Convertible local currencies as an economic development tool for businesses ?
Findings from an panel econometric analysis on french companies joining a Convertible ocal currency

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Foreword :
The following work is presently a doctoral dissertation chapter. It is originally written in French and, as the thesis manuscript is due in mid-September 2022, I have translated it only very roughly in order to provide some support for the presentation on 23 August. I apologise beforehand for the poor quality of the translation and thank you in advance for your understanding in these particular circumstances. It will be further edited to produce an article in the months following the thesis defense.
I also wish to thank Vincent Carret for his precious help in the webscraping of the Siren numbers of CLC member companies, which was necessary for the realisation of this work. I would also like to thank the Nouvelle Aquitaine region, Sciences Po Lyon, and the Centre de recherche de développement territorial de l'Université du Québec en Outaouais, which, through the MoLoNa and TerMos research projects and a study grant, financed access to the Fare files at the Centre d'accès sécurisé aux données (CASD) and enabled the realisation of this study.

Introduction :

The number of convertible local currencies (CLCs) grew particularly quickly in France during the 2010s, with a tenfold increase in the number of CLCs in circulation between 2011 and 2019 (Blanc, Fare, and Lafuente-Sampietro 2020). Thus, 82 MLCs were circulating in France at the end of 2019, covering nearly 30% of French municipalities. The rapid spread of this phenomenon has awakened and been reinforced by the joint interest of public authorities, which legislated on their status in 2014, as well as activist circles that present them as potential tools for ecological and social transition, notably through films such as Demain (Dion and Laurent 2015) or the online training course of the Colibris movement . This proliferation of projects and the attention received by CLCs in France leads us to question their social, economic and environmental effects from a scientific point of view. While there is already an abundant literature on alternative currencies (Blanc 2018a) and on the potential theoretical effects of CLCs (Fare 2016), the measurement and empirical evaluation of these effects is still weak and deserves to be investigated. In this work, we adopt an approach similar to that of public policy evaluation, thinking of CLCs as devices used or not by actors and trying to measure their impact by comparing a test group using an MLC to a non-user control group. We therefore seek to measure the benefits in terms of turnover that companies derive from their use of a CLC.

Convertible local currencies (CLCs) are monetary instruments for specific purposes and circulating alongside national currencies in a given territory. They are created and managed by groups of citizens gathered in non-profit organizations or community banks, sometimes supported by local public authorities. These currencies can take different forms, depending on the project, from paper banknotes to digital payments by card, text message or mobile application. What distinguishes them from other alternative currencies is the way they are issued. The currency is issued through the exchange of national currency units for local currency units at a fixed exchange rate. The currency obtained can then be used in shops and at companies, associations or institutions in the territory that accept it as a means of payment. The national currency used to obtain the local currency is kept in a guarantee fund, allowing the local currency to be converted back into national currency under the conditions set by the issuing institution. This conversion is generally forbidden for individual users, but authorized for companies at the price of conversion fees or at least implicit costs.

The CLCs thus build a separate monetary circuit, forcing their users to exchange among themselves to spend the CLC units they receive. The managing associations also participate in this linkage by playing an intermediation role through the provision of tools and the animation of the user community. The use of the CLC also acts as a signal, identifying economic actors who share similar values and thus splitting the market. The redirection of demand from MLC users to businesses in the monetary community, either through the mechanical constraint of their spending ability or the signal sent by the acceptance of MLC, may result in additional demand for MLC member companies and thus enable them to increase their total turnover. This increase in turnover is, in our view, imperfectly correlated with the turnover achieved in MLC. Indeed, if the constraint effect on the place of expenditure of the monetary units received as payment applies only to the revenues realised in MLC, the signal effect relates more generally to the enterprise as such. Thus, it is likely that actors will choose to buy from one enterprise in the monetary community rather than another because of its acceptance of MLC, while consuming from it in national currency. The additional activity generated by the acceptance of MLC does not therefore seem to be perfectly measurable thanks to the activity carried out in MLC and must therefore be measured on the scale of the companies total accounts.

In order to measure these changes in economic activity, we have chosen to conduct the analysis at a micro level. Krohn and Snyder (2008) have previously attempted to measure the effects of local currencies on economic development by comparing growth in US cities with and without local currencies. However, they failed to show significant impacts of local currencies, but we believe that because of the low territorial coverage of CLCs, the municipal scale they chose is too large to measure a general effect (Michel and Hudon 2015; Matti and Zhou 2022). Moreover, CLCs do not necessarily aim to develop an entire locality, but rather a selected territorial community. We therefore propose to focus the study on the community that actually uses the MLC, and thus to concentrate on the companies involved and not on the municipality as a whole. Our analysis is therefore positioned at the microeconomic and individual level of the activity of MLC member companies.

