Is Trust an Ultimate Cause? Its Role in the Long Run Development of Financial Markets in France

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Trust, it has long been argued, can facilitate economic transactions that make people better off and thereby have an enormous impact on economic growth (Arrow, 1972). Trust ought to have such an effect, if it means that people can invest without spending a great deal to keep from being defrauded. And its consequences should be particularly important in financial markets, for investors inevitably put their money in other people's hands.

There is some evidence that trust does work this way. A measure of trust taken from questionnaires is correlated with more rapid economic growth (Knack and Keefer, 1997). It also seems to explain individuals' willingness to invest in financial markets that cannot be traced back to differences in their wealth or their attitudes toward risk and ambiguity (Guiso, Sapienza, Zingales, 2005). But what in turn determines trust? It is usually presumed to reflect some form of "social capital," such as norms that discourage abuses of trust or social networks that facilitate punishment of untrustworthy behavior. The usual (though not universal) assumption is that this social capital will changes very slowly, if at all, in a given society, but it will vary a great deal from place to place.[1] The same will therefore be true of trust. And there is considerable evidence from surveys and from experiments that social capital and trust do in fact vary across societies and even within regions of a single country (Putnam, Leonardi, Nanetti 1993; Henrich et al., 2004; Guiso, Sapienza, Zingales, 2004, 2006). There is also evidence that the two are linked in financial markets (Guiso, Sapienza, Zingales, 2004, 2005).

If trust is generated by social capital and if it does in turn significantly affect financial markets, then its effects should be visible both today and in the past. More social capital (be it stronger norms to discourage fraud or more effective networks to punish deviants) will mean more trust and hence–if other things are equal–more lending and investment, no matter what the year. And if social capital changes slowly, then so will trust, and the effects of trust on lending or investment will remain the same over long periods of time.

As yet, no one has subjected this argument to a thorough statistical test.[2] In particular, no one has verified–at least quantitatively–that the relationship between trust and social capital holds in the past or that the effects of trust persist over time. The statistical evidence usually advanced in favor of trust mattering and of its being generated by enduring social capital simply cannot speak to this issue, for it all comes from current day observations--experiments, modern surveys, and cross sectional regressions–and thus cannot answer questions about the past or about change over time. Historical evidence would be an obvious way to see if the relationships observed today bear up over long periods of time, but no one has ever subjected them to the right sort of statistical scrutiny.

The omission is hardly a matter of mere antiquarian interest, for it raises fundamental questions about trust, social capital, and even about policy. If the sort of relationship between social capital and financial development that we see today were to end up disappearing in the past, then we would in fact have to rethink what trust in financial markets might be. We would either have to admit that in the long run trust does not matter in financial markets, or we would have to conclude that it is not generated by fixed or slowly changing social capital. It would have to come from some other source, at least in financial markets.

Historical evidence is critical here. Without it, we cannot tell how durable the effects of trust are, a matter that is essential for policy. Simply knowing that trust and lending are linked at one moment in time is not enough. If, for instance, the relationship between trust and lending does not vary across the centuries, then trust is unfortunately not a variable that can be changed to promote better social outcomes. At the other extreme, if trust is simply the way individuals respond to the returns on a certain form of human capital, then it is an effect, not a cause, and the connection will run from anemic loan demand via trust to a low level of lending, making policies about trust per se unnecessary. Finally, if trust and its effects are persistent, but the effects decay over time, then it will be an important policy variable.

Analyzing how trust, credit, and time interact requires a panel of observations with historical data. It also requires eliminating as many other sorts of variations in transactions costs as possible. The best way to do that would be to examine different markets within a single country. The legal costs of discouraging untrustworthy behavior will be virtually the same throughout the country, and hence trust, and the social capital that generates it, will have more of a role to play in explaining variations in the level of lending or investment between different markets. If trust and social capital change slowly, then their effect on lending or investment should remain the same over long periods of time. And the relationship between trust and social capital should hold in the past too.

Testing whether social capital generates trust in past financial markets and whether the effects of trust persist across time is therefore essential, and we perform such a test in this paper. We focus on lenders' trust in borrowers. Although it is not the only form of trust, it is essential for nearly all financial transactions, which typically involve a lender or other investor advancing money in return for a promised payment in the future, and all of our tests will concern this form of trust. The evidence for the tests come from 108 credit markets in one country–France–over a period of nearly two centuries. The markets we look at are precisely the kind that are stepping stones to economic development in poorer countries, and because they all lie within the same country, we can hold constant the judicial and political institutions that affect lending and in particular the legal costs of preventing or punishing fraudulent behavior. The next two sections of the paper develop the model of lending that underlies our statistical tests and present our data. We then carry out the tests and discuss their implications. We find that social capital has no significant effect on trust in the past, or at least the trust in borrowers that we consider here: it does not explain levels of lending that cannot be accounted for by economic variables. Furthermore, although there are persistent differences in the level of lending that can conceivably be interpreted as trust, they have no relationship to social capital, and they may well derive from other causes (such as informal institutions or the acquired expertise of financial intermediaries) and have nothing to do with trust at all. The implication, which we take up in our conclusion, is that trust in financial markets may be generated by something other than social capital; alternatively, it and social capital may only matter in certain settings. In particular, trust becomes important in settings where formal support for credit is low because of civil conflict, widespread corruption, or ethnic discrimination. The evidence from France suggests one might be better off treating the root of the problem rather than seeking to improve trust.

A Simple Model of Trust

How then should we conceive of trust in financial markets? Perhaps the easiest way to think of it is via an extremely simple economic model, one that helps pin down what it means when we say that trust should encourage more lending and investment. The model requires a few simple equations, and readers who prefer can simply jump to the end of the section because the underlying idea is quite simple. It is in fact merely the claim that, other things being equal, more trust ought to translate into more lending. Precisely how much money is lent out will of course depend on many things, including interest rates, borrowers' demand, and the amount of collateral they possess. But if these other factors are held constant, then more trust ought to mean that lenders are willing to advance more money to borrowers. Such a willingness is a prerequisite for most financial transactions, and it is essentially what is measured on experiments on trust. Via our model we can see whether this trust changes over time and whether it is related to social capital.

Imagine then a society in which individuals can either borrow or lend; such an imaginary society is not too far removed from reality when financial markets are underdeveloped, as in the past or in developing countries today, particularly when lending moves beyond the narrow confines of a family or village. Each individual in this society has some wealth w (the amount may be small or large). He can either lend this money out and earn interest i on the loan, or use it as collateral to borrow and set up a small business. If he sets up the small business, he will earn a rate of return r, which will depend on local demand and his talents as an entrepreneur. This rate of return r will be the same no matter how much he borrows; if he takes out a loan of size x, he will therefore end up with a return of $(1+r)\ x$ on the loan. We assume that there are a large number of individuals, so that any one of them will take the interest rate i as given (see the appendix for formal version of what follows).

Having earned this sum from his business, the entrepreneur will have to decide whether to default on his loan or pay it back. To keep things simple, let us assume that he has only these two possibilities–total default or repayment–and that if he defaults, he will face a penalty Tw that will be proportionate to his wealth w. If T is larger than 1, then a default will cost him far more than the assets he owns. Such a penalty, which goes beyond his wealth alone, could be interpreted as evidence of trust, as would be the case if borrowers who defaulted not only lost all their assets but faced social sanctions for violating social norms. Similarly, a T smaller than 1 might be interpreted as a lack of trust, although it could reflect other causes as well. A legal system that allowed a defaulting borrower to retain a large fraction of his assets would lead to a small T for reasons having nothing at all to do with trust.

When it comes time for the borrower to repay, he thus faces a choice between giving his lender $(1 + i)\, x$ or defaulting and forfeiting the penalty Tw. The borrower will default if $(1 + i)\, x > Tw$; knowing this, lenders will limit their loans to an amount $x = Tw/(1+i)$. Individuals who can set up businesses that will earn a rate of return $r > i$ will borrow up to their credit limit of $Tw/(1 + i)$; individuals with lower rates of return will lend their wealth out at the interest rate i. If the distribution of rates of return r among individuals in the market is given by the distribution $G(r)$ and the distribution of wealth among the same individuals is given by $F(w)$, then the fraction of individuals who lend money out when the market interest rate is i will be $G(i)$, and the average amount lent per person will be

$$G(i)\, E(w \mid r \# i), \qquad (1)$$

where $E(w \mid r \# i)$ is the average wealth of individuals with rates of return less than i. Similarly, the average amount borrowed per capita will be

$$(1 - G(i))\, T\, E(w \mid r > i) / (1 + i), \qquad (2)$$

where $E(w \mid r > i)$ is the average wealth of individuals with rates of return higher than i. To avoid complications, we shall assume that *F* and *G* are independent; in other words, an individual's skill as an entrepreneur has nothing to do with his wealth. For our simple model, this is not an unreasonable assumption, provided we think of the return *r* as innate talent as an entrepreneur.[3] Since *F* and *G* are independent, the average amount lent per person will be

$$G(i)\,E(w) \qquad (1')$$

and the average borrowing per person will be

$$(1 - G(i))\,TE(w)\,/\,(1 + i), \qquad (2')$$

where $E(w)$ is simply average per-capita wealth. Average borrowing per person will therefore be a function of the interest rate; average wealth; the distribution *G* of rates of return, which will reflect both the supply of entrepreneurial skills and the demand for entrepreneurs' services; and, last but not least, *T*, which is our trust measure but could reflect other factors as well, such as the legal system. A higher *T* implies–other things being equal–more credit per capita: the same fraction $G(i)$ of people take out loans, but loan sizes increase. In practice, *T* will end up being something of a residual, for we will measure it using a regression to filter out the amount of per-capita lending that cannot be accounted for by the economic variables or by legal institutions, which will be held constant if we remain in one country. But *T* is nonetheless what trust in financial markets ought to be—lending that goes beyond what collateral alone would justify.

