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Profit-Sharing and Wages: An Empirical Analysis Using French Data Between 2000 and 2007

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Abstract

Economic theory presents two main views on the effect of profit-sharing on wages. First, profit-sharing may substitute for base wages and have a neutral effect on total compensation. Second, it may be interpreted as an "efficiency wage" that increases total compensation. Existing empirical literature does not allow a determination of which of these two arguments is valid. This paper attempts to tackle this issue in the case of France for the 2000-2007 period. Based on a difference-in-differences selection model, our results suggest that profit-sharing has a neutral effect on total compensation. Several years after its implementation within firms, profit-sharing lowers base wages, which are offset by profit-sharing bonuses.

I. Introduction

In the literature, there are two different views of the effect of profit-sharing on wages, both of which claim that profit-sharing systems enhance firm performance. The first view argues that profit-sharing bonuses substitute for base wages (i.e., the fixed part of total compensation) and consequently have a neutral effect on total compensation (Weitzman 1984, 1985). The second view argues that profit-sharing increases labour productivity by enhancing workers' effort and cooperation (Kruse 1993; Kruse et al. 2010) or by retaining the most qualified and productive employees (Long & Fang 2012). A profit-sharing bonus is thus interpreted as an "efficiency wage" that adds to the base wage. Numerous empirical studies show that profit-sharing induces higher labour productivity and improves firm performance. However, the empirical literature on the effect of profit-sharing on wages is limited, and neither the Weitzman theory nor the efficiency wage hypothesis is borne out by existing results. This paper intends to tackle this issue in the case of France.

From an historical point of view, profit-sharing was implemented in France in the early 1960s to promote employee participation in management and to diminish conflicts between employees and employers. French labour law currently defines two profit-sharing mechanisms. The first mechanism, called "intéressement" and denoted "free" profit-sharing (FPS) in this paper, was introduced in 1959. Its implementation within a firm is voluntary, and the bonus depends on the firm's performance (e.g., financial results, labour productivity, absenteeism decrease, etc.). The second mechanism, called "participation aux bénéfices" and denoted "legal" profit-sharing (LPS), was implemented in 1967. It differs from FPS in three main respects: First, the system is compulsory for all firms with at least 50 employees[1]. Second, the law defines the computation formula of the LPS bonus. Finally, until 2009, LPS was designed as a deferred profit-sharing, so bonuses were not paid directly to employees. LPS bonuses were systematically saved though employee savings plans or others savings accounts that were designed to enhance the productive investments of firms. As in Great Britain, the introduction of tax and social benefits may have played a crucial role in the implementation of profit-sharing in France (Marsden & Belfield 2010). Indeed, profit-sharing systems, especially FPS, became widespread and popular at the beginning

of the 1980s when the French State implemented an attractive tax and social system for both employers and employees. By the early 2000s, FPS underwent a renewal because of new tax and social benefits for small firms and numerous legal regulations to promote collective bargaining on profit-sharing. Such advantages are designed to encourage employers to foster financial participation (e.g., profit-sharing, company savings plans, and employee stock ownership plans). On the employee side, tax and social benefits aim to encourage employees to invest a part of their compensation in employee savings plans. In French law, the goal of tax and social incentives is also to compensate for employees' exposure to risk owing to the uncertainty and variability of profit-sharing bonuses[2]. The introduction of a profit-sharing system raises the question of whether profit-sharing, which induces variability in employees' total compensation, affects employees' earnings.

In this paper, we focus on FPS because of its non-compulsory character. To assess the effect of FPS, we work with an original database that covers more than 22,000 French firms over the 2000-2007 period. Our research contributes to knowledge by assessing the effect of profit-sharing on both base wages and total compensation. Moreover, it sheds new light on the relationships between profit-sharing and wages by examining, on one hand, the influence of FPS during the first year after its implementation in firms and, on the other hand, the effect of FPS over seven years. The focus on the first year after the implementation of FPS is critical because French law prohibits the substitution of FPS bonuses for wages for at least one year after its introduction to prevent firms from using profit-sharing as a form of tax optimisation.

The remainder of this paper is organised as follows. The second section presents a review of the theoretical and empirical literature addressing the link between profit-sharing and wages. The third section describes our data and the econometric strategy, and the fourth section presents our results. Finally, we conclude by discussing the potential role of the tax and social reliefs from FPS in France.

II. A brief survey of the literature addressing the link between profit-sharing and wages

Economic theory provides two main views on the effect of profit-sharing on wages. Under the first view, profit-sharing has a neutral effect on total compensation and a negative impact on base wages. Under the second view, profit-sharing has a positive effect on employees' involvement and the retention of the most qualified workers. In this second case, profit-sharing induces an increase in total compensation.

II.1 Theoretical background

According to Weitzman (1984, 1985), profit-sharing bonuses substitute for base wages. The Weitzman theory focuses on the macroeconomic impacts of profit-sharing on unemployment. In the "share economy", which opposes a wage-based economy, the widespread use of profit-sharing increases firms' demand for labour and then ultimately resolves the problem of unemployment without generating inflation. Firms obtain higher profits and employment levels out. Moreover, regardless of the context (share economy or wage-based economy), the level of wages is identical even if the compensation structure differs. The share economy differs from the wage-based economy, however, in that employees' total compensation comprises a base wage (i.e., the fixed part of compensation) and a profit-sharing bonus. From a microeconomic point of view, if profit-sharing lowers base wages, the employment level will increase since firms will make employment decisions by taking into account the level of the base wage (and not total compensation). Profit-

sharing systems thus allow for adjustments to labour costs in the context of an economic downturn. Weitzman argues that firms with a profit-sharing system succeed in maintaining a certain level of employment (and thus profits) by decreasing profit-sharing bonuses, whereas firms without such a system reduce their level of employment (because of wage rigidity).