In order to carry out the econometric study measuring the effect of the acceptance of MLC as a mean of payment on the turnover of companies, we used data from the Fare file, which contains all the tax data of French companies from 2009 to 2019. Since companies are identified by their national Siren number, we can follow the evolution of their activity over the years and use the data in panel form, simplifying the identification of effects.

In this chapter, we will first describe the data used for this study and then the methodology applied for their analysis. We will then present the results of these econometric models and discuss them as a conclusion to this last chapter.

1. Data

To carry out this study, it was necessary to combine several complementary data sources. We first needed to obtain a list of companies that had joined MLCs in order to identify them in other databases. We also needed access to the production information of these firms and of firms in a control group over several years surrounding the dates when the firms joined an MLC, which we obtained from the Fare file.

a. FARE data and their preparation

The Fare file is a file containing all the tax data of French companies in the market sector and involved in productive activity, except for the financial sector and agricultural activities. Companies are identified in the file by their Siren number, a 9-digit public identifier.

The Fare scheme has been in existence since 2008 and has one vintage per year until 2019. However, the first vintages have different variables from the following years, some of which are necessary for our analysis, and we have therefore chosen to use only the vintages from 2010 onwards.

Table 1 - Number of observations in each year of the Fare file

Year	Observations
2010	3 340 887
2011	3 737 728
2012	3 866 486
2013	4 224 263
2014	4 385 731
2015	4 052 206
2016	4 245 075
2017	4 188 215
2018	4 290 267
2019	4 456 558

Total	43 677 123

Each year contains about 190 variables, containing various information ranging from the statistical status of the observation, to the variables of identification and administrative description of the enterprise (Siren, name of the legal entity, legal status, type of enterprise, sector of activity) to the fiscal data of activity (turnover, profits, value added, taxes, assets, number of full-time equivalent employees). This information provides a fairly accurate picture of the companies' financial situation.

c. Experience design

We identify 1,895 companies belonging to 9 french CLCs them in the Fare data. We then had to develop a strategy for selecting companies that were not members of CLCs as control. As we did not have a list of the members of the 80 French CLCs, we had to develop identification strategies in order to be sure to select companies that did not use CLCs.

A first solution was to choose areas with no known CLCs. This solution had the advantage of ensuring the absence of contamination between member companies of CLCs and those of the control group. Indeed, it could happen that, by being located in the same area, the positive effects from which the companies in the test group could benefit would be to the disadvantage of their neighbours. Thus, the measured effect would be overestimated, since the cyclical variation captured by the control group would take into account the negative externality of the use of the CLCs. Furthermore, choosing firms in areas without available CLCs limits the self-selection bias in the schemes. Firms in the control group without access to CLCs did not voluntarily choose not to use them. However, information on all areas with or without a CLC is currently not systematised and we were only able to obtain a list of departments without known CLCs, rather than a finer grid of employment areas or municipalities. This very broad identification of areas without CLCs leaves little choice of areas without known CLCs for selecting the control sample, and these areas turn out to have characteristics very different from those occupied by the CLCs in the test group. Indeed, the fact that an entire department is currently free of CLCs is potentially correlated with many characteristics that may have a joint effect on its economic development. For example, departments without CLCs include far fewer large cities than those with identified CLCs, as these schemes are often located around metropolitan areas ((Blanc, Fare, and Lafuente-Sampietro 2020). It therefore seemed to us that the companies in these localities were probably facing different environments and exogenous shocks than those of the companies in the test group, which could bias our analysis. Moreover, as the census of CLCs is still imperfect, it is not impossible that CLCs exist in some of these departments without our knowledge and could contaminate the control group.

We therefore abandoned this first solution in favour of selecting the control companies within the same employment zones as those of the test companies so that the companies in the test and control groups face similar exogenous contexts. Moreover, as the territories of the CLCs rarely overlap, we know that in these areas, firms not identified in our files are unlikely to be users of a CLC. However, this choice makes it possible for negative externalities to exist for the control group: the decision to enter the CLC of a tested firm could have a negative impact on the activity of firms in the control group in the same locality, due to a transfer of customers for example. As the coverage rates of CLCs in employment areas are still low, we believe that these externalities are minimal and unlikely to be observed at this stage of their development. Furthermore, there is a selection bias between the CLCs and the control group. Indeed, the latter have access to a CLCs, but have chosen not to join it, or have not been aware of it. The factors explaining this choice, such as the socio-economic environment in which these companies and their managers operate, are most likely not observed in the database and could have an effect on the turnover trajectories of these companies.

Despite these biases and in view of the impact identification method deployed, we have chosen the latter solution. We have thus restricted the analysis to companies present in the same employment zones as CLCs members and belonging to the same sector of activity, identified by their APE code, i.e. 1,997,832 controlled companies.