So far we have only dealt with a single market and made the interest rate *i* exogenous. What happens when there are several such markets, each of different size and each with its own particular measure of trust and its own distribution of wealth and of entrepreneurial talent? (These distributions of wealth and talent may differ from market to market, but once again we assume that in each market they are independent.) If we suppose that funds can be transferred between markets, then there will be an equilibrium interest rate i^* that will equate the supply of funds to lend and the demand for loans and that will be common to all markets. Funds will flow from markets with low trust to markets with high trust, and if we measure average borrowing per person in each market, in market j it will be

$$y_j = (1 - G_j(i^*))\, T_j\, E_j(w) / (1 + i^*), \qquad (3)$$

where G_j is the distribution of talent in the market, $E_j(w)$ is the average value of wealth in the market (calculated using the market's distribution of wealth F_j), and T_j is the market's trust measure. Note that the logarithm of y_j will be

$$\ln(y_j) = \ln(1 - G_j(i^*)) + \ln(T_j) + \ln E_j(w) - \ln(1 + i^*) \qquad (4)$$

The first term, $\ln(1 - G_j(i^*))$, will depend on the interest rate, the supply of talent, and local demand conditions, and in a regression we would expect it to be a linear function (at least to a first order approximation) of the logarithms of variables such as the interest rate; measures of local talent, such as literacy rates; and local indexes of demand, such as urban populations and the number of banks. Similarly, $\ln(T_j)$ will (at least approximately) be a linear function of the logarithms of measures of social capital embodied in norms or social networks, if trust is in fact related to social capital. Even if T_j is also affected by the legal system, the portion of T_j that is a function of legal or political variables will presumably be constant (and hence will be swept into the constant term) if we confine ourselves to data from one country, since legal and political institutions will then be the same for all markets.

The model is most easily interpreted as explaining what we would observe at one point in time in a cross sectional regression. In such a regression, the equilibrium interest rate will also be a constant, and the coefficients of the social capital measures will reveal whether trust T_i is related to social capital. But the model can easily be extended to observations of the same markets at different times, with the trust T_i in each market either being fixed or varying over time. We can test whether T_i is fixed using panel data, which can also tell us whether the relationship between trust and social capital persists across time. Both sorts of regressions can easily be run using the historical data we have collected.

Data for 108 French Credit Markets;1740 to 1899

The data we have gathered concern over two hundred thousand loans drawn from 108 credit markets scattered through France (see Figure 1 for a map and Table 1 for summary data on their populations). The markets were chosen to yield a stratified sample of towns and cities that would reflect the French population as a whole. The markets included Paris; other big cities such as Lyon; medium sized urban centers with 10 to 70 thousand habitants, such as Grenoble; and smaller towns with populations as low as 500 people. The loans in each market were drawn up by notaries (semi private court officers who preserved records and also provided legal and financial advice) and were subject to a tax. The notaries had to register the loans at the local tax office, where officials collected the tax and recorded information about the debts. We gathered information from the offices' archives, which covered lending in the municipality where the office was located and in surrounding towns and villages. Although there were certainly some small debts that did not appear in these archives, most lenders had a powerful incentive to report their loans to the offices and do so truthfully, for otherwise they would have had difficulty pursuing defaulting debtors in court. Unregistered debt was therefore likely to be minimal, though the exact amounts at stake cannot be measured precisely.[4]

For this paper we leave aside information about the identities of the parties involved in the loan contracts and focus on the number of new loans, average loan sizes, and loan durations; we then use this information to estimate the stock of outstanding debt in each market for 6 years: 1740, 1780, 1807, 1840, 1865, and 1899. (See the appendix for details about the data collection and the estimation process.) The dates of these estimates were chosen to be roughly a generation apart, with two dates (1780 and 1807) bracketing a devastating bout of hyperinflation during the French Revolution. The first date, 1740, was the earliest one for which we could collect data on lending and explanatory variables for all of our markets; the last date was the latest one for which we could get access to the records needed for the data collection.

One might naturally worry whether the French Revolution altered legal and political institutions so greatly that our data might be affected after 1807. Institutions did certainly change, but not enough to distort our data. The court system was reorganized, which perhaps reduced the costs of litigation, but the law governing credit contracts remained much the same. The same was by and large true for notaries, and it held as well for the officials who collected a tax on loans and registered them. Indeed, the nature of the tax and the size of the areas covered by each office hardly changed.[5] In any case, we can check whether the revolutionary turmoil made a difference by running our regressions for 1840-99, when the legal and political institutions were all in place and uniform across the entire country. It turns out that results do not depend on whether we use the data from the years 1840-99 or the entire sample.

The loans in our 108 markets were mortgages and business loans with durations running from a few months to several years or more (see Table 1 for summary data on the loans and on the variables used in our analysis). In 1740, 30 percent of them took the form of life or perpetual annuities, which lenders invested in for support in old age or to create a stream of income that profligate heirs could not dissipate. The rest were term loans with an average duration of slightly over 2 years. The annuities disappeared in the nineteenth century, while loan durations grew to almost 9 years by 1899. The sums involved were in total very large. If we extrapolate from our sample to France as a whole, the outstanding stock of this debt was perhaps 21 percent of GDP in 1740 and23 percent of GDP a century later. By 1899, it reached 44 percent of GDP. Even so, many of the loans involved only modest sums of money. Outside of Paris, for example, the median loan size was 500 francs in 1840, or roughly a year's earnings for a day laborer, and the records are full of smaller loans: 100 francs that the vintner François Meunier and his wife borrowed in 1840 from their neighbor, the laborer François Gressin, or 160 francs that the laborer Etienne Desgens owed the landowner François Poubeau in the same year.[6]

Unfortunately, the tax records we used to gather our data often omitted the interest rate charged on the loans.[7] Eighteenth-century term loans were particularly likely to leave out the interest rate, although it was usually 5 percent when it was indicated. Mention of the interest rates was more common in the nineteenth century, with 5 percent being the modal figure. The gap in rates between different markets was greatest in the 1899 cross section, when 90 percent of the records give an interest charged on the loan. At first glance, the figures from 1899 might seem to cast doubt on our assumption that our loan markets were integrated, because the average interest rates in distinct markets differed by as much 1 percent. Yet in 80 percent of the markets the averages fell in a narrow range between 4.22 and 4.73 percent, which suggests that the assumption is not unreasonable, particularly since the gap in rates between markets was smaller in our other cross sections. Furthermore, lenders participated in multiple markets, providing yet another sign of integration. Even in Lunel, the market with highest average interest rate in 1899—4.97 percent—there were in fact lenders who came from the cities of Montpelier and Nîmes. Such piecewise integration was in fact the norm in our credit markets.[8]

What about the collateral backing the loans, which is also an important part of our model? The original contracts preserved in the notarial archives describe the collateral in great detail, since providing such information was essential part of the notaries' service. But our fiscal records often gloss over that information too, which was not needed for collecting the tax, and going back to the original contracts to gather it would have slowed our data collection to a crawl.

We have, however, been able to examine the original contracts in several specific markets, and in these markets, loans with a duration of a year or more almost always involved real estate as collateral. In 1740, for example, annuities were nearly always collateralized on specific pieces of land or other real property, but shorter term loans (with a typical duration of 3 to 12 months) mention no specific collateral at all By 1780 specific real collateral did begin to appear in these term loans, which came to resemble modern day mortgages with balloon payments, and in the nineteenth century more than three quarters of all contracts involved mortgaged real estate of some sort. As for the remaining loans without any mention of specific collateral, they did usually give a general claim to the borrower's assets in case of default, but the loans backed by liens on specific collateral were paid off first, leaving lenders with general claims to share what was left.

The per-capita stock of outstanding loans in each market is our measure of y_j, borrowing per person in our model. It was calculated by dividing our estimates of the loan stock by the population in the surrounding canton, a small region that included the municipality where the loans were registered plus nearby towns and villages.[9] The calculation thus yields a panel of data, with a y_j for each market j and for each of our six cross sections from 1740 to 1899.

Trust and Social Capital

Our model implies that the y_j can be regressed on social capital measures and economic variables to see whether there is a persistent effect that can be interpreted as trust and, if there is such an effect, whether it is related to social capital. The regressions can either be panel regressions or cross sectional regressions for one of the six individual years. A panel regression addresses the question of trust's persistence and it can also be used to explore the relationship between trust and social capital. But a series of cross sectional regressions at different times can do the same; if one finds an effect that seems like trust in one cross section but not others, then it is difficult to maintain that trust is persistent. Normally, the panel regression is preferable to the cross sections, but the lack of data for certain periods makes it worth examining the cross sections too.