The Weitzman theory is subject to numerous counterarguments[3]. The most popular objection is that employees, who are supposed to be risk adverse, might resist additional hiring to avoid a reduction to their own profit-sharing bonus. Weitzman (1985) circumvents this criticism by recommending the implementation of social and tax incentives to encourage firms to introduce profit-sharing. However, this idea is unconvincing to some scholars. For instance, in the case of France, Cahuc and Dormont (1997) claim that the introduction of profit-sharing may induce a cost for firms, which is compensated for higher productivity. Numerous economists claim that the effects of profit-sharing on employment are related more to its impact on labour productivity than to its impact on wage flexibility. Several conditions are required to solve the free-riding problem that occurs in the presence of collective bonuses. In the vein of the "efficiency wage" hypothesis, Fitzroy and Kraft (1987, 1992) argue that profit-sharing induces higher employee cooperation when employers cannot observe individual effort or when tasks are independent[4]. Work organisation is also an important determinant of the effect of profit-sharing on productivity, with greater employee autonomy leading to higher labour productivity (Marsden & Belfield 2010). Numerous authors such as Cable and Fitzroy (1980) highlight that profit-sharing efficiency is related to the introduction of other human resource management systems that foster improved information sharing and employee participation in firm decision making. According to Prendergast (2000), the increase in productivity may also result from profit-sharing firms' selection of more productive workers. In this vein, some economists argue that profit-sharing increases the level of labour productivity because it allows firms to retain and attract the most qualified workers (Fang & Long 2012) or to increase investments in employee training (Gielen 2011; Green & Heywood 2011).

To sum up, economic theory predicts two opposite effects of profit-sharing on wages. On one hand, profit-sharing reduces base wages, which is compensated for bonuses (engendering a neutral effect on total compensation), and on the other hand, it adds to base wages.

II.2 Empirical evidence

From an empirical point of view, abundant literature exists on the links between firm performance and profit-sharing (for a survey, see Pérotin & Robinson 2003). Numerous studies show the positive effects of profit-sharing on labour productivity and firm performance in the United States (Kruse 1992, 1993), Germany (Cable & Fitzroy 1980; Fitzroy & Kraft 1987; Kraft & Ugarkovic 2006), France (Cahuc & Dormont 1997; Fakhfakh 1998; Fakhfakh & Pérotin 2000), and Great Britain (Bryson & Freeman 2010; Conyon & Freeman 2001; Robinson & Wilson 2006). Some of these studies demonstrate that labour productivity gains are due to not only profit-sharing but also improved employee participation or information sharing (Dube & Freeman 2010; Cable & Fitzroy 1980). Fakhfakh and Pérotin (2000) note that work organisation plays an important role in the effect of profit-sharing on productivity. In particular, profit-sharing has a positive impact on labour productivity when hierarchical monitoring is weak. Other empirical works focus on the impact of profit-sharing on worker behaviour and find a positive impact of profit-sharing on the decline of absenteeism in France (Brown et al. 2000) and Great Britain (Wilson & Peel 1991). In a more recent study, Blasi et al. (2010) find that profit-sharing is associated with lower employee turnover and greater loyalty in the United States. However, none of these studies question whether

wages might be affected by profit-sharing, and yet, wage flexibility and firm performance improvement are not necessarily opposing objectives (Weitzman 1984).

Empirical evidence on the effect of profit-sharing on wages remains scarce: few analyses use recent data, and the existing results are contradictory. In the case of France, Mabile (1998) uses the 1992 French survey on the structure of earnings and administrative data from collective agreements on FPS for the period between 1986 and 1992. The results reveal that employees who were within an FPS system earned higher total compensation in 1992 that those who were not within such a system. The difference in total compensation is approximately 4.3% within firms that effectively distributed a FPS bonus (compared with 1.3% within firms that did not). However, a few years after the implementation of FPS, the increase in base wages is lower in firms with FPS than in other firms, and the impact of FPS even becomes negative five years after the implementation of profit-sharing, while total compensation remains higher in firms with FPS than in other firms. The main drawback of this study is the lack of information on the economic situation of the firms: for instance, the observed higher wages and total compensation may be related to the greater economic performance of firms with FPS. These findings confirm other analyses, particularly from the United States. For instance, Bell and Neumark (1993) conclude that profit-sharing led to a 5% annual decline in labour costs between 1984 and 1987 in the unionised sector. Mitchel et al. (1990) similarly find a negative correlation between profit-sharing and wages over the 1981-1987 period. Likewise, comparing the wage level in the automobile industry in the United States and Canada over the 1982-1989 period, Katz and Meltz (1991) conclude that profit-sharing substitutes for wages in the United States, and they attribute the higher earnings perceived by Canadian workers to the avoidance of profit-sharing.

Other studies find a neutral effect of profit-sharing on wages. For instance, Forth and Millward (2004) use the 1998 Workplace Employment Relations Survey (WERS) to analyse the impact of high-involvement management practices on gross hourly earnings. They show that the individual influence of financial participation (i.e., profit-sharing and/or employee stock ownership) is not significant; however, the correlation becomes positive and significant when they take into account other high-involvement practices. Using the same data as Forth and Millward, McNabb and Withfield (2007) also find that profit-sharing alone has a neutral effect on wages but that pay is lower when profit-sharing is associated with other performance-related pay. However, these last two studies do not explain the impact of profit-sharing on the compensation structure.

Finally, some studies support the efficiency wage hypothesis. For instance, using data from the National Longitudinal Survey Youth, Afzar and Danninger (2001) find a positive impact of profit-sharing on wage growth in the United States for the period between 1988 and 1994. Further, Wadhwani and Wall (1990) use data on 101 British manufacturing firms for the 1972-1982 period and find a positive impact of profit-sharing on both base wages and labour productivity. Likewise, Bhargava and Jenkinson (1995) analyse data on 144 British firms over the 1979-1989 period and find that the introduction of profit-sharing increases base wages by 4% to 6%. For Germany, Hart and Hübler (1991) use individual data from the German Socio-Economic Panel for 1984 and 1985, and they find that base wages are higher in firms with profit-sharing than for other firms. Moreover, using several waves of the IAB Establishment Panel (2000, 2001, 2005, and 2007), Andrews et al. (2010) conclude that profit-sharing increases total compensation (including profit-sharing bonuses) within German establishments. For Canada, Long and Fang (2012) show that firms that introduced profit-sharing between 2000 and 2001 paid higher total compensation than other firms. More precisely, the authors find that total compensation increased by 15% five years

after the implementation of profit-sharing, whereas the increase in base wages remained small. The analysis by Baghdadi et al. (2013) based on the Annual Survey of the Cost of the Labour and the Wage Structure for 2006 (ECMOSS) on French firms corroborates the above results. Assessing the individual influence of FPS and the joint effect of employee stock ownership plans and FPS, these authors obtain findings consistent with the efficiency wage hypothesis: employee stock ownership plans and FPS have a positive joint effect on both total compensation and base wage for managerial and non-managerial workers, whereas FPS has a neutral individual impact on base wage and a positive individual impact on total compensation for 2006, wherein profit-sharing appears to be an "extra-payment".