In order to increase the similarity between the control group and the test group, we choose to restrict the samples to the companies present in the 2019 Fare vintage. This choice allows us to avoid dealing with the bankruptcy situations of the companies, in the test group as well as in the control group, but above all to keep only the companies with a long-term activity, whose evolution can thus be analysed. Thus, with this decision, only 1,701 enterprises are retained in the test group, i.e. 90% of the enterprises in the sample. However, on the side of the control enterprises, this decision allowed us to keep only 1,054,053 enterprises, i.e. 53% of the enterprises in the sample. This drastic restriction bring the profiles of the companies in the control group and the test sample closer together. Indeed, it seems that the CLCs member companies have a more durable period of activity than a large proportion of the other French companies and this choice makes it possible to limit this type of difference.

Finally, two last steps of data restriction consisted in removing the data statistically imputed by the teams producing the Fare files for some companies and in keeping only the observations of enterprises aged at least one year and with a turnover different from 0. The imputations are particularly important for microenterprises, i.e. companies composed from only one individual, which are very present in the test sample. This restriction decreases the number of enterprises in 2019 in the test sample to 1,215 and in the control group to 784,846. The choice to keep only enterprises older than one year is explained by the comparability of the activities of enterprises in their first year. Indeed, some enterprises may have been created at the beginning of the year and others in the last half of the year and therefore do not have the same number of half-years to compare in their first year of existence, in particular in order to measure their own evolution with respect to the following year. The restriction to turnover figures other than 0 comes from the hypothesis that a turnover equal to 0 is similar to an absence of activity that year, without being linked to an immediately productive problem. All these choices result in a test sample of 1,281 firms in total, of which 1,182 can be found in 2019.

Tableau 2 - Nombre d'entreprises par millésimes de Fare

Year	**Open in 2019**		**Not imputes**		**Turnover ≠ 0 and Age >0**	
Sample	**Test**	**Control**	**Test**	**Control**	**Test**	**Control**
2010	663	495 933	543	386 197	529	360 121
2011	751	571 869	593	427 042	571	396 867
2012	807	624 037	651	457 040	629	423 809
2013	912	694 300	679	489 192	650	452 246
2014	1 017	765 746	734	519 841	709	478 718
2015	1 104	832 515	825	582 267	793	533 862
2016	1 257	930 083	912	631 779	872	575 630
2017	1 425	1 062 458	1 028	691 898	988	625 651
2018	1 570	1 197 250	1 089	748 978	1 053	668 560
2019	1 701	1 442 609	1 215	784 846	1 182	699 205
Observations	11 207	8 616 800	8 269	5 719 080	7 976	5 214 669

2. Methodology

The 9 files of the Fare data enable to construct a longitudinal database whose panel structure can be a real asset for identifying effects. The panel data thus make it possible to include individual fixed effects, controlling for unchangeable characteristics of firms that can explain both their membership of a CLC and their economic trajectory, such as the personality of their manager or their customer target. In this type of model, the control group is essentially useful for measuring as accurately as possible the external cyclical variations captured by a time fixed effect. It is therefore important to obtain a control group with sufficiently similar characteristics to the test group, in order to be convinced that the variations in the activities of the firms in the test group would have been on average similar to those of the control group, in the absence of the use of a CLC.

We therefore proceed with a two-stage identification strategy. The first step is dedicated to the selection of a control group using probabilistic nearest-neighbour matching, similar to the strategy used by Quantin, Bunel and Lenoir (2021) for their evaluation of the effects of the Young Innovative Company scheme, also using the Fare file with heterogeneous entry dates into the schemes. The second step consists in applying a double fixed effect model to the final sample (Imai and Kim 2021).

a. The selection of the control group by matching

The first step is therefore to select a credible control group, in order to take into account in the estimation of the variations in activity that the CLCs member companies might have had if they had not joined the schemes. To do this, we use the matching method based on observed characteristics used by Quantin, Bunel and Lenoir (2021), in order to approximate as closely as possible the characteristics of the control group to those of the test group, in particular their turnover trajectory prior to joining the CLCs of the test firms. As the dates of entry and first observations in the Fare file were heterogeneous, we applied a matching model by cohort, defined by the first year of observation and the year of entry of the test companies. Potential controls were thus selected on the basis of their characteristics in the year of the first observation of the test firms and the year before they joined a CLC.

We selected three times as many controls as test firms in each cohort based on their propensity score. After various tests of methods, we opted for a classical nearest neighbour model, with distance measured by propensity score, itself estimated by logit regression. However, we forced an exact match by CLC region, the control having to be located in one of the employment zones of the CLCs in the cohort, by sector of activity in 17 categories and with a creation date of more or less 5 years similar to that of the test companies in the cohort. The objective of this model is not to predict the probability of a company joining a CLC, but to select companies with similar characteristics, whose turnover would have a similar variation over time outside of CLC membership.