To make it clear how the regressions will be specified and what hypotheses will be tested, let us return to our model. Whether we are using panels or cross sections, our regressions will always be based on equation (4). For the cross sections, the regressions to estimate will have the form:

$$ln\,(y_j) = a + b\,x_j + c\,z_j + u_j \quad (5)$$

Here j is the index for each of the 108 markets; u_j is the error term; a, b, and c are matrices of regression coefficients; x_j is a matrix of logarithms of measures of wealth and local demand conditions, which come from a first order expansion in logarithms of the terms $ln(1 - G_j\,(i^*))$ and $ln\,E_j\,(w)$ in (4); and z_j is a matrix of logarithms of social capital measures or other variables correlated with trust, which are derived in a similar fashion from the term $ln(T_j)$ in (4).

The panel regression will be similar except that the interest rate i^* will enter the regressions too, as a variable that varies over time but not from market to market.[10] The panel regression will also include fixed (or random) effects w_j that measure persistent characteristics of market j; since a persistently larger $ln(T_j)$ in market j might yield a larger w_j in a first order expansion, these fixed or random effects could be interpreted as long lasting trust. All the other variables in the panel regression can depend on both the market j and the date of the cross section t; formally, the panel regression will be:

$$ln\,(y_{jt}) = a + b\,x_{jt} + c\,z_{jt} + d\,v_t + w_j + u_{jt} \quad (6)$$

Here v_t is a matrix of dummy variables for each time period (except one) that captures changes in the interest rate, and x_{jt} and z_{jt} are analogous to the corresponding matrices of variables in (5).

Regression equations 5 and 6 will be used to test two hypotheses: first, that trust was related to social capital in the past, and second, that trust is persistent across time. If the first hypothesis is true, then the coefficients of the social capital variables in z_j should have large and significant coefficients with appropriate signs when the cross sectional regressions (5) are run with our historical data. If the second hypothesis is true, then either the w_j in the panel regressions should be large or the correlates of trust in z_{jt} should have large and significant coefficients with the appropriate signs.[11] If both hypotheses hold, then the social capital variables in z_{jt} should have large and significant coefficients with the expected sign in the panel regressions.

To see what these expected signs are, let us introduce all of our variables. The per capita value of the property tax serves as our wealth measure; it is available only for the 1840, 1865, and 1899 cross sections. (See the appendix for a description of the sources for these and the other explanatory variables and table 1 for summary data.) If a wealth measure is included in the panel regressions, they can therefore only be run for these 3 periods.[12] The local demand measures include the population of the municipality where the loans were registered, and the number of banks, which was not available for 1740 and 1780. Presumably migration would have swelled the population of the cities where demand was strong; note that these municipal populations are different from the canton population that was used to figure per capita wealth and the per capita loan stock. Banks would have opened in such cities too, making their number yet another index of demand. The banks specialized in providing short term mercantile credit, and because banking was essentially unregulated, they tended to open in markets were merchants were thriving, making their presence a good index of demand.[13] By the 1830s there were hundreds of firms or individuals (many of the individuals were wholesale merchants who offered trade credit on the side) providing such commercial banking services. About a third of them were in Paris and the rest primarily in the largest cities. As the century wore on, the banks spread rapidly through France's smaller cities. In 1829, only two out of every three cities with populations over 20,000 had a bank office; by 1851 all of them did. For cities between 5,000 and

10,000, the fraction with banks jumped from one third in 1829 to 87 percent in 1862.

Of these three variables, wealth is exogenous (at least in our simple model), and because the number of banks and the city populations are likely to be endogenous, we have used their lagged values in all of our regressions. We also have one measure of entrepreneurial talent–namely literacy rates, which were measured in the years 1820-30. Like the demand measures, literacy will affect the expression $ln(1 - G_j(i^*))$.

We have several social capital measures, with each one indexing a particular form of social capital. (It proved impossible to find other useable measures of social capital.[14]) Only one of the social capital indices varies over time; the others capture social capital at one specific date. Two of the measures concern Catholicism, which was, at least nominally, the religion of most French people. The first religious measure measures the extent to which local Catholic priests took an oath of allegiance to the French revolutionary constitution. The oath was required of all clergymen in 1791, but substantial numbers refused, with the opposition varying considerably from place to place (Tackett, 1986). The higher the numerical score in scale, the greater the fraction of the priests who took the oath, and the regions where most clergymen took the oath tended to be those where ties to orthodox Catholicism were weak and remained so throughout the nineteenth century. By contrast, the places where the clergy refused the oath were so strongly attached to Catholicism that all sorts of behavior was affected for decades–in particular, the use of birth control. The reasons for the differences were historical. In many areas where the clergy took the oath, not only had religious devotion faded away by the outbreak of the French Revolution, but shortly thereafter the clergy was removed during the revolutionary campaign against Catholicism. Parishes then went without priests for years, and when they finally returned (often it was not until well into the Napoleonic Empire or even later, after restoration of the monarchy in 1814**)**, the anti-clerical citizens were hardly inclined to listen to their pastors' advice. Meanwhile, in the regions where the clergy spurned the oath, most priests had gone into hiding and ministered to the faithful in private when they were persecuted. In these hotbeds of counter-revolution, the priests were hailed as heroes when orthodox Catholicism was restored, and their parishioners faithfully heeded their admonitions from the pulpit.[15]

Religious norms would thus exert more influence over behavior in the places where the clergy rejected the oath and where Catholicism remained robust–in other words in areas where the oath score was low. What would the norms say about repaying debts? Worries about usury had long since withered away in Catholicism, at least at the level of daily life, and failure to pay a legitimate debt would presumably be tantamount to theft, provided of course that the borrower was not facing some dire emergency (Hoffman, Postel-Vinay, Rosenthal 2000; Dumas 1935-65; Noonan, 1957). If so, then religious norms would pressure borrowers to uphold their end of the bargain, and logarithm of the oath scale would enter both panel and cross sectional regressions with a large, negative coefficient (see Table 2 for a list of the social capital measures and their expected sign in cross sectional and panel regressions).

There is, however, a second and different way that the oath measure could conceivably enter the regressions. Where priests refused the oath, religious norms should have been effective, but the contrasting anticlerical regions should have had a strong attachment to the secular norms of the Revolution and, later, the French Republic, which would entail respect for state courts and legal contracts. In areas with intermediate scores, however, it would not be clear whether either norm applied, and lenders would presumably have to exercise more caution. Such areas were likely to be divided between two mutually suspicious and even hostile camps: on one side, the royalist Catholics; on the other, the anti-clerical republicans.[16] Unfortunately, we cannot actually trace out the membership of camps since borrowers and lenders did not identify themselves as royalists, Catholics, republicans, or anti-clericals. But we know that they did exist from realistic nineteenth-century novels. In Balzac's *Illusions perdues*, for instance, the clergy and royalist officials in Angoulême simply cease doing business with the printer David Séchard when rival printers falsely accuse him of atheism and republicanism; he nonetheless continues to work for the city's merchants, lawyers, and notaries, whose liberal opinions place them in the opposite camp. Should one trust such evidence? Dismissing it simply because it comes from a novel seems foolish. Balzac after all was describing a city and a business that he knew well and he was in fact striving for accurate detail.[17]

Members of these two different camps would be leery of dealing with one another, particularly when something like lending was involved, for social networks would likely not cross the divide. How could, say, a republican lender pull strings in a royalist's social network in order to get a loan repaid? As a result, fewer loans would be made in places with such intermediate scores, because lenders would often have to rely on the legal system alone, and not on social networks. If so, then a dummy variable for places with such intermediate scores would have a negative sign in both the panel and cross sectional regressions.

A third measure of social capital–vote turnout–is identical to one used in Guiso, Sapienza, Zingales's 2004 study of trust in financial markets. For them, high vote turnout is a sign that behavior is guided by norms or by networks that punish aberrant behavior. One argument in favor of such a claim is that narrow economic self interest alone cannot justify turning out to vote, since the time spent voting cannot be justified by the infinitesimal odds that one's ballot will sway the outcome. This is a classic example of a free rider problem, and high voter turnout suggests that it has been overcome. Norms or other punishment of misbehavior would be one way to achieve such a solution.

We have turnout for one election only. Many other elections were held, but to guarantee that the voting statistics would be comparable, we have restricted ourselves to instances where the voting met four conditions: the elections had to all be of the same type, the electoral districts had to be nearly identical to our markets, the suffrage had to be the same and as broad as possible, and voters must not have faced pressure to spurn the election and stay home. Concretely, these conditions limited us to general elections (and not local ones) with universal male suffrage, and we further excluded cases in which the turnout was extremely low in some districts, which we took to be a mark of pressure on voters. We were left with the elections of May 1849, in which voters cast ballots for representatives in the Second Republic's new Assembly; in the future, however, we may include other elections as well.[18] If the arguments about norms are correct, a high turnout in the 1849 elections should boost T and thus have a large positive coefficient in both the panel and cross sectional regressions.

Draft resistance provides a fourth measure of social capital. Conscription works when draftees trust their government or are at least committed to following its orders (Levi 1998). Conceivably, trust in the government or commitment to its rules might translate into respect for legal agreements, such as loan contracts. If so, then indexes of draft resistance should enter into the regressions with a large negative coefficient.[19]

We have two such measures. The first is simply the fraction of the draftees who failed to report in the years 1820-30. Although failure to report when called might appear an unambiguous sign of draft resistance, it could also reflect something quite different. The draftee could be dead, in prison, or away working as a migrant laborer, as often happened in regions where large numbers of young men left to work part of the year as itinerant masons. As a result, this particular measure captures other phenomena besides draft resistance, and it was in fact highest in areas with large populations of migratory laborers.

