

The Common Currency Effect on International Trade: Evidence from an Accidental Monetary Union

Roger Vicquéry¹

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ABSTRACT

I rely on a historical natural experiment to provide, for the first time, a causal estimate of the effect of currency unions on international trade. Since the seminal paper by Rose (2000), a large literature has developed around currencies as a trade cost. However, self-selection and endogeneity bias implied by membership of a currency union are likely to be pervasive and might explain the large pro-trade effect of currency union found in the literature. I offer a quasi-experimental contribution by exploiting an exogenous variation in currency union membership, driven by an unexpected geopolitical shock – the 1861 Italian unification - and involving a French franc pan-European zone that existed throughout the 19th century. I employ original data and structural gravity equations to estimate an effect in the order of 35%, consistent with a large - if heterogenous - effect of common currencies on trade.

Keywords: Currency Unions, Common Currency, Trade, Natural Experiment, Gravity Regressions.

JEL classification: F15, F33, F54, N73.

¹ Banque de France, roger.vicquery@banque-france.fr.

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NON-TECHNICAL SUMMARY

A key economic benefit commonly associated with currency unions is an increase in cross-border flows, and notably trade. The effect of the adoption of a common currency on international trade has been found to be very large in previous studies, with average estimates suggesting an effect as high as a doubling of bilateral trade.

However, it is unclear whether such estimates are useful in guiding policy. Indeed, several factors might be spuriously biasing the measured effect upward. Countries adopting a common currency might at the same time be more likely to share other characteristics positively associated with international trade. For example, they might enjoy cultural or historical links, or an ex-ante positive trend of economic or political integration. Historical episodes of switches out of common currencies also tend to coincide with events negatively affecting international trade, such as decolonisation or civil wars. It is therefore far from clear that the estimates found in the literature are shedding lights on a policy-relevant relationship going from currency unions to bilateral trade.

To estimate a causal link between a common currency and bilateral trade flows we would ideally want to run an experiment, randomly assigning currency union membership to a pool of suitably similar countries, and then observe the ex-post difference between treatment and control groups. Faced with the impossibility of such an approach, one must turn to a quasi-experimental setting where real world events mimic the characteristics of an experiment.

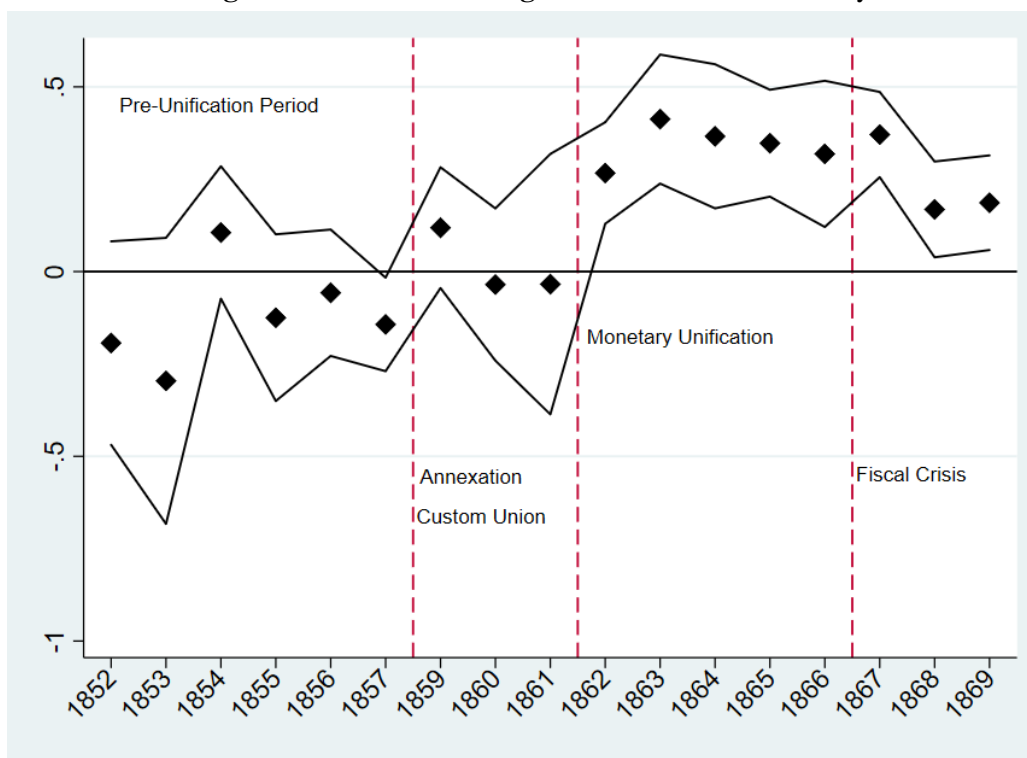
I focus on a historical setting where a sudden geopolitical shock, the Italian unification, led to random variations in currency union membership. In 1861, a small buffer state, the Kingdom of Piedmont-Sardinia, ended up unexpectedly annexing most of modern-day Italy, following a series of unintended diplomatic and military events. Piedmont was until then part of the French sphere of influence and as such a member of a French franc area that existed throughout the 19th century. The Italian unification therefore meant that, when Piedmont extended its institutions to the annexed territories, several Italian states exogenously found themselves in an accidental common currency relationship with France and its satellites.