The matching model used to calculate the propensity score was as follows:

$$\begin{aligned} P(CLV) = {} & \text{year of creation} + \\ & \text{Sector} + \\ & \text{Legal status} + \\ & \text{Employment area} + \\ & \text{Municipal density} + \\ & \text{Turnover}_{t1} + \\ & \text{Change in turnover}_{t2\text{-}t1} + \\ & \text{Number of employes}_{t1} + \text{Number of employes}_{t2} + \\ & \text{Profit}_{t1} + \text{Profit}_{t2} \end{aligned}$$

P(CLCs) is the probability of entering a CLC. In each cohort, the three controls per test observation are selected according to how close they are to the model score, combined with the restrictive conditions discussed above. If none or fewer than three controls score sufficiently well or meet the restrictive conditions, only those controls meeting the various conditions are selected. This procedure results in a sample of 3,368 control firms for the 1,281 firms in the test group.

In order to check the contribution of this sampling method and its potential impact on the final results of the study, we also selected a random control group of 3,843 firms.

In addition, due to the high variability of turnover in the upper echelons of the distribution sector, which affects the average turnover between samples, we chose to remove the 1% of companies with the highest turnover in the first year of observation in the Fare file, i.e. a turnover of more than €16,000. We therefore obtained a final sample of 1,268 test companies, 3,334 matched checks and 3,821 random checks.

The descriptive statistics for the different samples confirm the similarity between the characteristics of the matched and test samples, compared with the random sample (Table 5).

Table 3 - Descriptive statistics of the sample

Indicator	Test (n=1268)	PPM Control (n=3 334)	Random control (n=3 821)
Mean turnover			
t1	439 857	399 169	361 963
t2	561 248	462 000	X
Median turnoer			
t1	151 385	156 295	97 920
t2	178 925	160 565	X
Number of employees			
t1	3,8	2,6	2,6
t2	4	2,5	X
Municipal density			
1	49%	52,3%	66,1%
2	20,3%	24,6%	17,5%
3	27,8%	21,7%	15,5%
4	2,9%	1,4%	0,8%
Area			
1	9,7%	10,2%	4,5%
2	8,4%	6,8%	2,7%
3	38,1%	30,5%	2,6%
4	3,9%	3,5%	1,9%
5	15,5%	22,1%	10,2%
6	8,1%	8,5%	3,5%
7	4,6%	11,4%	35,7%
8	6,7%	6%	3%
NA	0,5%	0,9%	35,9%
Sector			
C1	11%	7%	1%
C5	3%	2%	1%
DE	0%	0%	1%
FZ	2%	2%	8%
GZ	35%	36%	14%
HZ	1%	1%	4%
IZ	21%	19%	8%
JZ	3%	3%	5%
KZ	0%	0%	3%
LZ	1%	1%	6%
MN	9%	15%	22%
OQ	7%	10%	18%
RU	7%	6%	8%
Legal status			
1 Individual entreprise	19%	25%	35%
5 Commercial society	78%	74%	62%
6 Other moral person	1%	1%	2%
9 Private groupment	1%	0%	0%

As the variable of interest in the study is turnover, we have analysed its distribution between the different samples in more detail.

Tableau 4 - Décile de chiffre d'affaire

	All observations			**First year of observations**			**Year before joining a CLC**	
Decile	Test	PPM	Random	Test	PPM	Random	Test	PPM
Min	210	-3 780	-126 030	690	-850	-28 960	690	-850
10%	42 805	36 800	26 597	33 677	30 332	18 140	37 916	32 012
20%	75 142	63 320	46 085	58 562	55 384	36 570	65 464	57 702
30%	114 037	90 523	66 170	80 361	81 255	52 110	93 190	82 488
40%	165 826	128 580	90 676	111 942	113 968	71 420	135 654	117 952
50%	238 525	178 135	127 385	151 385	156 295	97 920	178 925	160 565
60%	337 118	248 412	181 992	211 128	214 264	138 390	261 652	225 304
70%	473 080	355 424	278 607	298 981	300 976	205 846	381004	316 465
80%	746 800	550 126	463 480	463 858	449 808	336 240	588 978	503 804
90%	1 439 504	1 087 142	1 035 555	878 066	876 117	738 280	1 170 864	976 484
Max	20 590 230	39 034 140	87 537 610	14 863 570	15 466 770	15 580 730	16 288 630	31 551 810

It can be seen that in the first year of observation in the samples, the distribution of turnover of the enterprises in the test group and the matched control group is close, more so than in the random sample. The characteristics of the test and matched samples diverge more in the years before joining a CLC, without causing extreme differences, except for the maximum, showing potential divergences in evolution. Information on the year before joining is only available for the matched controls, due to their selection by cohort, and not for the random control group. However, both samples will be used to estimate the models in order to compare the results.

b. The two way fixed effect model

The identification strategy of the effect of using a CLCs uses the panel structure of the data. As the CLCs entry dates are heterogeneous and range from 2012 to 2020, a standard double-difference model comparing the test group with the control group before and after a scheme entry and assuming similar variation can not be used.