The second measure of draft resistance is much more clear cut. It is the percentage of the draftees in the years 1820-30 who escaped service because they had mutilated themselves–typically, by cutting off a finger. Apart from the occasional accident, the meaning of self-mutilation is unambiguous. And where such self-mutilation was common, T should have been low. The same should presumably be true for the other measure of draft resistance. Both variables should therefore have large negative coefficients in the panel and cross section regressions.

Our final measure of social capital is a legal one–the per-capita number of verdicts against defendants in civil and criminal trials, which we can measure in 1840, 1865, and 1899; it is our one measure of social capital that varies over time. We simply measure guilty verdicts in criminal trials and add to that the number of civil judgments against defendants; the total is then divided by the population. If the measure is high, then either there is a great deal of crime (relative to the population), or many legal transactions end up in court. Either way, trust would presumably be low, leaving a variable that should have large negative sign in the regressions.

Those are our measures of social capital; their expected signs in the regressions (if trust is related to social capital) are shown in table 2. In addition, we have two further independent measures of trust in borrowers that do not necessarily have any connection to social capital. They will be useful for testing whether trust in borrowers persists in credit markets even if it is unrelated to social capital. The first of these two measures is the share of outstanding loans in 1740 that were perpetual annuities; the second is the same measure, only in 1780. Again, the perpetual annuities involved a lender's giving money to a borrower in return for an annual stream of payments, which (at least in theory) could continue forever since it was the borrower alone (or his heirs) who decided when the principal was to be repaid. As we have noted, loans of this sort were common in 1740 and 1780, and they required that a lender enter into an open-ended and long term commitment to a borrower. Such a commitment was arguably a sign of trust, because it would be hard for a lender to know what would happen to a borrower's collateral in the years after the loan was first made. The share of such annuities in the total of outstanding loans (either in 1740 or in 1780) thus provides yet another measure of trust, but one that is not necessarily tied to social capital.

As a measure, it is a particularly interesting one, because it allows a possible test of the persistence of trust. Consider the share of perpetual annuities in 1740. If it entered the regressions with a positive sign in periods after 1740, this could conceivably be taken as evidence that the same trust which allowed lenders to make a long term commitment in 1740 was still working years later. If, however, the regression coefficient sign is zero or negative, then trust in 1740 apparently exercises no hold in later periods. In that case, trust, if it did play a role in financial markets, would simply not have any lasting effects; in other words, trust itself would vary. Similar arguments could be made about the share of perpetual annuities in 1780 if it is used as a variable in later periods.

For historical reasons, we might actually expect that this sort of trust shown in 1740 or 1780 might well fade away, particularly for the share of perpetual annuities in 1780. Lenders who entered into perpetual contracts in that year ended up losing heavily during the hyperinflation of the French Revolution (Hoffman, Postel-Vinay, Rosenthal, 2000). Their loans were all repaid in worthless paper money, and their long term commitment, which could be taken as a sign of trust, was sorely abused. It would not be surprising if they and their descendents refused to make such commitments in the future; their reluctance would then register in the regressions as a lack of trust. But as we shall see, that is not the only possible test for the persistence of trust.

Results

Let us then examine the evidence from the regressions. Again, we have two hypotheses to test: first, that trust was linked to social capital in the past, and second, that it persists across time. The first hypothesis requires non-financial measures of social capital, in order to get around the problem that low levels of social capital in financial markets may simply reflect low demand for loans. The second hypothesis requires panel data. We begin by assuming that the two hypotheses are both true, using panel regressions. We then move to repeated cross-section regressions to test the first hypothesis alone and explore the relationship between trust and social capital. We then return to panel regressions to test the second hypothesis by itself and see whether trust is persistent.

Let us first test whether both hypotheses are true. The test amounts to seeing whether the social capital variables in z_{jt} have large and significant coefficients with the signs shown in Table 2. The dependent variable in the panel regressions is the logarithm of the per-capita stock of outstanding debt; the other explanatory variables are logarithms of wealth (since our wealth measures are not available before 1840, it is omitted in regressions with cross sections before that year) and of measures of demand, including the number of banks and the population of the municipality where the loans are registered. Because at this stage we have no wealth measures for the first three periods and no count of banks for the first two, the regressions are run with all the variables for the period 1840-99, and without banks and wealth for 1740-1899 (Tables 3 and 4, regressions 1 through 7). The panel regressions are estimated random effects when our social capital measures do not vary over time.[20] Some of these time invariant social capital measures come from the period of the Revolution and some from the middle of the 19th century, but each one is measured only once. All the coefficients have a simple interpretation: they measure by what percentage the dependent variable would change if the independent variable increased by 1 percent (in economists' terms, they are elasticities).

The panel regressions show that our measure of lending per capita is clearly related to local conditions. Larger cities have large average outstanding debt, as do places with more banks and markets where taxes per capita are higher—precisely what our model of credit rationing would suggest. As far as our two hypotheses are concerned, however, the regressions argue against them both. Only one of the social capital coefficients turns out to be statistically significant and have the sign we would expect if trust were persistent and linked to social capital (Tables 2, 3, 4). That one coefficient comes from *failure to report*, our somewhat ambiguous measure of draft resistance, which could well reflect something besides social capital. If it did reflect social capital–if, for instance, a willingness to serve meant people had confidence in formal institutions–then we would expect a negative coefficient not only for *failure to report*, but for our other, less equivocal measure of draft resistance, *self mutilation.* But it in fact has a positive coefficient. As for the other social capital measures, *verdicts* has a coefficient with the wrong sign. Oath scale, the turnout in the 1848 elections, and our measure of religious divisions (*intermediate score*) do all have the expected sign, but neither one is close to being significant. Furthermore, virtually all of the coefficients are small, suggesting that even if a larger sample yielded more precise estimates the connection between trust and social capital would be economically trivial.[21] And the conclusions are the same whether we look at full panel or only at the data from 1840 on; they are therefore not the result of institutional change during the French Revolution.[22]

Let us set aside the second hypothesis temporarily and focus on the first one, which asserts that trust is tied to social capital. The regressions here are cross sectional, and they resemble those run by other researchers in that they take a reduced form approach to evaluating trust, using instruments culled from non-financial measures of social capital. Our non financial measures are in fact quite close to what others have employed. But there are two differences between their work and ours. The first is that we have no direct measure of trust, such as the surveys that some contemporary researchers employ. That is perhaps a weakness, but it is offset by a second difference, which works in our favor. That second difference is that we follow financial development over a period of a century and a half, something contemporary researchers cannot do.

Table 5 gives the results for a sample of our cross sectional regressions. Like the panel regressions, they provide little evidence in favor of a close link between trust and social capital, even though we can augment the set of control variables for several regressions. If we run the cross sectional regressions for 1865, for instance, we can add a measure of entrepreneurial talent (illiteracy, as measured among military recruits) and a measure of wealth inequality (the fraction of tax payers who had enough property to be eligible to vote in 1840).[23] The economic variables are jointly significant and generally have the predicted sign, except for banks once we control for wealth and inequality (through the number of males rich enough to vote in 1840 and illiteracy, which is correlated with poverty as well as entrepreneurial talent). Overall the economic variables explain 46 percent of the variance in the logarithm of per-capita lending.

Yet even with these additional variables, the social capital measures still fail to have coefficients that are both statistically significant and have the expected sign for the 1865 cross section (Table 5, regressions 2 through 7). The results are similar if we run the regressions for the other cross sections. Although we do not report all the results here, the only one that comes closest to demonstrating a relationship between trust and social capital is the 1840 regression with *failure to report*, our ambiguous measure of draft resistance (Table 5, regression 8). It has the right sign and the odds that it is a statistical fluke are less than 0.082. But again the measure itself is questionable, the coefficient is small, and in any case, this single result is a weak reed on which to stand an argument about the connection between trust and social capital. In fact it is remarkable that only one of several dozen coefficients (because we can run these regressions going all the way back to the Revolution) is statistically significant.

It thus seems that if trust does play a role in credit markets over the long term, it has little to do with social capital. Trust and social capital may be connected in certain markets today, but the connection is not universal, and in particular, it did not always hold in the past, even though the set of social capital variables was large. One could of course raise doubts about such a conclusion, arguing that our data are measured with error and that our regression coefficients are therefore biased toward zero. Or one could make a similar argument about omitted variables. But if so, why do we continue to find no convincing evidence in favor of social capital in 1865 and in 1899, when the data are quite accurate, the relevant political institutions have long been in place, and we can include additional variables as well? Furthermore, our data avoid some of the obvious drawbacks of the surveys that other researchers on trust use. Our evidence comes from real transactions in which people had sizable sums of money at stake—a big difference from surveys. True, our measure of trust in borrowers is something of a residual, but the lack of any relationship between it and conventional measures of social capital suggests that social capital is simply not essential for generating financial trust. The results may of course be different for the sort of small consumer debts that never appear in our data base, but that sort of credit was much less important than the lending in the markets we studied. And in them, social capital was not related to financial trust.

