomes such as employment or sales. A well-executed randomized survey could also deliver important information about the type of investment projects undertaken. Subsequent years will also reveal more about dynamic and long term outcomes, including default rates, though any data collected will also reflect the additional impact of the second phase of the program.

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Appendix

A HISTORICAL INVESTMENT OUTCOMES OF NHP AND NON-NHP FIRMS

Figure A.1
Mean investment volume (millions of 2013 HUF)

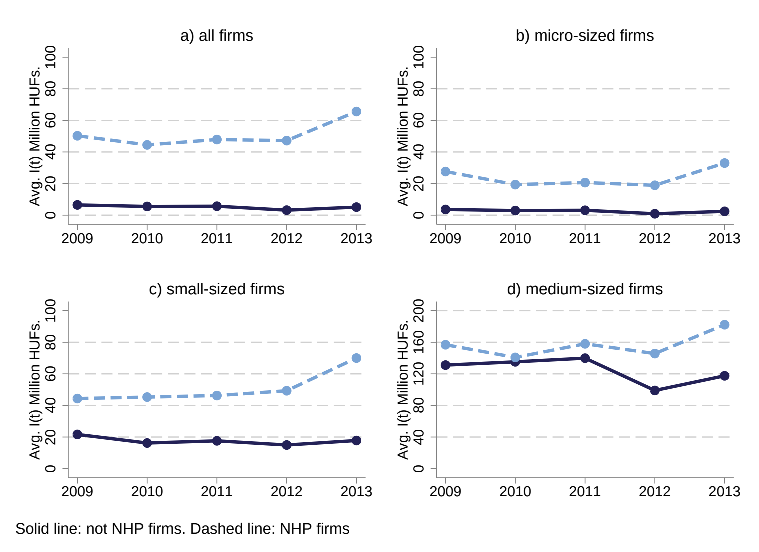
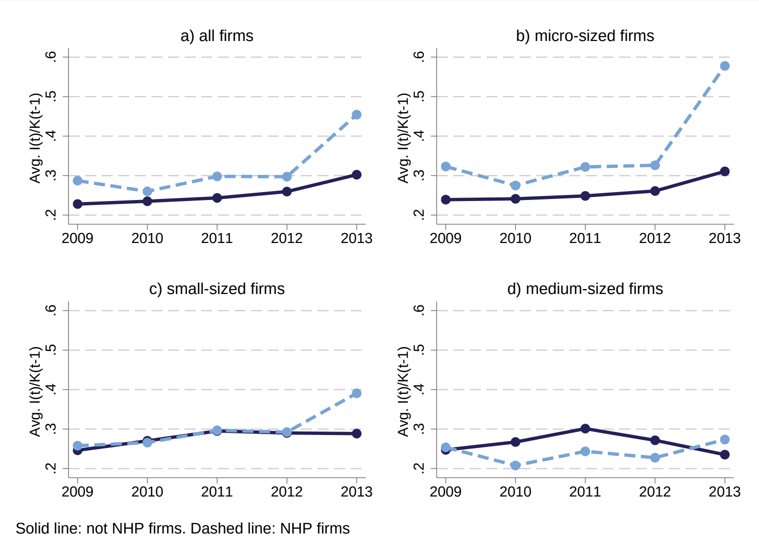


Figure A.2
Mean investment rate



B DATA APPENDIX

B.1 DATA SETS AND MATCHING ISSUES

The firm level database used in this study is provided by the National Tax and Customs Office (NAV, formerly APEH). It contains balance-sheet information on double-entry book-keeping companies subject to corporate taxation. The source of the data are the corporate tax reports. Our data gives full coverage of all firms from 2007 to 2012 and partial coverage of 2013.[12]

Table B.1.1
Share of identified NHP participants by program in 2013 (amounts in Bn. HUFs)

	Investment		EU pre-finance		Working capital		Refinance	
	N.obs.	Amount	N.obs.	Amount	N.obs.	Amount	N.obs.	Amount
TOTAL	3231	175.4	53.0	1.3	2131	113.4	2755	410.5
Not coprorated (1)	576	10.6	12	0.2	162	2.5	151	5.5
Corporations (2)	2655	164.8	41	1.1	1969	111.0	2604	404.9
Identified corporations in dataset (3)	2560	159.8	40	1.1	1914	108.9	2540	396.3
Sample for S4 (4)	2332	109.6	37	0.9	1875	107.1	2415	373.5
Micro (1-9)	1129	35.6	19	0.3	747	17.5	873	117.3
Small (10-49)	914	44.3	14	0.4	828	44.1	1048	115.6
Medium (50-149)	248	24.0	4	0.2	260	35.7	422	103.7
Medium (150-249)	41	5.7	0	0.0	40	9.7	72	36.8
share of identified (=3/2)	96%	97%	98%	100%	97%	98%	98%	98%
share of identified in S4 (=4/2)	88%	66%	90%	84%	95%	96%	93%	92%

The total amount of the NHP program is the aggregate of investment (175.4 Bn HUFs), EU pre-finance (1.3Bn HUFs), current assets (113.4 Bn HUFs) and refinancing (410.5 Bn HUFs) programs, totaling 700.1 Bn HUFs.