In sum, existing empirical evidence on the relationship between wages and profit-sharing remains insufficient to conclude which theoretical argument (substitution versus complementarity) generally prevails. As Long and Fang (2012) note, some studies use cross-sectional data that do not allow one to control for causal inferences. Rather, panel data are required to determine whether profit-sharing and wages are substitutes or complementary components of employee compensation. As mentioned above, in the case of France, the use of panel data appears to be crucial because of the legal prohibition against the substitution of profit-sharing for base wages during the first twelve months after the introduction of profit-sharing. In addition, most studies do not explain how profit-sharing might affect both base wages and total compensation. Thus, the effect of profit-sharing must be reconsidered by taking into account the date of the introduction of profit-sharing within a firm and the compensation structure.

III. Data and key variables

To assess the effect of profit-sharing on wages, we construct an original panel dataset by merging three statistical sources. First, we use the firm-level survey called the PIPA survey ("Participation, Intéressement, Plan d'épargne entreprise et Actionnariat des salariés"). This survey is conducted on a yearly basis by the French Ministry of Labour. It covers a representative sample of firms (approximately 17,000 firms) with at least 10 employees in the private sector and provides detailed information on the use of profit-sharing and employee savings plans among French firms as well as the amount of bonuses distributed. Information on wage bills from the DADS file (see *infra*) is also available. In the PIPA survey, variables refer to firms' practices during the year preceding the time of data collection: for instance, the 2001 PIPA survey provides wage bills paid by firms in 2000 and profit-sharing bonuses distributed in 2000 and calculated on the 1999 accounting year basis. In this paper, we use data from the PIPA survey for the 2000-2008 period to obtain homogenous data on firms between 1999 and 2007[5]. Second, we use two administrative sources produced by the French Institute of Statistics (Insee): the FICUS and the DADS files for the 1999-2007 period. The FICUS files provide various economic and financial variables (e.g., value added, capital investment, firm profit), and the DADS files provide information about wage levels and workforce characteristics (e.g., qualification, gender). In the DADS files, the concept of wage is interpreted broadly; thus, it actually refers to annual total gross compensation, including profit-sharing bonuses and supplementary top-up payments. No information on the structure of total compensation (basic wage, premium, and bonuses) is available. To determine how FPS affects wages, we construct two dependent variables:

- Total compensation (Twage; in logarithm form) denotes the gross annual wage perceived by an employee. Information for this variable comes from the DADS files and includes all payments made by employers each year (including the FPS bonus).
- Base wage (Bwage; in logarithm form) is the total gross annual wage minus the FPS

bonus. We construct this variable by using the DADS files to obtain total compensation and the PIPA surveys to obtain information on FPS bonuses (distribution and amounts).
As mentioned above, given the time lag of PIPA surveys, we analyse trends in total compensation and base wages between 2000 and 2007.

After merging the three statistical sources, we obtain an original unbalanced panel database of a large sample of firms over the 2000-2007 period. To differentiate the effects of FPS by the year of implementation, we construct four groups of firms by taking into account their practices at the first and the last observation: the first group, called "implementation of FPS" (Group 1), includes firms that implemented FPS for the first time during the 2000-2007 period; the second group, called "abandonment of FPS", includes firms that practiced FPS in 2000 but not in 2007 (Group 2); the third group, "existence of profit-sharing", includes firms in which FPS was implemented continuously between 2000 and 2007 (Group 3); the last group, "absence of profit-sharing", includes firms that never implemented FPS during the studied period (Group 4). At this stage, the main drawback is the difficulty of differentiating between firms that never implemented FPS during the 2000-2007 period and firms that did not implement FPS at either the first or the last observation. As a profit-sharing agreement usually last for at least three years, we exclude firms for which any change in FPS practices occurred within a three-year period. Finally, we work with an unbalanced panel of 22,980 firms with at least 10 employees that contains more than 86,400 observations over the 2000-2007 period. Table 1 reports the distribution of firms according to their FPS practices between 2000 and 2007. The "absence of FPS" group constitutes the largest group (more than 66% of firms), while approximately 22% of the sample comprises firms that implemented FPS each year between 2000 and 2007. Further, less than 8% of the sample comprises firms that began implementing FPS between 2000 and 2007, and only 3.7% of firms abandon FPS during the period.

[Table 1]

Our database enables us to assess the effect of FPS on total compensation and base wages while taking into account observable firm characteristics (e.g., sector, size, workforce characteristics, economic and financial indicators) and unobservable characteristics since data are available for at least two dates (i.e., the first and the last observations) (see Appendix A for descriptive statistics on the main variables).

IV. Empirical strategy

Since firms that implement profit-sharing share few characteristics with firms that do not (i.e., Group 4), we use an empirical methodology that enables us to account for these differences. Specifically, we use a propensity score matching model that was initially developed by Rosenbaum and Rubin (1983) to assess the effects of medical treatments. This method entails considering profit-sharing as a treatment and constructing for each firm that implemented FPS an identical counterfactual that did not implement FPS. Given our data, we have two types of firms: firms included in Group 1 or Group 3 that implemented FPS (FPS=1) and those included in Group 4 that did not implement FPS over the 2000-2007 period (FPS=0). The effect of FPS on wages is measured with the outcome variables. Thus, each firm is characterised by two potential outcomes: y_0 if FPS=0 and y_1 if FPS=1. However, the effect of FPS on wages ($C = y_1 - y_0$) is unobservable and individual. Consequently, the distribution of this effect is not identifiable since y_0 and y_1 are

never observed simultaneously. Only the achieved outcome can be observed.

Let Y_i be the vector of outcome variables. For each firm, only the couple (Y, FPS) is observed. Nevertheless, if the latent outcome variables are independent of the assignment to the treatment ($(y_0, y_1) \perp FPS$) —in others words, if the treatment is randomly assigned— then the average effect on the treated firms (i.e., firms with FPS) can be identified: $C_{treated} = E[(y_0, y_1)/FPS = 1]$. However, this property of independence is seldom confirmed. A solution would also entail constructing a control group, so that the distribution of a set of observable characteristics (i.e., a set of control variables, noted *X)* is identical to the characteristic set of firms implementing FPS. In this way, we are able to reduce the selection bias. The identification condition also becomes less restrictive, and the independence property has to be checked ($(y_0, y_1) \perp EP/X$). If numerous control variables are taken into account, finding a counterfactual for each treated firm becomes problematic. According to Rosenbaum and Rubin (1983), the conditional independence with respect to the set of control variables is equal to the independence relative to the propensity score $P(X)$, which is a one-dimensional summary of matching variables that estimates the probability of being assigned to the treatment, conditional these variables: $(y_0, y_1) \perp EP/X$. The literature defines numerous propensity score matching methodologies. For instance, Caliendo and Kopeining (2005) recommend the use of several estimators. If we obtain similar results, the chosen approach does not matter. Therefore, we implement two matching strategies: nearest-neighbour matching with and without replacement and radius matching. In this paper, we report only the results obtained by using radius matching (with a radius equal to 0.001) since we obtain very similar findings by using nearest-neighbour matching.