If the evidence thus argues against the first hypothesis, what about the second one, that trust is persistent? There is at least some conceivable support for it. If we run the panel regressions without social capital measures, we can then use fixed effects to measure persistent differences between markets. The fixed effects turn out to be appreciable: they explain a significant fraction of the variance in the per-capita stock of lending (between 63 and 67 percent), and an F-test shows that the odds of all the fixed effects being zero is less than 0.0001 (Table 6, regressions 1 and 3).[24] The panel regressions thus imply that there are large and persistent differences between markets that our economic variables (wealth and the demand measures, banks and city size) cannot account for. The persistent differences may be interpreted as evidence that trust exists and endures over time, but it should be stressed they may also reflect other causes as well, such as geographic conditions that favor economic growth, informal institutions that have nothing to do with trust, or the learning and experience of local financial intermediaries that can be passed on to their successors.

Possible evidence that the differences may have been generated by something like trust comes from two further panel regressions, which add the share of perpetual annuities in either 1740 or 1780 and look at its effect over the years 1840-99. The annuity share turns out to have a large positive and significant coefficient in both regressions. A market in which all loans in 1780 were perpetual annuities had, other things being equal, a 58 percent higher per-capita loan stock in 1840-99; for the 1740 annuity share, the impact was even higher–an 82 percent jump in the per-capital stock (Table 6, regression 5 and 6). Regressions run with the same annuity shares for longer periods (when we unfortunately lack data on banks and wealth) lead to similar results (Table 6, regressions 7 and 8).

One might be tempted to interpret these findings as evidence that trust has a lasting effect on financial markets: after all, that the annuity share in 1740 influences borrowing some 100 to 150 years later is quite a striking result, particularly since it comes from regressions from the years 1840-99, when the relevant legal and political institutions are not changing. Conceivably–so the explanation might go–individuals might have been inclined to lend money out in perpetual annuity contracts in those markets where people had developed effective social sanctions against potential defaulters. If so, then perhaps the trust formed in these markets might have became a perennial factor in the local lending culture and thus persisted for generations.

Yet while that interpretation may seem plausible at first glance, it has a fatal disadvantage: it simply cannot stand up to historical reality. In 1790, the French Revolutionary government, which was falling short of revenue, embarked on an ambitious scheme to nationalize church properties and to issue currency (assignats) backed by the confiscated wealth (Hoffman, Postel-Vinay, Rosenthal 2000). The printing presses churned out so many reams of assignats that the currency lost 99 percent of its value in a mere five years. All that would be of little importance save for the fact that for ancient legal reasons all loan contracts were nominal and thus not indexed against inflation. Essentially, the loan contracts took two forms. One type was a term loan with a short, specified maturity (typically under three years except in Paris). Lenders who had put out their money in these contracts were by and large able to get it back before the currency's value plunged. The second contract type, however, was the perpetual annuity, and repayment of the capital for these loans was at the discretion of the borrower (provided of course he continued to pay the interest due). For these contracts, borrowers could wait as inflation eroded the currency and then repay their annuities in worthless paper. The lenders could do nothing to force repayment. Our estimates for Paris are that more than 90 percent of the perpetual annuities were paid off and that the lenders lost on average 75 percent of the pre-revolutionary value of their investments. The inflation, quite simply, was a dagger aimed at the trust shown by individuals who lent money out in annuities. The connection with trust is inescapable, for borrowers could have chosen not to repay in worthless assignats and then borne the full cost of their loans once the currency had been stabilized. Although markets with high annuity shares before the Revolution may have been places where trust was high, they should have had very low trust afterwards.

If trust was important, we would thus expect lenders in markets with high annuity shares to shun lending after the French Revolution. But if that was how many lenders acted, then we should have observed exactly the opposite in the regression–namely, a coefficient that was large and negative. The positive coefficient thus begs explanation, but fortunately there is a simple alternative that makes sense of our results. Markets with large annuity shares in 1740 witnessed a great deal of lending in 1780 because they were the markets where there was more wealth and demand for credit. If the Revolution left the distribution of economic activity intact and informal cultural variables were unimportant, then we would expect markets with big annuity shares to recover swiftly and to again have sizeable demand for credit after the Revolution. And that is precisely what happened. Although bloodshed and tumult during the Revolution slowed the recovery, the redistribution of property (albeit considerable) was sudden and definitive and hence did not disrupt the economy greatly. The original property owners had little time to allow assets to depreciate much, and some of the Church property that was auctioned off was actually converted to productive use—in particular monastic buildings. The real economy quickly recovered and with it the large demand for loans in the markets with all the eighteenth-century annuities. With a delay (one that was a bit longer in Paris and other cities where annuities were popular), so did the supply of loans. By 1807 the stock of outstanding debts had already climbed to 72 percent of its 1780 value, if we use our figures to extrapolate to France as a whole.

This demand side story has an appealing consistency that the trust story lacks. For otherwise, we would have to believe that trust could somehow survive the social upheavals of an event like the French Revolution and persist without flinching for years—even though it was trust in annuities that had suffered the greatest damage from the revolutionary inflation. What could make it through such turmoil unbowed? It would simply be impossible for trust not to change. History mounts a powerful argument for persistent differences in demand and against differences in trust.

Conclusion

When we began this inquiry, we firmly believed that credit required trust and in particular that lenders had to trust borrowers, since a debtor could affect a creditor's ability to recover his capital in many ways. In prior work, we had emphasized the importance of formal institutions and human capital development in explaining the growth of lending in Paris (Hoffman, Postel-Vinay, Rosenthal 2000). In extending our research to encompass not just Paris but a large sample of cities and towns, we encountered some startling differences in the institutions that sustained lending (Hoffman, Postel-Vinay, Rosenthal 2004). It was our ambition to connect our analysis of institution with the study of trust and trustworthiness.

Yet to our surprise we found that trust was neither persistent nor firmly linked to conventional measures of social capital. At first glance, this result seems absurd given the importance accorded to trust in financial markets. It seems doubly absurd given the weight many scholars have put on the role of informal networks in financial affairs. And it may even seem ridiculous given the emphasis individuals active in financial markets have always placed on trust and on reputation. After all, trust or its abuse has been advanced to explain aggregate fluctuations (in financial crises stemming from loss of confidence), individual success (whether Rothschild or Morgan, whose fortunes were built on trust) and individual failure (individuals denied credit because of a lack of trust) (Hoffman, Postel-Vinay, and Rosenthal, 2007). That seemed true as far back as the eighteenth century and it remains true today. Yet the contradiction here is more apparent than real. Although trust is critical, it is not hard to manufacture in adequate amounts in economies and societies that are not pathological. As a result, from a statistical perspective trust in the financial arena is an intermediate variable that evolves rather quickly (at least from the sort of generational perspective that we have been investigating) in response to shocks to the demand for credit.[25]

Problems of trust do of course arise when societies have severe problems of racketeering. Indeed, in such societies—southern Italy, for example—participation in formal financial markets may well make one a target for thieves, extortionists, or embezzlers. What then should policy makers in such societies do? No policy they adopt will change an individual's trust unless it attacks the ultimate cause of the lack of trust–namely, racketeering. In this instance, as in cases of civil war or racial and ethnic discrimination, capital will spurn financial markets and flow only through peculiar channels. Individuals without access to these channels may well be shut out, and attempts to eliminate mistrust without treating the larger social problem will have little effect.

In economies that have escaped such intractable social and political problems, the problems for policy makers are different, even if the societies are poor. Capital markets in such economies may simply be under developed, and the historical experience of France and other countries in the North Atlantic suggest that in this case there are two sets of institutions that are worth nurturing: those that encourage savings and those that boost the flow of information between borrowers and lenders. Savings are important because even today most loans go to individuals or firms that have accumulated assets which can be used as collateral. Even unsecured debt is usually taken on by individuals and firms that are already rich. An important way for countries to increase lending is thus to reduce the cost of saving in the form of financial assets; once individuals have accumulated such assets, they can leverage them. Savings of this sort does require that individuals believe that their wealth is secure. That belief, which could be called trust in formal institutions, seems to have been prevalent early on in France.

The importance of institutions that help convey information is also clear, for if information flows are blocked, lenders cannot detect borrowers' misbehavior, and everyone will be encouraged to take advantage of everyone else. Reduced information can then quickly translate into generalized distrust. The institutions that facilitate the flow of information can thus heighten trust. If a country has both these institutions and the ones that foster savings, then our evidence suggests that the more informal institutions affecting trust will fall into place on their own.

Appendix

1. The model

Our model abstracts from an economy in which there are a finite number of markets for credit, with each one being large relative to the loans that any individual makes. It assumes that there are N such markets, with the j-th market having a continuum of individuals of positive mass m_j and the sum of the m_j equaling 1. The m_j are measures of the relative size of the markets, which would be relative populations if the number of actors in each market were finite. Capital flows from market to market as individuals borrow and lend; the equilibrium condition for the interest rate i is that average excess demand for loans is zero for the entire economy:

$$\Sigma_j\, m_j\, [(1 - G_j\,(i))\, T_j\;\; E_j\,(\, w \mid r > \; i) \,/\, (1 + i) \;-\; G_j\,(i)\, E_j\,(\, w \mid r \leq i)] = 0$$

where the sum is taken over all N markets, G_j is the distribution of entrepreneurial rates of return talent in market j, $T_j > 0$ is trust in the market, and $E_j\,(\, w \mid r > \; i)$ is the expected value of wealth in the market for individuals with $r > i$. This expected value is calculated with respect to the market's distribution of wealth F_j. Because we assume that the distribution of wealth and entrepreneurial talent are independent in each market, this condition becomes:

$$\Sigma_j\, m_j\, [(1 - G_j\,(i))\, T_j\;\; E_j\,(\, w\,) \,/\, (1 + i) \;-\; G_j\,(i)\, E_j\,(\, w)] = 0 \qquad \text{(A1)}$$

We assume that the distribution functions G_j are absolutely continuous and that there is a maximum feasible rate of return $R > 0$ such that $G_j(R) = 1$ for all markets j. The expression on the left hand side of A1, which we will call D, is therefore negative for $i = R$ and positive for when $i = 0$. Since D is a continuous function of i, there must therefore be at least one interest rate i^* for which A1 holds.