The firm level balance sheet data is matched with information on NHP -participation, including the type and amount of the loan taken out, supplied to MNB on a mandatory basis by mediating financial institutions. As the NHP scheme was also available for non-incorporated economic agents (e.g. self-employed) we are only able to identify a fraction of the participants. Table B.1 gives an overview of the data by the aim of the loan. For example, in the case of the new investment, out of the 175.4 billion HUFs underwritten, 10.6 billions (about 6%) are with not incorporated agents. For working capital financing and refinancing the corresponding figures are 2 and 1 percents. At the same time, we uncover minor discrepancies when matching NHP information of incorporated agents with the balance sheet data. Depending on the program, we are not able to match 2-4 percent if the observation and underwritten values in the case of incorporated firms.[13]

Given that the NHP was targeted at the small- and medium sized enterprises we keep only firms in the sample that satisfy the necessary criteria. In Table B.1 we relate the firms participating in NHP for any purpose and who took out loans for new investment to the full sample of firms by firm-size. The statistics for 2013 show that the number of firms in our sample is over 400 thousand, the majority (79%) of which are micro-sized enterprises, while the minority (6.5%) is small and medium sized. Unfortunately, we lack the size information for some firms and could not categorize them.[14]

[12] As writing this paper, we do not yet have information on firms with non-standard accounting years, nor do we observe those who are self-correcting/finalizing their tax-report. The finalized data will be available only in late 2014 or early 2015. Nevertheless, non-standard accounting year is typically chosen by large multinationals out of the scope of NHP. Thus, we hypothesize a possible bias to be minuscule.

[13] Discrepancies can arise for multiple reasons. First, the 2013 balance sheet data is not finalized and does not cover all firms. Other issues include mistyped identifiers or consolidated balance-sheets.

[14] We have tried to improve size statistic availability by imputing employee data from nearby year for a firm-year observation in the data. After, we also imputed employment statistics with regression models using sectoral average wage and total payroll as explanatory variables. The figure in Table B.1 and B.1 and on not available size category reflects figures resulting from the aforementioned efforts.

Table B.1.2
Sample of firms and NHP participants in 2013 (amounts in Millions of HUFs)

Size of firms	Total num. of firms	identified in sample			sample of model S4		
		No. of firms	% in total	avg. amount	No. of firms	% in total	avg. amount
NHP for all purposes							
Micro (1-9)	306,633	4106	1.3%	88.7	3878	1.3%	0.9
Small (10-49)	33,742	3734	11.1%	105.2	3704	11.0%	8.6
Medium (50-149)	3,763	1187	31.5%	263.6	1178	31.3%	52.6
Medium (150-149)	738	195	26.4%	518.1	194	26.3%	84.0
n.a.	55,792	347	0.6%	389.1		0.0%	
all	400,668	9574	2.4%	134.2	8959	2.2%	2.6
NHP for investments							
Micro (1-9)	306,633	1220	0.4%	18.3	1129	0.4%	13.9
Small (10-49)	33,742	925	2.7%	18.9	914	2.7%	18.6
Medium (50-149)	3,763	250	6.6%	33.6	248	6.6%	33.8
Medium (150-249)	738	41	5.6%	51.1	41	5.6%	51.6
n.a	55,792	123	0.2%	158.1		0.0%	
all	400,668	2560	0.6%	26.0	2333	0.6%	19.0

The Table describes our sample of firms in 2013 and describes the distribution of the general NHP program participation and average loan-size by firm-size in the left block.In the right block the analogous descriptive statistics are presented for the NHP new investment scheme.

B.2 VARIABLES

Dependent variables We use two dependent variables. First, real investment which we define as: $I_t = IPI_{2013} \times (DEPR_t + \Delta(FA + IMMAT))$, where FA and IMMAT are the end of the year stock of fixed- and immaterial assets, respectively. DEPR is the depreciation amount for year t and Δ is the first-difference operator. IPI_{2013} is a NACE rev.2. 2 digit sector level investment price index that is used to calculate real values of investments in 2013 terms. For new born firms $(FA + IMMAT)_{t-1}$ is considered zero.