The panel allows us to control for an individual and unobservable fixed effect that simultaneously affects the treatment and outcome variables. We are able to match differences between firms by using a difference-in-differences selection model and observing firms over time (Heckman et al. 1997; Heckman et al. 1998). This methodology allows us to take into account both observable and unobservable characteristics of firms that implemented FPS when we evaluate the causal effect of FPS on wages. This approach entails observing the variation of the outcome variable between two dates (first difference) and comparing this variation between the treated and the untreated firms (second difference). The formula of the treatment effect on the treated firms is as follows:

$$\Delta = \frac{1}{N_1}\sum_{i\in I_1}\left\{\left(Y_{t,i} - Y_{t',i}\right) - \sum_{j\in I_0} M^i\left[\frac{\left[\left(P\left(X_j\right) - P\left(X_i\right)\right)\right]}{\sum_{i\in I_0}\left[\left(P\left(X_j\right) - P\left(X_i\right)\right)\right]}\right]\left(Y_{t,j} - Y_{t',j}\right)\right\}$$

Where N_1 is the number of firms that implemented FPS. I_1 represents the whole sample of firms that are involved in this profit-sharing mechanism, and I_0 represents the sample of firms that are not. P(X) is the estimated propensity score, and Y, the wage level. $M^i[]$ represents the average value of the outcome variable among the population of firms *j* that belong to the control group and that are selected among firms *i*. *t* and *t'* represent the two periods, before and after the treatment assignment. This estimator is supposed to satisfy the common trend assumption that we will simultaneously find treated and untreated firms for each value of the matching variable.

Our empirical strategy involves two steps. In the first step, we run a probit model to estimate the propensity score. We thus estimate two models because our goal is to assess the effects of FPS on

wages within two groups of firms: Group 1 ("implementation of FPS") and Group 3 ("existence of FPS"). The first model (Model 1) estimates the probability that a firm will begin implementing FPS between 2000 and 2007, and the second model (Model 2) computes the probability that a firm will implement FPS each year (without discontinuity). From the literature on the determinants of FPS, we retain three categories of control variables (regardless of the model): firm characteristics (firm size, sector, business group affiliation), economic performance indicators (economic rate of return, labour productivity) and workforce characteristics (proportion of managerial employees, proportion of women). Since we do not necessarily compute wage differences during the same period, we also introduce a variable that takes into account the number of years between the first and the last observation for each firm ("time window"). Moreover, we consider the potential simultaneous implementation of compulsory profit-sharing (LPS) (and bonus distribution) because of the potential correlation between LPS and FPS. Finally, we introduce years as dummy variables to take into account economic circumstances. All variables are lagged by the first observation. In addition, we consider the distribution of quantitative variables (by quartile) as dummy variables. To check whether the probit models provide a sufficient specification to evaluate FPS practices, we implement a balancing test that analyses standardised differences. For this test, we compute the mean of each control variable for the treated and untreated firms and thus estimate the reduction in selection bias associated with the difference in average differences before and after matching. In the second step, we estimate the average treatment effect on the treated (ATT) to show the difference in wage variation between the treated and the untreated groups by using the radius matching estimator. During this step, we compute standard deviations with and without bootstraps, which appear to be nearly identical.

V. Results

We first present the estimated effect of FPS implementation and then discuss the effect of the existence of FPS on wages. Subsequently, we check the robustness of our results.

V.1 Estimated effect of the implementation of FPS

To assess the effect of the implementation of FPS, we compare both total compensation and base wages between firms that implemented FPS for the first time between 2000 and 2007 (Group 1) and firms that did not implement FPS (Group 4). As explained above, we first run a probit model to compute the propensity score, which refers to the probability that a firm introduces FPS during the 2000-2007 period. The first stage enables us to discuss the determinants of FPS implementation within firms. The results are listed in Appendix B. As shown, the coefficient for the variable "time window" is positive and significant, indicating that firms with FPS survive longer than firms without FPS. Moreover, we find that the implementation of FPS is significantly associated with several firm characteristics. Regarding firm size, the probability of implementing FPS increases with firm size, especially up to the 500-employee threshold. Sector and business group affiliation are also important factors. Relative to the reference sector (energy), most of sectors are negatively associated with the probability of implementing FPS. The independent firms are less likely to implement profit-sharing. The relationships between firm performance and FPS implementation are rather surprising. The probability of introducing FPS decreases with profitability. We speculate that less profitable firms may introduce FPS to boost their economic performance. In line with the efficiency wage hypothesis, we find a positive correlation between labour productivity and FPS implementation. Workforce composition likely plays an important role in FPS implementation. Similar to Fakhfkah and Perotin (2000), we find that the introduction of FPS is associated with a higher proportion of managers. This result may indicate that firms with

FPS may attract the most qualified workers (Fang & Long, 2012). In addition, the estimated probability of introducing FPS decreases with the proportion of women. Finally, LPS and FPS seem to be complementary mechanisms since the implementation of FPS is positively correlated with the existence of LPS. However, the distribution of LPS and the probability of introducing FPS are negatively correlated. With respect to this result, we speculate that firms that already distribute LPS cannot pay additional bonuses (i.e., FPS).

As detailed in the previous section, we check whether two conditions that are necessary for the propensity score estimation are satisfied. The first condition is that the common support of treated and untreated firms is sufficiently large (see Appendix C). The balancing test that analyses standardised differences reveals that the bias is strongly reduced after the matching procedure. For almost all the variables, the bias reduction is approximately 90% (see Appendix D).

In the second step, we estimate the causal effect of FPS implementation by controlling for observable and unobservable characteristics. In this way, we use the estimated probabilities on the treated and the untreated firms to compute the effect of FPS implementation on differences in both total compensation and base wages. The matching results are reported in Table 2. Two outcomes variables are defined as the variation in total compensation and the variation in base wages between 2000 and 2007. For each of these outcomes variables, Table 2 presents the difference in variation between firms that implemented FPS (Group 1) and firms that did not (Group 4) before and after matching.