This i^* will be unique if there is at least one market j for which $G_j(i^*) < 1$, for then D, which is nonincreasing in i, will actually be decreasing in a neighborhood of i^*. If we assume that such a market exists and also that the density functions for the G_j are all continuous, then we can apply the implicit function theorem to i^* as a function of T_k, the measure of trust in market k. The equilibrium interest rate i^* will then be an increasing function of T_k. An increase in T_k will raise the average supply of funds $m_j\, G_j(i)\, E_j(w)$ from other markets ($j \neq k$) and reduce the average demand in these markets. Its effect on the demand in market k, however, will be ambiguous, because the rising interest rate may offset the effect of greater trust.

For our regressions, we are concerned with per-capita borrowing in each market, which in our model is $(1 - G_j(i^*))\, T_j\, E_j(w)/(1 + i^*)$. The logarithm of this expression is the dependent variable in our regressions, and to a first order approximation it will be a linear function of the interest rate, average wealth, and our indexes of demand and trust, if we assume that G_j and T_j are continuously differentiable functions of the demand and trust indices.

2. Sources for data

Thanks to generous support from the Sage Foundation, we have managed to gather data on some over two hundred thousand loans spread out over 160 years and 108 separate markets in 6 cross sections: 1740, 1780, 1807, 1840, 1865, 1899. The markets were chosen to form a stratified sample of French towns and cities according to their population; the sample includes Paris; three other large urban centers (Lyon, Rouen, Toulouse); 13 medium sized cities such as Amiens with populations between 20,000 and 50,000; and 40 smaller cities with populations between 5,000 and 20,000; and 61 towns with populations under 5000. Our evidence, it should be stressed, comes not simply from the cities and towns themselves but from the surrounding countryside as well.

In addition to the credit data, we have also collected data on financial intermediaries, populations, economic development, bankruptcies, wealth, inequality, human capital, and social capital in each of the 108 markets. Here we will describe our sources and how we estimated the per-capita stock of outstanding debt in each market.

To estimate this stock, we used records of loan registration that survive as far back as the early eighteenth century. Lenders had to have their loans registered with a local registration office and pay a tax on the transaction. If they did not do so, they would have difficulty enforcing their loans in court in case of default, and they therefore had a powerful incentive to register the loans and report truthfully the terms of the loan contract. The registration offices were located in towns and cities but they registered transactions for the surrounding countryside as well. Although the registration was reorganized late in the French Revolution, the nature of the tax and the size of the areas covered by each office hardly changed over time.[26] Typically each office covered an area that was nearly the same as a nineteenth-century French *canton*, a small administrative unit averaging some 150 square kilometers in size.

For each market and cross section, the registration records gave us the number of new loans made, the types of loans, their size, and, in most cases, their duration (the number of years before the loan had to be repaid). In the eighteenth century, data on durations in certain types of loans had to be gathered directly from the original loan contracts, which survive in the archives of notaries, the legal officials who drew up loan contracts and also arranged loans.[27]

To calculate the outstanding stock of debt, we took the new loans registered in each market in the years of our six cross sections and multiplied the value of each loan by its duration. The sum of these products is our estimate for the loan stock. The calculation assumes that the market is in a steady state, but a detailed investigation of the credit market in Paris shows this method is a good approximation. We could also calculate the fraction of loans of each type and single out particular sorts of debt, such as annuities.

The dependent variable in our regressions was the logarithm of the outstanding stock per person, which we calculated by dividing the outstanding loan stock by the population of the district that the office served.[28] Our regressions also used the population of the city where the registration office was located as an explanatory variable; again, this population was not the same as that used to compute the per-capita loan stock.

As for our other explanatory variables, the wealth measure came from property tax records in 1840, 1864, and 1899. Tax records and the Bottin Annuaire (a national guide that provided commercial, administrative, and personal information for all French cities, including those eligible to vote) also provided our measure of inequality, the fraction of taxpayers who had enough assets to be eligible to vote in 1840.[29] Literacy rates came from draft records for the years 1820-30, which in addition furnished our two measures of draft resistance.[30] The religious measures of social capital were graciously provided by Timothy Tackett; they come from his data as published in Langlois, Tacket, and Vovelle (1996). The judicial measures of social capital (the number of verdicts against defendants in civil and criminal trials in 1840,1865, and 1899) come from Compte général de l'administration de la justice criminelle en France pendant l'année 1840 (Paris, 1841), table LXXVI ; Compte général de l'administration de la justice criminelle en France pendant l'année 1865 (Paris, 1866), table LXXVIII; and Compte général de l'administration de la justice criminelle en France pendant l'année 1898 (Paris, 1899), table XXX.

Finally, the number of banks was taken from the Bottin Almanach or Annuaire for the years of our nineteenth-century cross sections. If bank data was unavailable for that exact year, we used figures for the nearest available year (data from 1829 for 1807, from 1862 for 1865, and from 1898 for 1899).[31]

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Table 1: Descriptive Statistics

1. Loan characteristics (values in 1840)			
Characteristic		All markets	Without Paris
Average loan size (francs)		3860	1847
Median loan size (francs)		---	500
Average loan duration (years)		4.62	4.68
2. Variables and measures of trust and social capital			
Variable Name	Description/Units	Mean	Standard Deviation
Dependent variables (values in selected periods)			
Per-capita stock of loans in 1740	livres	54.00	110.22
Per-capita stock of loans in 1780	livres	70.03	159.90
Per-capita stock of loans in 1807	francs	53.09	82.76
Per-capita stock of loans in 1899	francs	270.69	294.68
Social Capital Measures			
Oath scale	Index of fraction of priests taking oath of loyalty to the revolutionary constitution in 1791, with higher score on 1 to 6 scale meaning more priests took the oath	3.29	1.52
Intermediate score	Dummy variable equaling 1 if score on 1 to 6 oath scale was 2, 3, or 4; variable equals 0 otherwise.	0.59	0.49

Turnout 1849	Vote turnout in 1849 (percent eligible voters)	70.59	8.32
Resist draft: self mutilate	Local draftees (per 1000 recruits) exempted 1820-30 for loss of a finger, presumably due to self mutilation. Because it could be zero, what was entered in the regressions was the logarithm of this variable plus 0.01.	1.10	0.88
Resist draft: fail to report	Local draftees (per 1000 recruits) who failed to report in 1820-30. Because it could be zero, what entered the regressions was the logarithm of this variable plus 0.01.	6.13	12.59
Verdicts (value in 1840)	Verdicts against defendants in civil and criminal trials (per 100 inhabitants)	0.85	0.61
Verdicts (value in 1865)	Verdicts against defendants in civil and criminal trials (per 100 inhabitants)	0.67	0.23
Verdicts (value in 1899)	Verdicts against defendants in civil and criminal trials (per 100 inhabitants)	0.83	0.83
Trust measures			

Annuity share 1740	Share of perpetual annuities in total stock 1740.	0.72	0.27
Annuity share 1780	Share of perpetual annuities in total stock 1780	0.50	0.31
Other variables (value in selected periods)			
City population (in 1840)	Population of city where registration office located	19853	85734
Banks (number in 1840)	Number of banks; because it could be zero, what was entered in the regressions was the logarithm of this variable plus 1.	4.46	21.06
Wealth (value in 1840)	Property tax per 1000 people (francs)	4.69	1.35
Voters	Number of men eligible to vote in 1840	213.00	1113.22
Illiteracy	Percent of draftees who are illiterate, 1820-1830.	49.82	19.62

Source: See appendix for sources used.

Note: For detailed description of the variables, see the text and the appendix. The table shows the per-capita stock of loans for 1740, 1780, 1807, and 1899. Values for other variables are shown for the first and last dates they are available only. Monetary amounts in 1740 and 1780 were in livres, which equaled 0.989 francs, the currency unit created during the French Revolution. To get a sense of the value of the value of the amounts involved, in 1740 an unskilled day laborer earned 1 livres a day in Paris; in 1840, his wages had climbed to roughly 2.4 francs a day; and by 1899, they were nearly 5 francs a day. We did not compute median loan sizes for all markets including Paris because of the different sampling strategy used with the Paris records. The average loan durations is calculated weighting durations by loan sizes.

Table 2: Hypotheses and Expected Signs of Regression Coefficients

If trust is related to social capital	
Measure of social capital:	Expected sign of coefficient
Intermediate score	Negative
Oath scale	Positive
Turnout in 1849	Positive
Resist draft: self mutilate	Negative
Resist draft: fail to report	Negative
Verdicts	Negative
If trust is persistent	
Measure of trust:	
Annuity share 1740	Positive
Annuity share 1780	Positive

Source: See text.