Second, we use investment rate: I_t/K_{t-1}, where K_{t-1} is lagged real capital. The real capital is calculated as the values $FA+IMMAT$ for each year deflated by IPI_{2013}. This simple method has the advantage that it can provide a good denominator to assess relative size of loans in NHP.

The distributions of both investment and investment rate have extremely fat tails. In order to avoid the results to be driven by outliers we dropped observations where real investment is higher than HUF 10 billion or disinvestment is lower than minus HUF 10 billion.[15] In the case of investment rate we dropped observations that are higher than the 95th percentile or lower than the first percentile of the investment rate distribution in the given year. See Table B.2 for descriptive statistics on investment and investment rate.

[15] In 2013 this affects 14 out of more than 300 thousand firms.

Table B.2.1
Real investment and investment rate in 2012-2013

	I_t		I_t/K_{t-1}	
t	mean	median	mean	median
2012	6.935	0.07	28%	4%
2013	8.047	0.11	32%	6%

Real investment is expressed in million HUFs in 2013 terms. Statistics are calculated for SME firms only.

Control variables To control for access to finance and selection to NHP and inveswe use firm level financial and economic indicators: equity, total assets, leverage, leverage share, return on assets, collateral, export share, liquidity. The definitions and descriptive statistics for these variables are collected in Table B.2.2. It provides descriptive statistics for the SME sector, for the NHP participants and for NHP new investment firms. Statisitics reveal that NHP participants on average are larger in terms of assets and equity, have lower leverage and are more profitable.

Table B.2.2
Firm level financial control variables in 2013

		all firms		NHP firms		NHP new invest.	
variable	calculated as	mean	s.d.	mean	s.d.	mean	s.d.
equity	equity (mill. HUFs)	120	11500	320	636	297	622
assets	log of total assets (log of mill. HUFs)	8.7	2.4	12.4	1.5	12.2	1.5
leverage	long+ short liabilities / total assets (%)	100%	154%	55%	28%	53%	26%
short leverage	short /(short+long) liabilities (%)	87%	28%	61%	27%	59%	27%
return on assets	profits / assets (%)	-36%	203%	7%	14%	8%	12%
collateral	fixed assets / (fixed assets + immat.) (%)	1%	6%	1%	4%	1%	4%
export share	share of exports in sales (%)	24%	42%	8%	21%	9%	23%
liquidity	current assets / total assets (%)	73%	32%	47%	26%	42%	23%

Notes: This Table lists the control variables used in equation S4. In columns (2) the definition of each variables is provided along with the unit of measurement (in brackets). In the remaining columns descriptive statistics, mean and standard deviation, are provided for three samples: the population of SME firms, NHP participants and NHP participants with new investment loans. Leverage, short leverage, collateral and export share variables are winsorized to be between zero and one.

B.3 ESTIMATION SAMPLES

Due to missing observations on dependent or control variables, outlier control, and the firm creation/destruction process, we cannot use all SMEs in the NAV database to estimate our models. Furthermore, different specifications are estimated on somewhat different samples. Table B.3.1 summarizes the sample sizes used in estimating the models reported in this paper.

Table B.3.1
Sample sizes

	Treatment=all NHP loans		Treatment=new investment loans	
	δ	δ^*	δ	δ^*
s1	653,419	673,791	649,902	673,791
s2	628,033	602,128	624,521	602,128
s3	597,952	578,796	594,520	578,796
s4	542,677	534,219	539,252	534,219

C ADDITIONAL EMPIRICAL EVIDENCE ON THE CORRECTION PROCEDURE

C.1 REGRESSION DISCONTINUITY ANALYSIS

Here we consider investment volume as a function of the number of employees in a neighborhood of the exogenous program eligibility cutoff ($c = 250$ employees). As participation for firms below the cutoff is voluntary and firms above the cutoff are ruled out, the relevant design is fuzzy regression discontinuity with one-sided non-compliance (see, e.g., Imbens and Lemieux 2008). Denoting the number of employees by L, ATT for firms with $c = 250$ employees is identified by[16]

$$\lim_{\Delta \to 0} \frac{E[Y_{13} \mid c - \Delta \leq L \leq c] - E[Y_{13} \mid c \leq L \leq c + \Delta]}{E[P \mid c - \Delta \leq L \leq c]}. \tag{13}$$

In Table C.1.1 we show estimates of the numerator of (13) for various choices of Δ. (We also use neighborhoods of c that are not symmetric around c, we consider firms with not more than 300 employees.) In each case we restrict the sample to firms with number of employees falling into the given neighborhood, and regress 2013 investment volume on a constant and the dummy variable $Z = 1(L \leq c)$. Thus, the estimated slope coefficient is the average investment of firms below the cutoff minus the average of firms above the cutoff, restricted to the given neighborhood. Due to relatively small sample sizes, outliers have a particularly large effect on this exercise. Therefore, we drop firms with very large absolute investment volumes; we report results for cutoffs equal to HUF 5 billion and 1 billion, respectively.