In the absence of a common entry date for all test observations, it is not possible to define when the control group would have been treated if they had joined a CLC and thus compare their performance with that of the test group after treatment. The common solution in this case is the double fixed effects model (Stevenson and Wolfers 2006; Hoynes, Schanzenbach, and Almond 2016; Goodman-Bacon 2021; Callaway and Sant'Anna 2021), which consists of adding individual fixed effects to the linear model, allowing to control for all the invariant and unobserved characteristics of individuals that could influence both their economic activity and their choice of membership in a CLC, and annual fixed effects, allowing to control for the effects of economic conditions influencing both the test and the control groups. The fixed effects thus make it possible to reduce the risk of variable omission, at least for individual and unchangeable characteristics.

Table 7 - Treated and untreated enterprises in the test group by year of observation

Year	Not yet treated	Already treated
2010	517	0
2011	560	0
2012	596	22
2013	528	111
2014	539	159
2015	534	247
2016	480	380
2017	427	549
2018	305	736
2019	132	1 038

The linear model estimated using the R package plm (Hsiao 2014) is as follows:

$$\text{Turnover} = ß_1 \text{ IdMLC}_{it} + ß_2 \text{ Characteristics}_{it} + c_i + t_t + \varepsilon_{it}$$

IdMLC is an indicator taking the value 1 when the company is a member of a CLCs and 0 when it is not. The time-varying control characteristics are

- Demographic: age, statistical category of firm size, sector of activity, legal status of the firm, number of full-time equivalent employees
- Spatial: employment area, CLCs area and municipal density in 2018

Most of these characteristics show little temporal variation, however, over ten years of observations, companies sometimes move and evolve and these changes seem important to take into account in their development process. The interpretability of the control coefficients is however rather weak, as they potentially reflect more the effect of change than that of status, sector or geographical area.

The matched control group also allows for the addition of a variable from the matching method and thus brings the control group firms closer to the test group firms with which they were matched. Thus, if each test firm does not have at most three directly dedicated control firms, we know which control firms were chosen for each cohort. This specification allows the variable T1 to be added to the model, taking the value 1 for all firms in a cohort when the test group firms in the cohort have joined a CLCs and 0 the rest of the time.

The double fixed effect model has a particular interpretation. It consists of calculating for each variable in each observation year, their deviation from the individual's mean for that variable. It thus measures the correlation between the variations of the dependent variable at the individual mean with the variations of the other characteristics of the individual at their individual mean. The addition of a time fixed effect makes it possible to remove from this first difference the annual variations of each year with that of the average of the years.

$$(TO_{it} - TO_m - TO_{mt} + TO_m) = ß(x_{it} - x_{im} - x_{mt} + x_m) + (c_i - c_{im}) + (\varepsilon_{it} - \varepsilon_{im} - \varepsilon_{mt} + \varepsilon_m)$$

It thus removes the invariant characteristics ci, since ci is constant $c_i - c_{im} = 0$, as well as their correlation with the explanatory variables and the individual and time invariant error terms. The new conditions of validity of the model are then that the covariance of the variation of the variable of interest with respect to its mean with the variation of the individual residuals varying in time with their mean is equal to 0.

$$\mathrm{Cov}((x_{it} - x_{im} - x_{mt} + x_m),\ (\varepsilon_{it} - \varepsilon_{im} - \varepsilon_{mt} + \varepsilon_m)) = 0$$

This condition remains relatively strong, since a change in unobserved and variable firm characteristics that affect turnover, such as a change in management, may well also influence the choice of using a CLCs for example.

To test the appropriateness of using the fixed-effects model, it was compared with a simple linear model and a random-effects model, which assumes that the individual and invariant error terms are uncorrelated with the explanatory variables and therefore do not need to be removed. The Fisher test comparing the fixed effects model and the simple linear model is significant. The results of the two models are therefore different, proving that the fixed effects are not zero. Similarly, the comparison of the fixed-effects model with the random model is carried out using a Hausman test (Hausman 1978), testing the similarity between the two models. As the test is not significant, the null hypothesis of similarity is rejected and the random effect model is considered unreliable compared to the fixed effect model.