Table 3: Test of Both Hypotheses: Selected Coefficients from Panel Regressions, 1840-99

	1	2	3	4	5	6	7
Variable							
City Population	0.38 4.99	0.37 4.82	0.36 4.85	0.36 4.91	0.39 5.34	0.37 5.05	0.06 0. 28
Banks	0.15 1.71	0.15 1.73	0.15 1.77	0.15 1.82	0.15 1.84	0.13 1.57	-0.12 -0.98
Wealth	0.20 1.82	0.19 1.73	0.18 1.63	0.18 1.63	0.17 1.52	0.18 1.70	0.11 0.89
Social Capital Measure	inter-mediate score	oath scale	turnout 1849	resist draft: self mutilate	resist draft: fail to report	verdicts	verdicts
Social Capital Coefficient	-0.13 -1.04	-0.11 -1.05	0.36 0.74	0.06 1.81	-0.06 -2.33	0.14 1.26	0.22 1.40
N	290	290	294	299	302	302	302
Specification of Effects	random	random	random	random	random	random	fixed
Fraction of variance due to effects	0.32	0.31	0.33	0.31	0.31	0.32	0.66

Source: See text.

Note: As explained in the text, the dependent variable is the logarithm of the per-capita stock of outstanding loans, and the explanatory variables are all logarithms, except for *intermediate score*, which is a dummy variable equal to 1 in markets that had intermediate scores on the oath taking scale. See table 1 and the text for details. *Banks* and *city population* are lagged. The regression also included dummy variables for the different cross sections to capture changes in the interest rate and other time varying effects. Coefficient estimates are on the top of each cell, T-statistics estimated standard errors are below.

Table 4: Test of Both Hypotheses: Selected Coefficients from Panel Regressions, 1740-1899

	1	2	3	4	5	6	7
Variable							
City Population	0.55 12.28	0.55 12.19	0.55 12.65	0.54 12.63	0.55 13.04	0.53 12.46	0.54 12.46
Social Capital Measure	inter-mediate score	oath scale	turnout 1849	resist draft: self mutilate	resist draft: fail to report	verdicts in 1840	verdicts in 1865
Social Capital Coefficient	-0.02 -0.15	-0.06 -0.67	0.13 0.31	0.03 0.88	-0.04 -1.75	0.10 1.20	0.05 0.35
N	498	498	505	513	518	518	518
Specification of Effects	random	random	random	random	random	random	random
Fraction of variance due to effects	0.31	0.31	0.30	0.30	0.29	0.30	0.30

Source: See text.

Note: Because the variable *verdicts* is not available before 1840, regression 6 was run using its value in 1840 for the entire panel; regression 7 did the same, using value in 1865. The other explanatory variables and the dependent are as in Table 3, except that *banks* and *wealth* are omitted. Coefficient estimates are on the top of each cell, T-statistics are below.

Table 5: Is Trust Related to Social Capital? Selected Cross Sectional Regression Coefficients

	1	2	3	4	5	6	7	8
Year of Cross Section	1865	1865	1865	1865	1865	1865	1865	1840
Variable								
City Population	0.27 1.66	0.28 1.64	0.25 1.48	0.28 1.65	0.25 1.53	0.26 1.61	0.26 1.65	0.33 2.10
Banks	-0.08 -0.59	-0.09 -0.64	-0.06 -0.43	-0.06 -0.38	-0.07 -0.49	-0.08 -0.57	-0.08 -0.57	-0.01 -0.07
Wealth	0.38 1.39	0.41 1.40	0.37 1.30	0.36 1.23	0.35 1.26	0.34 1.23	0.35 1.29	-0.01 -0.04
Voters	0.24 1.83	0.24 1.72	0.24 1.81	0.21 1.51	0.25 1.84	0.27 2.03	0.25 1.88	0.19 1.40
Illiteracy	-0.34 -1.67	-0.36 -1.71	-0.34 -1.62	-0.31 -1.45	-0.34 -1.65	-0.30 -1.48	-0.39 -1.84	-0.51 -2.26
Social Capital Measure	none	inter-mediate score	oath scale	turnout 1849	resist draft: self mutilate	resist draft: fail to report	verdicts in 1840	resist draft: fail to report
Social Capital Coefficient		-0.05 -0.28	-0.19 -1.26	0.13 0.19	0.06 1.24	-0.05 -1.26	-0.13 -0.94	-0.07 -1.76
N	90	86	86	87	89	90	90	91
R^2	0.46	0.45	0.46	0.45	0.47	0.47	0.47	0.47

Source: See text.

Note: There are two new explanatory variables in some of the cross sectional regressions. The first is *illiteracy*, the logarithm of the illiteracy rate among local military recruits, as measured in the years 1820-30. The second is *voters*, the fraction of tax payers in 1840 who were wealthy enough to meet the property requirements for voting; it is a measure of inequality, since it rises as the distribution of wealth becomes more unequal. Both of these new variables enter the regressions as logarithms. For further details about the two new variables, see Table 1. The dependent variable in the regressions and all the other explanatory variables are as in Table 3, with lagged values (from the previous cross section) of *banks* and *city population* . The regression also included a constant term. Coefficient estimates are on the top of each cell, T-statistics are below.

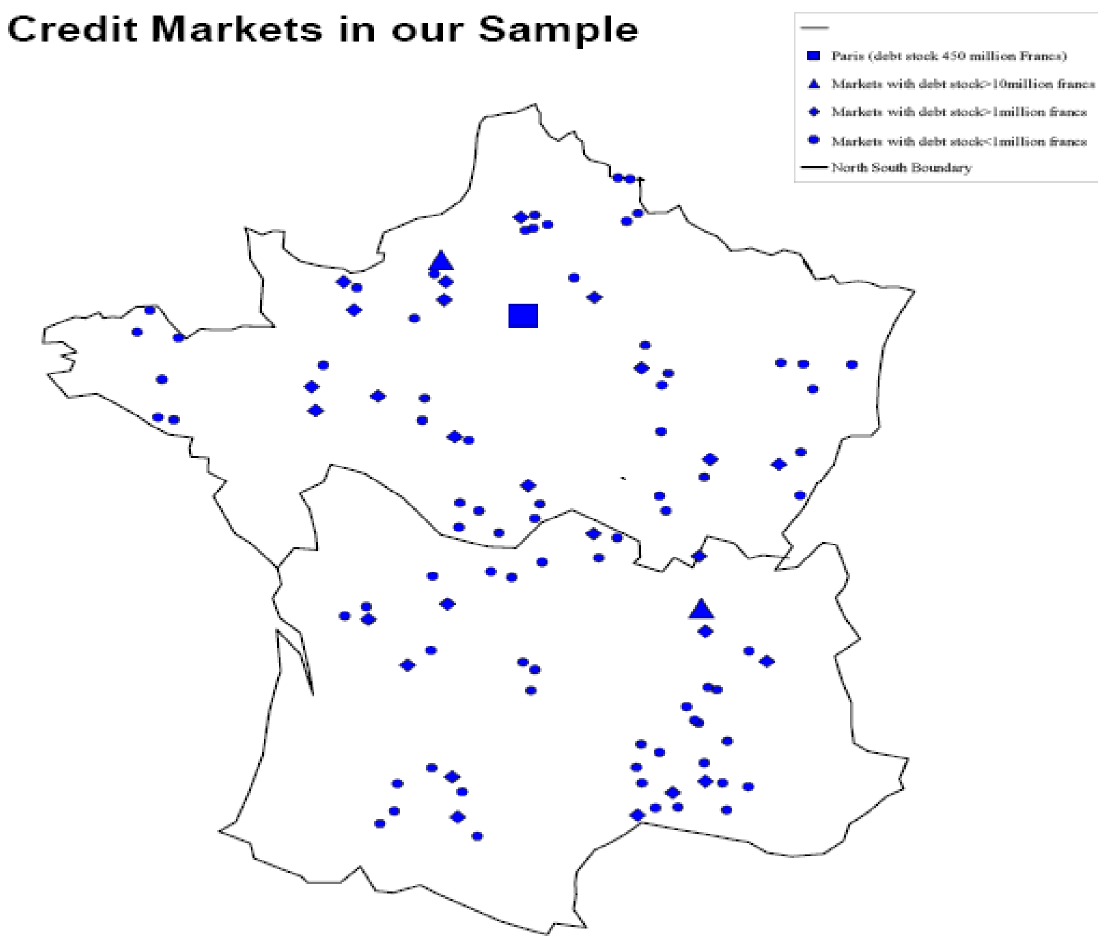
Table 6: Is Trust Persistent? Selected Coefficients from Panel Regressions

	1	2	3	4	5	6	7	8
Panel	1740- 1899	1740- 1899	1840- 1899	1840- 1899	1840- 1899	1840- 1899	1780- 1899	1807- 1899
Variable								
City Population	0.005 0.03	0.54 12.83	0.03 0.16	0.36 5.03	0.34 4.70	0.36 5.09	0.50 12.00	0.51 12.01
Banks			-0.12 -0.97	0.15 1.76	0.13 1.58	0.14 1.66		
Wealth			0.10 0.79	0.19 1.75	0.15 1.40	0.18 1.71		
Trust Measure	none	none	none	none	annuity share 1740	annuity share 1780	annuity share 1740	annuity share 1780
Coefficient of Trust Measure					0.82 3.93	0.58 3.17	0.78 4.27	0.54 3.22
N	518	518	302	302	299	299	514	407
Specification of Effects	fixed	random	fixed	random	random	random	random	random
Fraction of variance due to effects	0.63	0.30	0.67	0.32	0.28	0.29	0.26	0.29

Source: See text.