Table C.1.1
Estimates of the numerator of eq. (13)

Outlier cutoff	5 billion			1 billion		
Empl. range	220-280	200-300	170-300	220-280	200-300	170-300
≤ 250 dummy	-324.7^{**}	-282.0^{***}	-275.2^{***}	−20.46	−23.58	−37.62
	[141.5]	[99.06]	[100.0]	[35.02]	[26.70]	[24.59]
Const.	528.0^{***}	508.6^{***}	508.6^{***}	205.3^{***}	206.3^{***}	206.3^{***}
	[135.0]	[94.68]	[94.60]	[29.21]	[22.38]	[22.36]
Obs.	258	437	694	237	404	641
R^2	0.028	0.027	0.017	0.002	0.002	0.004
Part. rate	12.5%	16.40%	14.70%	12.7%	16.66%	14.97%

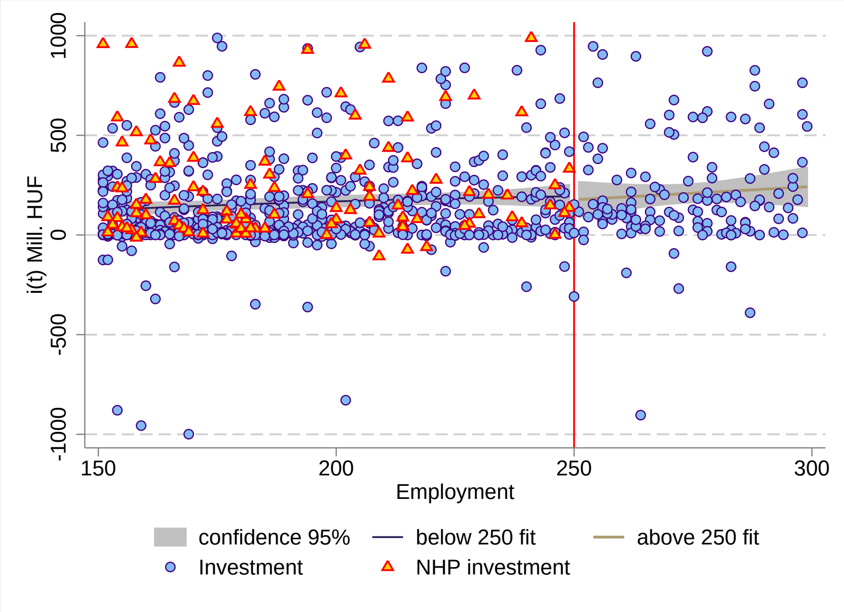
Note: The coefficient estimates are in millions of 2013 HUF. Participation rate, the denominator of (13) is the number of NHP firms divided by the number of firms in the sample below the cutoff.

As seen in Table C.1.1, the estimated slope coefficients are negative throughout, which would correspond to a negative program effect. The statistically significant and large negative values in the left panel are due mostly to a handful of firms with over 250 employees that invested in excess of 1 billion. Once these firms are dropped, the difference between the two sides of the cutoff becomes much smaller and statistically insignificant. Thus, there is absolutely no evidence that average investment just below the cutoff is systematically larger than average investment just above the cutoff.

Figure C.1.1 visually reinforces this finding. Here the depicted firms are in the [150,300] employee range, covering the whole upper medium sized category. Absolute investment volume is capped at HUF 1 billion. NHP firms are denoted by triangles, while non-NHP firms are dots. When investment is regressed on employment separately on both sides of the cutoff, there is no evidence whatsoever of a significant discontinuity between the two regression lines. (Indeed, participating firms seem to be mixed in evenly with the rest.)

[16] More generally, for a small $\Delta > 0$, the numerator of (13) divided by $E[P \mid c - \Delta \leq L \leq c] - E[P \mid c \leq L \leq c + \Delta]$ gives the average treatment effect for the subpopulation of firms that (i) comply with the instrument $Z = 1(L \leq c)$; (ii) and have employees in the $[c - \Delta, c + \Delta]$ range. This is a conditional version of the well-known LATE parameter of Angrist et al. (1996). However, because only firms below the cutoff are eligible for the program (one sided non-compliance), and Z can be regarded as completely random in small neighborhoods of c, LATE coincides with ATT (see, e.g., Donald et al. 2014).