[Table 2]

The results reveal that the difference in total compensation (with FPS bonuses included) between firms that implemented FPS and firms that did not is positive and significant before matching (by 6.9 points). On average, total compensation increased at a faster rate in firms that implemented FPS between 2000 and 2007 than in firms that did not. Once we control for observable and unobservable characteristic, the difference is still positive, but it becomes non-significant. As expected, the results show that the difference in base wages is not significant before and after matching, suggesting that firms respect the legal prohibition of the substitution of FPS for wages during the first year after the introduction of FPS. Finally, the ATT based on the difference-in-differences model also suggests that the effect of FPS implementation is neutral regardless of the outcome variable. Based on these results, we conjecture that during the year after FPS implementation, FPS bonuses may be too low to substitute for or complement base wages. Finally, these findings do not enable us to validate the Weitzman theory or the efficiency wage hypothesis.

V.2 Estimated effect of the existence of FPS

Furthermore, we assess the effect of FPS implementation over several years, particularly since 2000. The results of the probit model (Model 2) estimating the probability of the existence of FPS since 2000 are reported in Appendix E. The results are similar to the findings reported in the previous section. Once again, the results show that the existence of FPS is strongly correlated with firm characteristics. The estimated probability increases with firm size. As mentioned above, substantial differences across sectors are observed, with almost all sectors (except for the financial and real estate sectors) relative to the energy sector showing negatively correlations with the existence of profit-sharing. Moreover, the probability of the existence of FPS increases with the proportion of managers. Further, the most profitable firms are less likely to have practiced FPS

over the studied period, whereas the most productive firms (in terms of labour productivity) often more likely to have practiced FPS since 2000. Finally, FPS and LPS are positively correlated.
As in the previous section, the two conditions that are necessary for a good specification are satisfied. The common support (see Appendix F) is sufficiently large such that the propensity scores of treated and untreated firms are satisfactorily superposed. Moreover, for almost all variables, the bias reduction is approximately 90% after matching (see Appendix D). Table 3 reports the results regarding the effects of the existence of FPS.

[Table 3]

Before we take into account observable and unobservable firm characteristics (i.e., before matching), firms with profit-sharing pay higher total compensation and base wages than firms without profit-sharing. The difference of trends in total compensation is indeed positive and significant (approximately 4 points), and after matching, this difference remains positive but becomes non-significant. Meanwhile, before matching, the results do not show a difference of trends in base wages between firms with profit-sharing and other firms. However, after matching, this difference becomes negative and significant. In other words, profit-sharing has a neutral effect on total compensation and a negative impact on base wages. These results are consistent with the Weitzman hypothesis. The effect of profit-sharing is neutral because employees receive total compensation that is equivalent to the base wage paid by non-profit-sharing firms. Profit-sharing should thus be interpreted as a device whereby the profit-sharing bonus compensates for the wage moderation. Our conclusions also confirm the findings of Mabile (1998) that highlight the role played by the date of FPS introduction.

V.3 Robustness check

To test the robustness of our results, we implement an alternative econometric strategy: an instrumental variable method with panel data. With this strategy, we can control for unobservable heterogeneity by using the "within" estimator instead of a double-difference matching estimator. We also assume that the amount of the FPS bonus (and implicitly its distribution) is endogenous: it depends on the choice of the firm to implement FPS.

We use a fixed-effect, two-stage least squares estimator (FE-2SLS). The main drawback of our data is the difficulty of finding a valid instrumental variable. We choose to consider the lagged distributed bonuses as an instrumental variable. In most cases, this variable would not be appropriate; however, it is the only variable available in our data. We take into account differences in observation duration between firms because our panel data are unbalanced. For each firm, all variables are also centred by weighting them by the observation duration.

The results confirm our previous findings: FPS substitutes for base wages in firms with FPS. Moreover, total compensation is not affected, as the trends in total compensation are similar between firms with FPS and firms without FPS.

VI. Conclusion

This paper investigates the effect of "free" profit-sharing (FPS) on employee earnings for the period between 2000 and 2007 in France. For this purpose, we distinguish between trends in base wages and trends in total compensation. We also differentiate the effects between the years of FPS implementation since the French Law prohibits the substitution of profit-sharing for wages during

the first twelve months after the introduction of FPS. For this analysis, we construct an original unbalanced database that reflects FPS practices within more than 22,000 firms over the 2000-2007 period. To account for the observable and unobservable characteristics of firms according to their FPS practices, we use a difference-in-differences selection model.

First, we show that the introduction of FPS does not affect base wages or total compensation. Taking into account observable and unobservable firm characteristics, we show that the trends in base wages and total compensation within firms with FPS are similar to those in firms without FPS. This neutral influence suggests that profit-sharing bonuses are too weak to substitute for or complement wages during the first year after the implementation of FPS in a firm. Second, the comparison between firms that have practiced FPS for a long time and firms that never implemented FPS between 2000 and 2007 reveals that FPS bonuses substitute for base wages. While the effect of FPS on total compensation remains neutral, it becomes negative on base wage. This result is consistent with the mechanism described by Weitzman (1984, 1985): FPS bonuses offset the wage moderation such that total compensation is similar between firms with profit-sharing and firms without profit-sharing, even if the compensation structure is different.

This paper suggests that FPS generally did not provide employees with additional compensation beyond their base wages between 2000 and 2007. This finding raises the question whether existing tax and social relief might lead employers and employees who are involved in profit-sharing to share a "tax rent". Estrin et al. (1987) and Wadwhani (1988) note the potential inefficiency of tax relief in Great Britain since it may lead to "cosmetic profit-sharing". Shedding new light on the debate regarding profit-sharing, our results suggest that the gains for employees from FPS might be related more to social and tax advantages than to the FPS bonus. Indeed, employees who save their FPS bonus though employee savings plans receive tax exemptions. From the employer point of view, FPS might represent not only a flexibility wage tool but also a tax optimisation device since it lowers base wages that are currently subject to social contributions.

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Table 1 – Distribution of firms according to their FPS practices

	Nb of firms	% of firms	Nb of observations
Group 1: Implementation of FPS	1,713	7.5	8,434
Group 2: Abandonment of FPS	860	3.7	3,782
Group 3: Existence of FPS	5,055	22.0	22,314
Group 4: Absence of FPS	15,352	66.8	51,961
Total	22,98	100	86,491

Source: Unbalanced panel of firms. PIPA Survey (2000-2008); DADS and FICUS files (1999-2007).
Field: Firms with at least 10 employees in the private sector.