Note: Annuity share 1740 and 1780 are not entered as logarithms. All the variables are logarithms and are the same as in tables 3 and 4. Banks and city populations are lagged. The regression also included dummy variables for the different cross sections to capture changes in the interest rate and other time varying effects. Coefficient estimates are on the top of each cell, T-statistics estimated standard errors are below.

Figure 1


Credit Markets in our Sample
Paris (debt stock 450 million Francs)
Markets with debt stock>10million francs
Markets with debt stock>1million francs
Markets with debt stock<1million francs
North South Boundary

1. Putnam, Leonardi, and Nanetti (1993) argue that social capital in different parts of Italy has ancient historical roots; it thus must change very slowly, if at all. More recently, however, Miguel, Gertler, and Levine have used repeated surveys to show that social capital does seem to have changed during industrialization in Indonesia (2006).

2. Although Miguel, Gertler, and Levine (2006) explore changes in social capital during industrialization, they do not investigate the relationship between social capital and the sort of trust in financial markets that is the subject of this paper. They do examine the number of credit cooperatives, but for them it is simply one of several measures of social capital, and not an index of financial trust.

3. The entrepreneur's return r could of course be related to his education, which a wealthy entrepreneur could purchase, but we can control for that in our regressions. A more complicated dynamic model could also introduce a correlation between w and r.

4. Unregistered debt included informal consumption loans and certain forms of merchant credit that were not subject to the registration requirements. For an individual merchant, the mercantile credit could be important, but because only a small number of people took out such loans, they would count for very little in our calculation of the per-capita debt stock that is our dependent variable. They would count for even less since such debt was short term (typically 90 days or less) and our dependent variable (as we explain in the appendix) takes into account the duration of the loans. As for the informal consumption loans, they too would be small, given the incentives that lenders had to register anything sizeable.

5. For the relevant legal, political, and administrative reforms under the Revolution and Napoleon, see Woloch (1994).

6. These examples come from the registration office records for Dun-sur-Auron at the Archives départementales du Cher, 1 Q 4025 (6 January 1840).

7 Many of the original term loan contracts in the notarial archives also omitted the interest rate, simply saying, for instance, that a borrower owed a particular amount due on such and such a date.

8. The differences in interest rates could also reflect risk premiums and fees, which are not part of our simple model.

9. We used total loan stock rather than the volume of new loans because the loan stock comes closer to capturing the notion of total borrowing that is at the heart of the model. Imagine, for instance, that an entrepreneur can fund a three-year construction project to build a new factory by taking out a $1,000 loan for three years, or by taking out three successive $1,000 loans, each for one year. If we were only taking into account the volume of new loans in a single year (which is essentially what we would be doing in a one year cross section), then one of the one-year $1000 loans would count for as much as three-year $1,000 loan; if, however, we weight by duration (as in the computation of loan stock), then the three-year loan is worth three times as much.

10. The interest rate will not figure in the cross sectional regressions because it will be constant in any single cross section. It will enter the panel regression via both the term $ln(1 - G_j(i^*))$ and the term $ln(1 + i^*)$. To a first order approximation, we can expand $ln(1 - G_j(i^*))$ as a linear function of $ln(1 + i^*)$ and other variables, and thus the interest rate will appear in the panel

regression simply as a coefficient times $ln(1 + i^*)$, which we can capture by using time dummies in the panel regression. Since $ln(1 + i^*)$ is very close to i^*, we could also simply insert the average interest rate in the regressions.

11. Although one can try to test the persistence of trust by running successive cross sectional regressions for different years, the results could well be misleading, because the effects of the w_j could be mixed in the constant term with the impact of varying interest rates and other time dependent variables. One could therefore not tell for sure whether trust was changing. In the panel regression, the interest rate and the other time-varying variables can be captured by dummies for the time periods.

12. If it is left out of the regressions, there is of course the problem of omitted variables bias, as when any other variables are omitted.

13. This short term bank credit was usually not registered with the tax offices and so does not figure in our data. But as we have explained above, the omission is not likely to affect our per-capita loan stocks y_i significantly, because the mercantile loans were short term and only a relatively small number of merchants were involved.

14. As we explain below, finding useable voting data was difficult, and it is even harder to gather reliable data on guilds. The sort of information about associations that is often used to measure social capital was not available until the 1870s or later. Conceivably, distance from a given market to Paris might be considered a proxy for social capital, because Paris was the source of political and cultural change, but it was not correlated in any plausible way with our

social capital measures and it in any case had no significant effect when inserted in our panel regressions. Finally, we do have some evidence on professional and familial ties linking borrowers and lenders in a few selected markets; such ties (which were important in Paris in the seventeenth century, though not thereafter) might be considered yet another proxy for social capital. Gathering similar information for even a reasonable sample of our markets, though, would have been prohibitively time consuming.

15. For the relationship between the oath and the use of birth control (via the effect that the French Revolution had on Catholicism), see Sutherland (2003), pp. 193-94, 242-45, 345. In unpublished research, David Weir has also noticed the connection between the oath and birth control. As Sutherland points out (2003, pp. 186-198), the revolutionary campaign against traditional Catholicism was far from uniform, and as a result, the correlation between rejecting the revolutionary oath and subsequent attachment to Catholicism is not a perfect one. But there is still a correlation.

16. For other examples of this social rift as a backdrop in novels, see Stendhal, Le rouge et le noir. The divisions here may in fact have reached back to eighteenth century; see, for instance , Vovelle (1973), Norberg (1985).

17. Balzac had himself worked in the printing business and he had made several extended visits to friends in Angoulême. And he was so attentive to detail that he even asked his friends there for information about place names and a city map, which he completed by reading guides to Angoulême.

18. We leave alternative elections for further research : in particular those for the *Etats*

Généraux in 1789 and various general elections during the Third Republic. On the May 1849 elections, see Dupeux (1962) and Vigier (1963). The detailed results for 1849 are in the Archives Nationales, C 1467-1578.

19. Conscription can also be undermined by inequality, which is at least in part an economic variable. In an unequal society, average wealth will be poor measure of collateral, because many people will have little or no collateral, and per capita lending may therefore be less than what the average wealth implies. The expected effect, again, will be a negative relationship between resistance to conscription and lending, although it may be in part economic.

20. Our panel regressions always use a random effects specification when we have an explanatory variable (such as measure of trust or social capital) that does not vary across time. Otherwise, we used a fixed effects specification, although in some cases we have also shown the results with a random effects specification for the sake of comparison. Fixed effects have certain advantages over random effects, but they are inappropriate when explanatory variables do not vary over time. The random effects estimator assumes that the time invariant social capital or trust measures are not correlated with the economic variables; that may have been a problem for our trust measure *annuity share 1740*, for our social capital measure *election turnout 1849*, and for *verdicts* when its value in 1840 is used in a random effects regression in Table 4 (regression 6). On the other hand, one can argue that the random effects estimator is more appropriate for all of our regressions because we are dealing with a sample of markets.

21. One might worry that the social capital measures could be jointly significant, but if they are

all added to a random effects regression for the years 1840-1899, the null hypothesis that they jointly have no effect cannot be rejected at the 10 percent level (P = 0.1038). The test used *self mutilate* rather than *fail to report* to measure draft resistance.

22. Because Miguel, Gertler, and Levine (2006) found that out migration eroded social capital in Indonesia, one might wonder whether our panel regressions would change if we took migration into account. We can do so in a crude way by using changes in city populations as a yardstick of migration and adding an interaction term with our measures of social capital. If we do so, the results are unchanged, and the coefficients of the social capital measures are all statistically insignificant.

23. Wealth restrictions on male suffrage ended in 1848 in France; earlier in the decade only one out of 40 adult males had been eligible to vote.

24. Random effects regressions (Table 6, regressions 2 and 4) also point to sizeable w_j.

25. Our claim here parallels what Miguel, Gertler, and Levine (2006) found about the relationship between industrialization and social capital.

26. Before the French Revolution, the registration was known as the *contrôle des actes;* after the Revolution, it was the *enregistrement des actes civils publics*.

27. In a small number of cases where records were destroyed or were unavailable we had to seek other data in the notarial archives or in judicial records. That was the case, for example, in Caen, where the records were destroyed during the Normandy invasion, and in Paris, where registration did not exist in the eighteenth century.

28. For the population of the area served by each office, we used the canton population (or its geographic equivalent in the eighteenth century), which we calculated by summing the populations for the corresponding parishes (in the eighteenth century) or communes (in the nineteenth century). We wish to thank Claude Motte for graciously making this population data available.

29. Bottin and Tynna (1840). Unfortunately, 1840 was the only year when the Bottin gave the list of taxpayers eligible to vote.

30. The sources here are Aron, Dumont, and Le Roy Ladurie (1972) and the “Comptes numériques et sommaires sur les jeunes gens” for each department, which are in the Archives nationales, F9 150 through F9 261.

31. Bottin and Tynna (1829, 1840), for 1829 and 1840; Bottin (1862, 1898) for 1862 and 1898.