We also tested the heteroscedasticity of the fixed-effects model using a Breush-Pagan test (Breusch and Pagan 1979), which tells us that the data are heteroscedastic. Similarly, we found that the residuals of the regressions suffer from autocorrelation. These two findings prompted us to calculate the precision of the estimated parameters by taking into account individual and temporal aggregations, through the use of a correlation matrix incorporating these two dimensions (Cameron and Miller 2015; Thompson 2011), using the vcovDC function of the plm package (Hsiao 2014).

3. Results

We systematically tested the model with the control selected by matching and with the randomly selected control, to determine if this choice was important or not.

Table 5 - General results

Model	Control PPM	Random Control
Absolute variation		
Without T1	39 516 ; s.e. = 21 752	49 821 ; s.e.= 38 357
With T1	62 470* ; s.e. = 26 136	T1 non disp.
Logarithmic		
Without T1	0,11*** ; s.e. = 0,02	0,15*** ; s.e. = 0,02
With T1	0,09** ; s.e. = 0,03	T1 non disp.

The general model, with all observations, does not give significant results. There is a positive trend in the effect, but the variance is too high to be able to conclude convincingly that the effect is strictly greater than 0 and, above all, precise.

Incidentally, we also estimated the model by transforming the dependent variable, turnover, into logarithmic form. This transformation enable to estimate the variation in turnover as a function of the model's parameters. Applying the transformation to obtain the percentage of variation to the estimators obtained, we obtain an average increase of 12% in turnover linked to the use of a CLC with the matched control sample and 16% with the random sample. It is also interesting to note that the results with the two samples are close enough to be consistent, but that the matched sample gives slightly weaker effects, potentially due to the closer proximity of the company profiles to those of the test sample and therefore taking better account of cyclical effects.

This difference in significance between the absolute and rate of change results leads us to the hypothesis that despite the limitation of turnover to the lowest 99%, a high variability in high turnover, potentially without causal link with the use of a CLC, could bias the average of the absolute effects. By looking at variation, very strong absolute effects on high turnover and potentially just temporally correlated with CLC use but not really explained by it become less important and bias the estimators less.

We therefore decided to use the model on sub-samples created on the basis of company size. We selected all the companies that have ever had the chosen status, i.e. microenterprises, small and medium-sized enterprises (SMEs), intermediate-sized enterprises (ISEs) and large companies. These two categories are grouped together because of the small size of the remaining sample.

Tableau 6 - Results according to the companies size

	Control PPM	Random Control
Microentreprises :		
Absolute	34 064* ; s.e. = 13 884	43 501** ; s.e. = 13 645
Logarithmic	0,09*** ; s.e. = 0,02	0,11*** ; s.e. = 0,02
Small and medium companies :		
Absolute	214 811** ; s.e. = 78 312	180 710 ; s.e. = 155 973
Logarithmic	0,12*** ; s.e. = 0,03	0,15*** ; s.e. = 0,04
Intermediary and large companies :		
Absolute	-881 553 ; s.e. = 712 805	-680 716 ; s.e. = 1 028 013
Logarithmic	0,00 ; s.e. = 0,08	0,11 : s.e. = 0,11

The above hypothesis seems to be confirmed on the sub-samples. This time we observe small but significant effects for microenterprises, around €34,000 per year, but larger effects for small and medium companies, consistent with their size. The rates of change are similar, at around 10%. For intermediary and large companies, the effect becomes negative and insignificant, both in absolute terms and in terms of the rate of change, confirming the greater volatility of turnover in the upper echelons of distribution and the much less perceptible effect of CLCs for this type of company.

These differentiated effects allow us to propose interpretations of the effect of CLCs on activity. Thus, it is possible that microenterprises and small and medium companies, with smaller production volumes, benefit more from inclusion in a territorial network in terms of the internalisation of demand. Their production potentially corresponds more to activities oriented towards the domestic sector and perhaps responds more to local demand, which the CLCs are more successful in redirecting. Similarly, the effect of CLCs, even if small in magnitude, has a larger relative share in the initially smaller turnover of these companies and is therefore more easily perceptible and significant. Thus, in the context of intermediary and large companies, the marginal contribution of CLCs is potentially invisible in the face of an already very large volume of production. Moreover, the variation in the activity of these large companies is potentially subject to important exogenous events not causally linked to the use of CLCs, but which may occur simultaneously with their use and have a strong impact on the turnover of certain companies.

In order to complete these initial results, we conducted additional analyses to study a possible differential effect of the size of the CLCs and their time of use.

To do this, we created a first variable separating the CLCs into three categories. The first is the Eusko alone, due to its number of user companies being at least twice as high as the others. The second combines the CLCs with between 400 and 500 user firms, i.e. the Cairn, the Doume and the Gonette, and the third the remaining CLCs, with less than 300 user firms.

The model is run on the whole sample, but replacing the indicator of membership of a CLC with this variable.