Figure C.1.1
Investment by firms in a neighborhood of the 250 employee cutoff



C.2 COMPARISON OF MARKET BORROWERS AND PROGRAM PARTICIPANTS

In Table C.2.1 we compare firms that took out a new market loan in 2011 with NHP participants. More specifically, we compare the (marginal) distribution of a number of investment-relevant covariates across the following groups and years (note that we use lagged values of firm characteristics): (i) year 2010 values for firms that took out any type of NHP loan in 2013; (ii) year 2012 values for the same group of firms; (iii) year 2010 values for firms that took out a new market loan in in 2011; (iv) year 2010 values for firms that took out program loan for "new" investment; (v) year 2012 values for the same group of firms; (vi) year 2010 values for firms that took out a long term market loan in 2011. The variables examined are log assets, leverage, export share, return on assets, liquidity ratio, and proportion of assets that can serve as collateral (see Table B.2.2 for more precise definitions).

By and large, Table C.2.1 shows that NHP participants in 2013 and market borrowers are reasonably similar. There are some differences in terms of leverage (NHP participants are somewhat less levered); liquidity (NHP participants are somewhat less liquid); and return on assets (NHP participants have somewhat higher returns). All these characteristics are explicitly controlled for in model *S*4.

Table C.2.1
Some observed characteristics of NHP participants and market borrowers

	NHP (all) 2011	NHP (all) 2013	New loan 2011	NHP (inv.) 2011	NHP (inv.) 2013	New lt. loan 2011
log assets						
mean	12	12.2	10.9	11.6	11.9	11.3
s.d.	1.8	1.6	1.8	1.9	1.8	2.0
p25	10.9	11.1	9.7	10.5	10.7	10.0
p50	12.1	12.3	10.8	11.8	11.9	11.3
p75	13.3	13.4	12	13	13.1	12.6
leverage						
mean	54.20%	51.4%	57.70%	49.00%	45.2%	60.00%
s.d.	35.10%	27.6%	39.30%	38.40%	28.9%	42.10%
p25	33.40%	31.9%	35.20%	26.60%	24.5%	36.70%
p50	51.60%	50.0%	56.10%	44.80%	42.1%	57.90%
p75	72.10%	69.8%	75.90%	65.80%	63.2%	78.30%
export share						
mean	8.30%	7.4%	6.80%	8.40%	7.6%	9.00%
s.d.	21.80%	19.1%	20.70%	22.30%	20.1%	24.20%
p50	0%	0%	0%	0%	0%	0%
p75	1.90%	2.4%	0.00%	1.00%	1.5%	0.40%
return on assets						
mean	6.70%	8.9%	6.40%	7.70%	10.8%	4.60%
s.d.	24.10%	17.0%	39.30%	22.20%	23.6%	41.10%
p25	2.20%	2.7%	2.20%	2.10%	3.3%	1.50%
p50	5.70%	6.4%	5.90%	6.20%	8.0%	5.30%
p75	11.10%	12.5%	12.70%	12.90%	15.9%	11.50%
liquidity ratio						
mean	51.20%	50.9%	61.50%	52.30%	52.5%	57.10%
s.d.	28.10%	27.6%	28.50%	27.30%	26.1%	29.50%
p25	29.50%	29.8%	39.00%	31.40%	32.6%	33.10%
p50	50.30%	50.3%	64.60%	50.40%	51.2%	57.50%
p75	73.90%	72.5%	87.30%	74.00%	72.1%	83.80%
collateral as % of total assets						
mean	0.80%	0.9%	1.00%	0.70%	0.7%	1.00%
s.d.	4.20%	4.6%	5.40%	3.60%	3.7%	5.30%
p50	0%	0%	0%	0%	0%	0%
p75	0.20%	0.20%	0.10%	0.10%	0.10%	0.10%

D SENSITIVITY ANALYSIS

D.1 ESTIMATION PERIODS

Here we present several tables that show how the estimation results change for models *S*1 through *S*4 if we vary the years included in the estimation period. In addition to the reported baseline of 2013-12, for the basic DID regressions we consider 2013-2011, 2013-2010, and 2013-2009. For the corrective regressions the baseline is 2011-2010; the alternative periods considered are 2010-2009, 2009-2008, 2011-2009, and 2011-2008. Take, for example, line *S*4 in Table D.1.1, which shows estimation results for the broad program definition. We see that there is considerable variation in $\widehat{\delta^*}$; e.g., it is almost double the baseline for the period 2010-2009. Nevertheless, adding just the year 2009 to the baseline period (2011-2009) does not change the correction term by much. Subtracting the smallest $\widehat{\delta^*}$ from the largest $\widehat{\delta}$ for model *S*4 gives roughly HUF 30 million as the average treatment effect; at the other extreme, we obtain HUF 10 million as a lower bound. The chosen benchmark specification splits this difference right down the middle. In general, we are rather wary of including the crisis years 2008 and 2009 in the estimation sample for the correction term. This is a major reason why we decided to opt for 2011-2010 as the benchmark period here. Given this choice, we also opted for a two-year period for the DID regressions, leading to 2013-2012 as the benchmark.