Table 2 - Estimated effects of FPS implementation (average treatment effect on the treated - ATT)

Outcome variables		Difference	Standard-error	Standard-error estimated by bosstrap
Total compensation (by employee, with FPS bonuses included)	*Before matching*	0.069***	0.007	
	After matching (ATT)	0.006	0.008	0.009
Base wage (by employee, with FPS bonuses excluded)	*Before matching*	0.053	0.007	
	After matching (ATT)	-0.009	0.008	0.007

Source: Unbalanced panel of firms. PIPA Survey (2000-2008); DADS and FICUS files (1999-2007).
Field: Firms with at least 10 employees in the private sector.
Note: Significance level: *** (1%).

Table 3 - Estimated effect of the existence of FPS (average treatment effect on the treated - ATT)

Outcome variables		Difference	Standard-error	Standard-error estimated by bosstrap
Total compensation (by employee, with FPS bonuses included)	*Before matching*	0.042***	0.005	
	After matching (ATT)	0.001	0.007	0.006
Base wage (by employee, with FPS bonuses excluded)	*Before matching*	-0.001	0.005	
	After matching (ATT)	-0.04***	0.007	0.006

Source: Unbalanced panel of firms. PIPA Survey (2000-2008); DADS and FICUS files (1999-2007).
Field: Firms with at least 10 employees in the private sector.
Note: Significance level: *** (1%).

Appendix A: Main variables (descriptive statistics)

	Group 1: "Implementation of FPS" (8,434 obs.)	**Group 2: "Abandonment of FPS" (3,782 obs)**	**Group 3: "Existence of FPS" (22,314 obs.)**	**Group 4: "Absence of FPS" (51,961 obs.)**	**Global sample (86,491 obs.)**
Nb of firms	**1,713**	**860**	**5,055**	**15,352**	**22,980**
% of global sample	7.5	3.7	22.0	66.8	**100**
Firm size (nb of employees)					
From 10 to 49	14.06	17.08	12.97	50.63	35.91
From 50 to 99	9.32	13.43	9.15	14.27	12.42
From 100 to 249	26.58	27.39	23.56	17.54	20.43
from 250 to 499	25.68	22.77	24.8	10.22	16,0
From 500 to 999	14.37	12.27	14.64	4.16	8.22
More than 1,000	9.98	7.06	14.87	3.19	7.03
Sector					
Agri-food industry	7.73	3.49	6.11	4.2	5.01
Consumer goods industry	6.58	8.49	6.17	6.06	6.24
Automotive industry	1.87	1.77	1.98	0.86	1.29
Capital goods industry	8.11	9.33	8.34	5.84	6.86
Intermediate goods industry	15.88	19.09	18.89	11.71	14.29
Energy	0.97	0.69	1.61	0.26	0.7
Construction	7.97	5.29	4.59	9.83	8.1
Commerce	12.98	16.47	17.44	21.28	19.27
Transports	7.2	9.07	4.64	6.76	6.35
Financial activities	4.01	1.67	7.48	1.19	3.11
Real-Estate activities	3.02	1.08	3.16	1.29	1.93
Business activities	14.68	15.44	14.85	19.2	17.47
Personal and domestic services	4.75	3.62	2.32	7.31	5.61
Education, health and social services	4.01	4.26	2.14	3.77	3.4
Administration	0.24	0.24	0.26	0.43	0.36
Belonging to a group					
Yes	86.79	84.82	89.95	59.03	70.84
No	13.21	15.18	10.05	40.97	29.16
Profitability rate					
1rst quartile	25.67	36.77	24.57	25.79	25.96
2nd quartile	27.45	26.77	27.78	23.5	25.07
3rd quartile	26.57	19.96	26.18	24.47	24.9
4rth quartile	20.31	17.1	21.47	26.25	24.08
Labour productivity					
1rst quartile	19.17	27.29	16.69	29.67	25.2
2nd quartile	24.28	27.29	20.99	26.9	25.14
3rd quartile	27.37	26.47	26.87	23.64	24.96
4rth quartile	29.18	18.75	35.45	19.78	24.69

Appendix A (Cont.)

	Group 1: "Implementation of FPS" (8,434 obs.)	Group 2: "Abandonment of FPS" (3,782 obs)	Group 3: "Existence of FPS" (22,314 obs.)	Group 4: "Absence of FPS" (51,961 obs.)	Global sample (86,491 obs.)
Nb of firms	**1,713**	**860**	**5,055**	**15,352**	**22,980**
% of global sample	7.5	3.7	22.0	66.8	**100**
Proportion of women					
1rst quartile	25.04	25.4	22.83	26.77	25.56
2nd quartile	26.66	26.63	26.76	23.77	24.92
3rd quartile	25.63	25.82	28.45	23.07	24.78
4rth quartile	22.67	22.16	21.97	26.39	24.74
Proportion of managers					
1er quartile	18.91	18.12	13.68	31.93	25.44
2ème quartile	28.24	28.53	28.56	23.6	25.52
3ème quartile	29.13	27.69	29.2	22.29	24.93
4ème quartile	24.02	25.66	28.56	22.18	24.12
FPS					
Distribution of bonus (% of firms)	74.72	47.1	84.76	-	81.33
Average bonus (euros)	873	847	1,427	-	1,384
LPS					
% of firms with LPS	72.11	62.45	79.92	38.46	53.49
Distribution of bonus (% of firms)	64.98	50.85	72.82	66.51	67.84
Average bonus (euros)	1,254	1,138	1,546	1,323	1,400

Source: Unbalanced panel of firms. PIPA Survey (2000-2008); DADS and FICUS files (1999-2007).

Field: Firms with at least 10 employees in the private sector.