Table 10 - Results by CLC size

CLC Size	**Control PPM**	**Random Control**
Absolute		
Big	14 811 ; s.e. = 30 231	28 739 ; s.e. = 38 597
Medium	12 292 ; s.e. = 27 373	24 536 ; s.e. = 37 046
Small	133 249 . ; s.e. = 68 565	132 501 : s.e. = 97 754
Logarithmic		
Big	0,1*** ; s.e. = 0,02	0.13*** ; s.e. = 0,02
Medium	0,11*** ; s.e. = 0,03	0,14*** ; s.e. = 0,04
Small	0,14*** : s.e. = 0,03	0,19*** ; s.e. = 0,04
Microentreprises		
Big	21 481 ; s.e. = 25 057	31 845 ; s.e. = 25 391
Medium	31 177 ; s.e. = 22 566	40 038. ; s.e. = 22 587
Small	67 834 . ; s.e. = 36 249	76 218* ; s.e. = 35 780

With these specifications, the absolute effects observed are not significant, partly because of the smaller sample sizes for each modality of the variable of interest and the high variability within each category. However, it is interesting to note that there are significantly larger effects, both in absolute terms and in terms of the rate of change for the small CLCs members. These are the only ones for which the effect is weakly significant in absolute terms with the matched control group and in both samples for microenterprises. Although the low significance of these results does not allow us to draw very strong conclusions, we can nevertheless propose an interpretation of these differences in magnitude.

For example, it is conceivable that, contrary to popular belief, small CLCs have a greater effect on the activity of the firms using them. This greater effect could be due to a network effect. Large CLCs, by integrating many providers, potentially reduce the number of additional clients for each user firm. Thus, in a large network, consumers and businesses have more choice in how they spend their CLC units and member businesses are therefore more likely to compete to meet this demand. For example, it is highly likely that several firms will have similar activities in the network and will therefore compete for the additional customers brought by the use of CLCs, and other characteristics of the firms, such as their location, reputation or prices, may come more into play. In a smaller network, users are more constrained in their choice and the acceptance of CLCs is potentially a more important criterion for the selection of a provider or supplier. This hypothesis leads us to consider the question of the optimal size of a CLCs and its territorial coverage. For example, would a CLC covering all the businesses in a locality have any effect on its users?

Another, potentially complementary, interpretation can also be considered. Some currencies might rely heavily on social and transactions networks that are already implemented in the territory. However, the existence of prior interpersonal networks, while facilitating the establishment of the CLCs, can limit their own effect. For example, the recruitment of service providers by going up the chain facilitates the circulation of money, but does not directly provide new customers for the businesses, since the suppliers of the user businesses are recruited. The CLCs are therefore superimposed on a network of pre-existing transactions and have a weaker intermediation and internalisation effect on transactions. It can therefore be assumed that in the context of a small CLC, the difficulties of the CLCs in developing may be due, among other things, to a less developed social network. Thus, the CLCs participate more in the activation of proximities and the creation of an ad hoc community and would therefore have a greater economic impact on the members of this new community.

Although this result cannot be given the firmness of a conclusion at this stage, it does allow new hypotheses to be put forward, which it would be interesting to study in greater detail during more in-depth case studies. It also makes it possible to question the quasi-systematic study of the Eusko as a model currency, and for which this thesis was no exception. In the end, the results obtained may not be so much overestimated because of the exceptional size and location of this currency, but perhaps underestimated because of its particular territorial context and the size of its network.

Similarly, we created a variable categorising the time spent in the CLCs between the first year, the second year and more than two years.

Table 11 - Results by time in the CLCs

Time in CLC	Control PPM	Random Control
Absolute		
0-1 year	35 251* ; s.e. = 16 952	44 946 ; s.e. = 29 709
1-2 year	50 716 . ; s.e. = 29 134	56 685 ; s.e. = 38 454
+ de 2 year	35 992 ; s.e. = 31 628	50 865 ; s.e. = 57 049
Logarithmic		
0-1 year	0,08*** ; s.e. = 0,01	0,11*** ; s.e. = 0,01
1-2 year	0,12*** ; s.e. = 0,02	0,16*** ; s.e. = 0,02
+ de 2 year	0,15*** ; s.e. = 0,02	0,2*** ; s.e. = 0,02
Microentreprises		
0-1 year	27 380* ; s.e. = 12 675	33 240** ; s.e. = 12 509
1-2 year	37 466* ; s.e. = 14 781	47 001*** ; s.e. = 13 287
+ de 2 year	40 685 . ; s.e. = 21 545	55 869* ; s.e. = 22 622

As with the previous complementary results, we have difficulty in finding significant absolute effects, notably because of the small sample size within each category and the high variance of the dependent variable. The rate of change effects are significant and increasing, in similar orders of magnitude to those found in the previous model specifications. Focusing on microenterprises, the effects are more significant due to the lower variability of turnover in this sub-category, as in the original model. As before, the coefficients are not precise enough to interpret their difference robustly. However, for microenterprises at least, there appears to be an increase in the effect over time between the first and second year of use. This difference may be due to the time spent using the CLCs in the first year, as some firms may have joined in the last quarter of the year and thus observed almost no effect, while others will have already had a full year of use by the time they report. The coefficient for companies that have been members for more than two years is even less accurate than for the other two categories. Except in the full sample model with matched control, where it is much lower than that of firms using CLCs for more than one year, it remains at a level relatively close to the category that precedes it. It is therefore not possible at this stage to conclude either that the effect of using a CLCs has decreased or increased over time.