Table D.1.1
Robustness of coefficient estimates to choice of estimation period: NHP

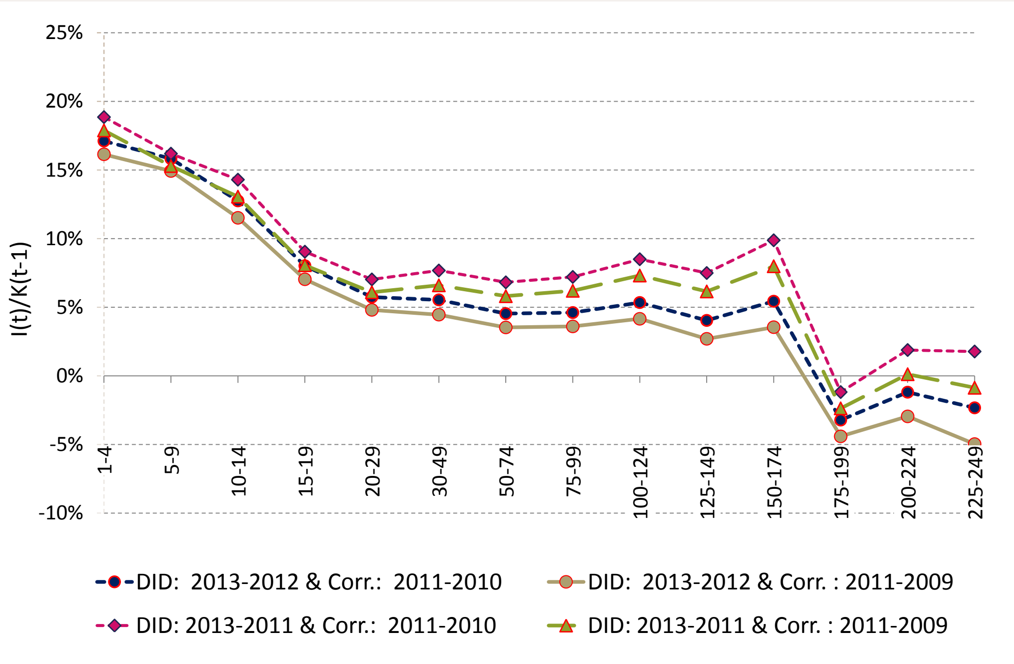
	equations for δ				equations for δ^*				
	sample periods				sample periods				
model	2013-2012	2013-2011	2013-2010	2013-2009	2011-2010	2010-2009	2009-2008	2011-2009	2011-2008
s1	20.10***	20.60***	22.05***	21.56***	2.088	2.165	1.715	2.015	1.946
	[2.270]	[2.097]	[2.024]	[2.021]	[1.407]	[1.651]	[1.617]	[1.610]	[1.437]
s2	47.63***	46.85***	43.22***	47.49***	10.81*	28.06***	21.92***	4.365	-3.192
	[3.861]	[4.786]	[4.492]	[4.688]	[5.770]	[6.173]	[7.117]	[7.284]	[7.363]
s3	40.09***	39.28***	35.61***	38.39***	13.29**	23.17***	13.25**	10.94**	6.536
	[3.985]	[5.077]	[4.664]	[4.564]	[5.808]	[5.705]	[5.785]	[5.520]	[5.594]
s4	30.19***	34.96***	38.12***	38.43***	11.12**	20.45***	13.44***	14.15***	19.14***
	[3.827]	[4.287]	[4.332]	[4.545]	[4.658]	[5.921]	[4.315]	[4.802]	[5.642]