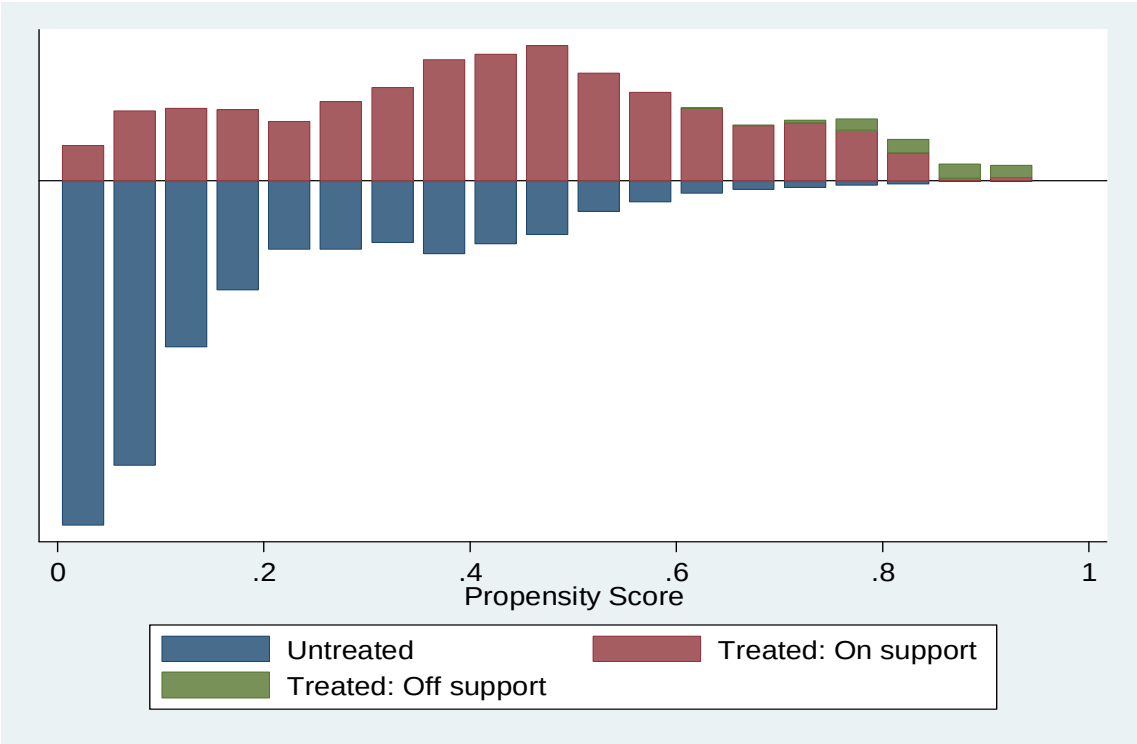
Reading note: In all, 7.5% of firms implemented profit-sharing between 2000 and 2007. 14.1% of these firms employ between 10 and 49 employees.

Appendix B: Estimation results of the probability for introducing FPS between 2000 and 2007 (Model 1)

Variables		Coefficient	Standard-errors
Firm size (ref.: from 10 to 20 employees)	From 20 to 49	0.094*	0.0542
	From 50 to 99	0.203***	0.0589
	More than 500	0.401	0.0748
Business group affiliation	Independent firm	-0.250***	0.0398
Sector (ref.: energy)	Agri-food industry	-0.177	0.211
	Consumer goods industry	-0.488**	0.211
	Automotive industry	-0.176	0.238
	Capital goods industry	-0.34	0.209
	Intermediate goods industry	-0.372*	0.206
	Construction	-0.359*	0.2082
	Commerce	-0.594***	0.205
	Transports	-0.388*	0.21
	Financial activities	0.041	0.223
	Real Estate activities	-0.03	0.228
	Business activities	-0.536***	0.205
	Personal and domestic services	-0.457**	0.211
	Education, health and social services	-0.42*	0.218
	Administration	-0.266	0.3097
Profitability rate	2nd quartile	-0.116***	0.0435
(ref.:1rst quartile)	3rd quartile	-0.097**	0.0437
	4rth quartile	-0.172***	0.043
Labour productivity	2nd quartile	0.086**	0.04338
(ref.:1rst quartile)	3rd quartile	0.041	0.04657
	4rth quartile	0.188***	0.04963
LPS	Yes	0.47***	0.047
Distribution of LPS bonus	Yes	-0.108***	0.04
Proportion of women	2nd quartile	-0.061	0.0429
(ref.:1rst quartile)	3rd quartile	-0.086*	0.0466
	4rth quartile	-0.098**	0.049
Proportion of managers	2nd quartile	0.168***	0.0438
(ref.:1rst quartile)	3rd quartile	0.195***	0.0455
	4rth quartile	0.172***	0.0499
Time window	Yes	0.15***	0.0069
constant		-1.716***	0.2133

Source: Unbalanced panel of firms. PIPA Survey (2000-2008); DADS and FICUS files (1999-2007).
Field: Firms with at least 10 employees in the private sector.
Notes: Significance levels: *** (1 %); ** (5 %), * (10 %).

Appendix C: Propensity scores distribution (Model 1)



Appendix D: Balancing test (Models 1 and 2)

		Bias reduction after matching	
Variable		Model 1	Model 2
Firm size (ref.: from 10 to 20 employees)	From 20 to 49	98.3	99
	From 50 to 99	94.2	94.7
	More than 500	91.8	96
Business group affiliation	Independent firm	99.3	98.4
Sector (ref.: energy)	Agri-food industry	70.8	70.8
	Consumer goods industry	55.5	26.9
	Automotive industry	96.4	74.1
	Capital goods industry	77.6	88.8
	Intermediate goods industry	97.4	75.9
	Construction	64.8	87.5
	Commerce	93.1	77.4
	Transports	ns	99
	Financial activities	94.5	98
	Real Estate activities	72.6	97.5
	Business activities	73.6	13
	Personal and domestic services	87.7	100
	Education, health and social services	ns	99
	Administration	73.2	97.6
Profitability rate	2nd quartile	57.4	71.4
(ref.:1rst quartile)	3rd quartile	ns	48.3
	4rth quartile	89.8	62.8
Labour productivity	2nd quartile	ns	95.3
(ref.:1rst quartile)	3rd quartile	ns	87.2
	4rth quartile	93.7	99.8
LPS	Yes	99	99.9
Distribution of LPS bonus	YES	96.4	99.4
Proportion of women	2nd quartile	ns	91.7
(ref.:1rst quartile)	3rd quartile	99.9	87.9
	4rth quartile	4.3	97.3
Proportion of managers	2nd quartile	90.5	65.1
(ref.:1rst quartile)	3rd quartile	97.2	91.7
	4rth quartile	85.6	71.7
Time window	yes	97.9	95.9