4. Discussion

In this work we measured the effect of using a CLC on the companies turnover. To do this we used the natural experiment of their self-selection into a CLC to assess changes in their turnover before and after this event.

We obtain relatively large and significant results, although not very precise, for micro, small and medium-sized companies. The magnitude of the results, in the order of €30,000 for microenterprises and €200,000 for small and medium-sized companies, should be interpreted in the light of the turnover scale declared by the companies (Table 6). Indeed, the average turnover in the first year of observation of the companies and in the year before joining the CLCs varies between €350,000 and €550,000 and the median turnover is between €100,000 and €150,000. As a result, the rate of change effects are quite high, ranging from 8% to 16% increase in turnover between the years when a CLC is used and the previous years, which are statistically significant results. This amplitude seems to us to be particularly strong, especially when put into perspective with the feelings expressed by the companies in the Mouvement Sol survey, where 59% of the companies said they had not observed any effect on their turnover and 33% declared a marginal effect. However, it is possible that companies do not perceive the link between the increase in their business and membership of the CLCs. Indeed, as mentioned above, users often do not pay in CLCs to their suppliers or providers who accept CLCs and are chosen for this. As a result, companies may not be aware that this new customer base is due to their acceptance of CLCs.

Furthermore, we only have fiscal information from firms and while the double fixed effect model best controls for unchanging firm characteristics and aggregate business cycle effects, it is possible that the uptake of CLCs is correlated with a changing firm characteristic, whether it is a change in management or production methods, an adjustment to poor firm performance or conversely an additional commitment for firms in a growth period. All of this information is missing and constitutes potential omitted variable bias, which the dual fixed effects model is not sufficient to correct.

Moreover, this lack of more qualitative information on the companies is reflected in the selection of the control group. The control group is essentially used to calculate the annual fixed effect, which removes the effects of the business cycle from the measure. The matching model does allow for the selection of a control group with characteristics closer to those of the random sample of companies tested. However, there are unknown characteristics of the test companies that are unchangeable and that may explain their use of a CLC. These characteristics, such as a commitment to the organic production, which has been growing in recent years, or an interest in cooperating with other territorially-based businesses, may also have an effect on variations in their economic activity. The matched enterprises, although very similar in terms of known characteristics, may have different profiles on these dimensions and thus have different economic trajectories over the years of the study, and not only because of the non-use of CLCs. The use of the matched control group already reduces the magnitude of the measured effect compared to the random sample. It could therefore be assumed that a better control group would improve the measurement of annual business cycle effects and could reduce the size of the estimated coefficients. For future research, it could be envisaged to pre-select control firms for fewer CLC cases, but based on a more detailed qualitative knowledge of their territory, which would then be selected in Fare in the same way as the list of CLCs member firms.

We also decided to avoid the management of bankruptcies and attrition by keeping only the companies still active in 2019 in the population studied and therefore potentially with a more solid activity. While this choice reduced the control population much more than the test population, it potentially removed firms with declining trajectories from both sides, on which we cannot therefore estimate any effects. Similarly, the absence of associations and agricultural enterprises from the Fare file reduces the population analysed significantly and unfortunately does not allow us to generalise our results beyond the non-agricultural market sector.

Despite these methodological limitations, these results are encouraging. Indeed, such a study had never been conducted before and the question of the contribution of the use of a CLC for businesses is central, both for the actors in the CLC field, the public authorities who may choose to support this type of project or not, and for academic research, for whom these conclusions provide information that was previously lacking regarding the effectiveness of CLCs. Thus, the measurement of a significant positive effect at least for small businesses raises the question of the use of CLCs as economic development tools.

This first observation opens up the field of questioning on the network effects and the coverage rate of CLCs. For example, at what level of coverage of all the businesses in a locality or territorial community would the CLCs no longer have a positive effect? Similarly, is the use of a CLC by certain actors in a locality and the increase in their activity at the margin to the detriment of the activity of other companies in the area? All of these questions open up new avenues of research, which we chose not to address in this thesis, but which it could be interesting to address in further research, possibly beyond CLCs to other phenomena of territorial intermediation.

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