Table D.1.2
Robustness of coefficient estimates to choice of estimation period: NHP for investment

	equations for δ*				equations for δ*				
	sample periods				sample periods				
model	2013-2012	2013-2011	2013-2010	2013-2009	2011-2010	2010-2009	2009-2008	2011-2009	2011-2008
s1	57.19***	57.85***	60.05***	60.08***	8.413***	9.473***	3.586*	10.92***	10.15***
	[3.323]	[3.071]	[3.045]	[3.013]	[3.117]	[2.461]	[2.072]	[2.988]	[2.881]
s2	77.99***	77.71***	75.23***	77.28***	24.05***	30.32***	25.78***	30.09***	27.13***
	[5.496]	[6.092]	[5.643]	[5.602]	[4.442]	[4.330]	[5.211]	[4.042]	[4.425]
s3	70.51***	69.85***	67.78***	69.26***	27.46***	19.22***	13.85***	31.77***	29.54***
	[4.815]	[5.549]	[5.039]	[5.005]	[6.034]	[4.170]	[4.494]	[5.837]	[5.811]
s4	60.59***	62.54***	65.33***	65.03***	22.94***	14.45***	10.80***	27.20***	30.40***
	[4.857]	[5.305]	[5.201]	[5.334]	[4.604]	[5.033]	[4.012]	[4.992]	[5.245]

At first glance, $\widehat{\delta}$ and $\widehat{\delta^*}$ appear rather sensitive to the choice of the estimation period. Nevertheless, this does not translate to substantially different conclusions regarding the proportional program effect as a function of size. In Figure D.1 we display four different estimates corresponding to different choices of the two estimation periods. As can be seen, the difference between the highest and lowest running lines can reach as much as 5 percentage points (or even more) at higher employment levels, where there are relatively few observations. There is much less variation for micro and small firms, where the number of observations is much larger. Importantly, however, all four lines tell the same basic story about the effectiveness of the program. In sum, it appears that most of the sample-choice variation in the estimated average effects is due to the fact that there are relatively few observation in the (upper) medium size category.

Figure D.1.1
Sensitivity of the proportional program effect to the choice of the estimation period



Finally, we present robustness checks for the aggregate results derived from model *S*4. Table D.1 displays the estimated aggregate effect of the broad program for every possible combination of the various estimation periods for $\widehat{\delta}$ and $\widehat{\delta^*}$. Again, at first glance there appears to be a lot of variation, from HUF 61 billion to HUF 155 billion, but if one drops the two smallest outliers, the range of the estimates reduces quite substantially, and the reported benchmark effect (HUF 119.5 billion) is roughly the average of the remaining figures. Furthermore, if we focus attention on the first column, where the estimation period for the corrective regression does not contain crisis years, the variation reduces to HUF 120-155 billion, which is a very tolerable range. Table D.1 shows the corresponding figures for the narrow treatment definition. As can be seen, here the variation in the estimated aggregate effect is reassuringly small altogether.

D.2 SECTORAL HETEROGENEITY

We also briefly examine the sectoral heterogeneity of the treatment effect by including sector interactions into model *S*4. While some of these interactions turn out to be statistically significant, Table D.2 shows that the estimated aggregate program effect changes (drops) only by a relatively small amount after adding this model feature. However, grouping the firm level decompositions by sector reveals that the two models are not always in full agreement. As shown in Figure D.2, allowing for sectoral heterogenity in the broadly defined treatment effect significantly reduces the estimated share of program-induced investment in the agricultural sector (for the other eight sectors there are just small changes in the results). For the narrow treatment definition, significant reductions in the estimated program effect can be seen in the retail/wholsale and construction sectors. In sum, exploring sectoral heterogeneity in more detail appears to be a relevant direction for future research, but leaving it unmodeled does not seem to cause significant bias in our results.

Figure D.2.1
Sensitivity of the proportional program effect to the choice of the estimation period