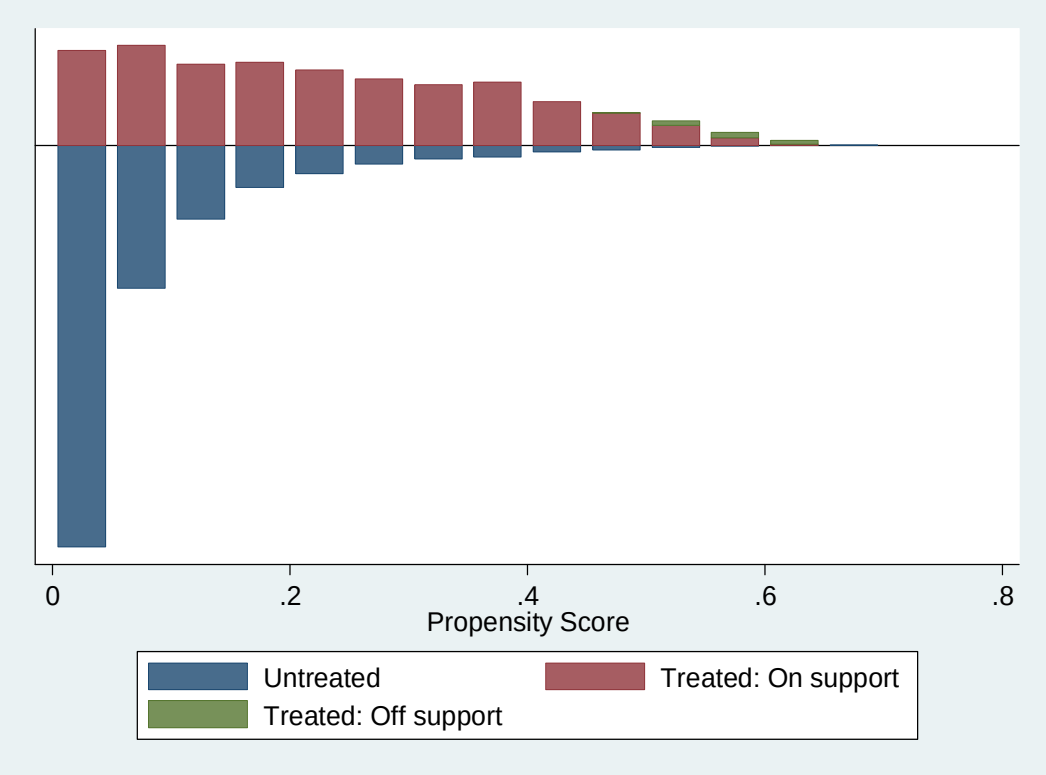
Source: Unbalanced panel of firms. PIPA Survey (2000-2008); DADS and FICUS files (1999-2007).
Field: Firms with at least 10 employees in the private sector.
Reading: "ns" means that differences between treated and untreated firms were not significant before matching. Regarding the Model 1, the introduction of the variable "business group affiliation" as an independent variable enables us to reduce differences between treated and untreated firms after matching on average by 99.3%.

Appendix E: Estimation results of the probability of the existence of FPS between 2000 and 2007 (Model 2)

	Variable	Coefficient	Standard-deviation
Firm size (ref,: from 10 to 20 employees	From 20 to 49	0.154***	0.0403286
	From 50 to 99	0.335***	0.0439457
	More than 500	0.838***	0.0549857
Business group affiliation	Independent firm	-0.355***	0.0295567
Sector (ref.: energy)	Agri-food industry	-0.561***	0.1455333
	Consumer goods industry	-0.807***	0.1436555
	Automotive industry	-0.420***	0.1661659
	Capital goods industry	-0.621***	0.1420611
	Intermediate goods industry	-0.573***	0.1393118
	Construction	-0.800***	0.142941
	Commerce	-0.780***	0.1387735
	Transports	-0.728***	0.1444179
	Financial activities	0.1	0.1504872
	Real Estate activities	-0.1054934	0.1550196
	Business activities	-0.747***	0.1390818
	Personal and domestic services	-1.106***	0.1472962
	Education, health and social services	-0.807***	0.1519458
	Administration	0.558**	0.2286543
Profitability rate	2nd quartile	0.0218153	0.0330422
(ref.1rst quartile)	3rd quartile	-0.0259058	0.0335068
	4rth quartile	-0.057*	0.0328695
Labour productivity	2nd quartile	0.186***	0.0339556
(ref.:1rst quartile)	3rd quartile	0.265***	0.0350857
	4rth quartile	0.458***	0.0366712
LPS	Yes	0.571***	0.0365483
Distribution of LPS bonus	Yes	0.101***	0.0309794
Proportion of women	2nd quartile	-0.054*	0.0323402
(ref.:1rst quartile)	3rd quartile	-0.0124096	0.0347545
	4rth quartile	-0.0213477	0.0365487
Proportion of managers	2nd quartile	0.314***	0.0331136
(ref.:1rst quartile)	3rd quartile	0.294***	0.0345175
	4rth quartile	0.271***	0.037636
Time window	Yes	0.041***	0.0053278
constant		-1.083***	0.1464249

Source: Unbalanced panel of firms. PIPA Survey (2000-2008); DADS and FICUS files (1999-2007).
Field: Firms with at least 10 employees in the private sector.
Notes: Significance levels: *** (1%); ** (5 %); * (10 %).

Appendix F: Propensity scores distribution (Model 2)



[1]This legal requirement does not apply to some firms. In particular, public firms, administrations, mutual societies, holding companies, and firms that do not generate profit are not concerned.

[2] Social contributions do not apply to profit-sharing bonuses. To benefit from social and tax reliefs, firms have to define an FPS agreement, which can be reached with personal representatives or employees. In addition, FPS bonuses must be variable and random. In if profit-sharing bonuses substitute for wages, the French administration cannot claim damage if a twelve-month delay has occurred between the implementation of profit-sharing and the wage cut. On the employee side, tax exemptions are granted if employees agree to save FPS bonuses through company savings plans. To prevent firms from engaging in tax optimisation, in 2009, the French legislator introduced a new contribution ("social forfait"), which is equal to 2% of profit-sharing bonuses. In 2014, this contribution, which is paid by employers, is about 20%.

[3] For a discussion of objections to the Weitzman theory, see, for instance, Blanchflower and Oswald (1987), Nuti (1987), and Wadhwani (1988).

[4] Cooperation between employees occurs under a "peer-pressure" context, where workers are supposed to place pressure on others colleagues to monitor shirking behaviour (Lazear & Kandel 1992; Lazear 1995).

[5] This survey has been conducted since 1989, but it was amended in 2000 and 2006. Since 2000, data are not restricted to only firms for which profit-sharing agreements are registered by the French Ministry of Labour. After 2006, firms with less than 10 employees are excluded.

TEPP Working Papers 2015

15_2. A meta-regression analysis on intergenerational transmission of education: publication bias and genuine empirical effect

Nicolas Fleury, Fabrice Gilles

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