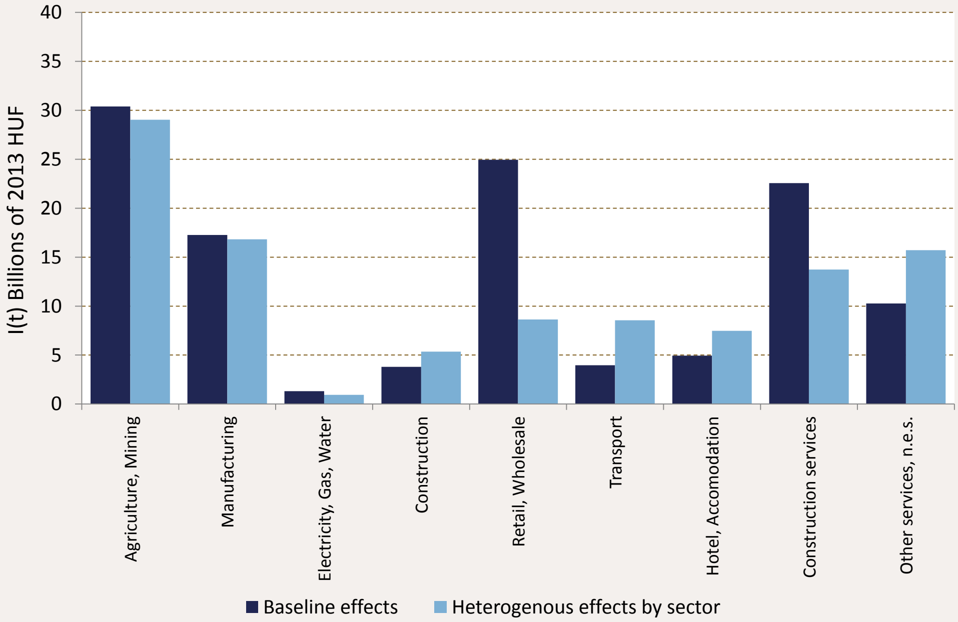


Figure D.2.2
Sensitivity of the proportional program effect to the choice of the estimation period

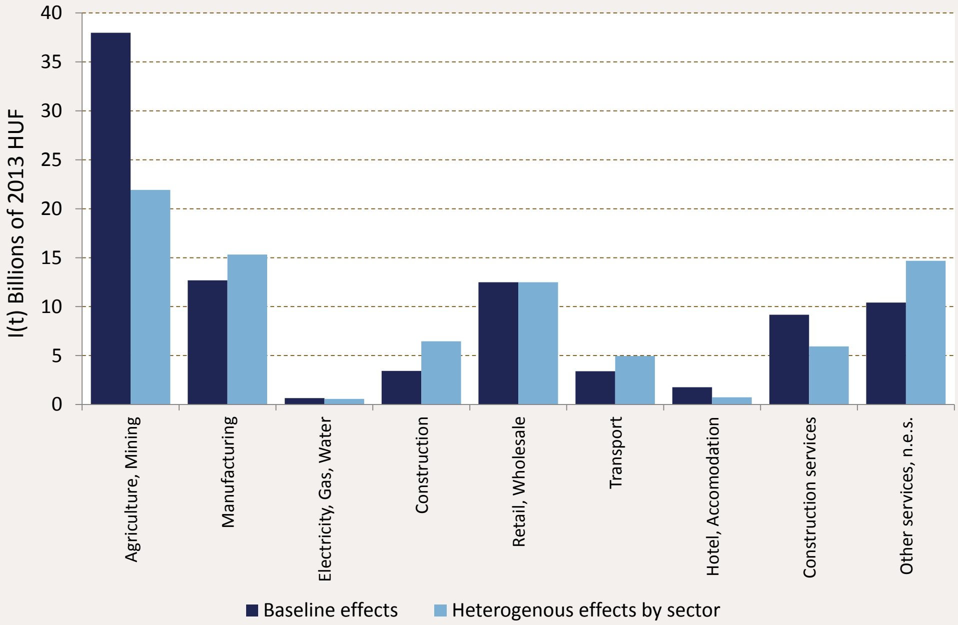


Table D.1.3
Model S4: robustness of aggregate effect in billions of HUF: NHP

	Correction				
DID	2011-2010	2010-2009	2009-2008	2011-2009	2011-2008
2013-2012	119.52	60.97	86.90	99.76	71.90
2013-2011	152.73	94.19	120.11	132.97	105.11
2013-2010	154.96	96.41	122.33	135.19	107.33
2013-2009	154.37	95.82	121.74	134.60	106.74

minimum value is 60.97, maximum is 154.96

Table D.1.4
Model S4: robustness of aggregate effect in billions of HUF: NHP for investment

	Correction				
DID	2011-2010	2010-2009	2009-2008	2011-2009	2011-2008
2013-2012	91.98	90.84	100.48	82.12	73.62
2013-2011	100.39	99.25	108.89	90.53	82.03
2013-2010	103.35	102.21	111.85	93.49	84.99
2013-2009	102.72	101.58	111.21	92.85	84.35

minimum value is 73.62, maximum is 111.85

Table D.2.1
Robustness: Model S4 with heterogenous firm effects

	actual	w.o. NHP	due to NHP	% of actual
Treatment = all NHP loans				
S4	387.8	268.3	119.44	30.8%
S4 w/ heterogeneous sector effects	387.8	281.5	106.26	27.4%
Treatment = investment NHP loans				
S4	225.0	133.0	92.0	40.9%
S4 w/ heterogeneous sector effects	225.0	141.9	83.1	36.3%

MNB Working Papers 2

The Impact of the Magyar Nemzeti Bank's Funding for Growth Scheme on Firm Level Investment

Budapest, April 2015