The paper relies on original data, compiled by foreign embassies in Italy, that I collected from diplomatic archives and historical publications. These new data allow me to observe Italian bilateral trade before and after unification at the pre-unitary state level. I can therefore estimate the common currency effect on international trade relying on a gravity model, in line with the contemporary literature. My findings indicate that, based on the quasi-experimental environment I examine, we should expect an increase of bilateral trade in the region of 35% once a common currency is implemented.

The estimate is robust to different specifications of the gravity equation, sampling of the data and, more importantly, is not driven by any contemporaneous third factor. Looking at the staggered timing of the implementation of the Italian unification as well as to product-level tariffs data, I provide evidence that results are not driven by a change in tariff policy. I also show they cannot be explained by the adoption of a new unit of measurements, historical trends, or path dependency related to the Napoleonic occupation.

I find the effect to be heterogenous within each bilateral pair. In line with previous work, the estimated common currency effect is found to be larger for ex-ante lower trade shares. Additionally, while large in absolute terms, the magnitude of the effect I uncover is meaningfully smaller than the average one estimated in the literature. This might signal that an upward bias related to the endogeneity of currency unions might indeed be an issue in the existing literature. However, the paper largely confirms the early policy implications of the literature in a quasi-experimental setting, as we should expect a common currency to have a significant positive effect on bilateral trade.

Figure: The Leads and Lags of the Common Currency Effect



This figure shows the common currency effect on bilateral trade, estimated by year, across key sub-periods of the Italian unification process, with the switch to common currency occurring for all treated pairs by 1863.

Monnaie commune et commerce international : estimations à partir d'une union monétaire accidentelle

RÉSUMÉ

Cette étude repose sur une expérience naturelle historique afin d'estimer, pour la première fois, l'effet causal d'une monnaie commune sur le commerce international. Depuis les travaux de Rose (2000) une littérature abondante s'est intéressée aux monnaies en tant que coût de transaction dans le commerce international. Néanmoins, il apparaît probable que des biais d'endogénéité et d'auto-sélection soient intrinsèquement liés à l'adoption d'une monnaie commune, ce qui pourrait expliquer les effets significativement positifs trouvés par la littérature. Je propose une approche quasi-expérimentale en exploitant un épisode de variation exogène dans l'adoption d'une monnaie commune, induite par un choc géopolitique – l'unification Italienne en 1861 - et impliquant la zone monétaire du Franc français au 19^{ème} siècle. Grâce à une base de données nouvellement constituée et par le biais d'équations de gravité structurelles, je suis en mesure d'estimer un effet de l'ordre de 35%. Mes résultats mettent en avant un effet significatif, bien qu'hétérogène, des monnaies communes sur le commerce bilatéral.

Mots-clés : monnaie commune, unions monétaires, commerce international, expérience naturelle.

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1. Introduction

This paper follows a growing literature that relies on historical natural experiments to tackle important empirical questions (Cantoni and Yuchtman, 2021) where causal inference is impractical with contemporary data. I fill an important gap in the literature on the real effects of exchange rates by providing, for the first time, quasi-experimental evidence on the effect of a currency union on international trade. To do so, I exploit an exogenous variation in the membership of a currency area driven by an unexpected geopolitical shock, the Italian unification in 1861.

Rose (2000) first estimated the effect of currency unions on bilateral trade. Looking at post-WWII currency areas, he suggested their effect to be as high as a trebling of trade. A vast literature has since attempted to dispute, with some success, Rose's seminal findings. Despite those efforts, the "Rose Effect" of common currencies on trade has managed to survive. In their meta-analysis, sampling 37 estimates using structural gravity equations, Head and Mayer (2014) computed an average estimated effect of 136%.

One key reason why Rose's insights have been met with skepticism is the endogeneity bias implied by the decision to join a currency union. As noted by Head and Mayer (2014) the literature's findings should not be interpreted as *preferred* estimates of the causal effects of the policy variables. This is an important point, given that trade creation is commonly considered as the key benefit stemming from the establishment of currency unions. Whether a causal "Rose Effect" exists is therefore a topical policy question, as calls to leave the Euro Area have mounted in several European nations and new projects of monetary integration are considered around the world, particularly in Africa.

The issue of self-selection into common currencies was raised early on by Persson (2001). However, those concerns have so far only partially been addressed by the literature using matching techniques, deeming experimental evidence "*practically impossible*" to obtain (Persson, 2001). Indeed, in order to infer causality on the effect of currency areas, one would ideally need countries to randomly join a common currency. As it is unlikely that we will observe a random adoption of the Euro any time soon, I turn to

the history of European monetary integration to make causal inference tractable.

My identification strategy relies on a quasi-experimental setting, involving what is probably the closest historical precedent to the Euro Area. Since the Napoleonic period and throughout the 19th century, the French franc monetary standard was adopted by several European nations, resulting in a large common currency area. The latter was institutionalised in 1865 as the Latin Monetary Union. Around 1861, a major exogenous variation in the membership of the French franc zone took place (Figure 1). It was brought about by the sudden unification of the Italian peninsula by the Kingdom of Piedmont-Sardinia, following random diplomatic and military events. Piedmont-Sardinia was a long-standing French satellite state, home of an important Francophone minority and, importantly, an early adopter of the French franc in the first half of the 19th century. This meant that the Italian states annexed by Piedmont-Sardinia suddenly found themselves to be part of an accidental currency area with France and its French-speaking satellites.

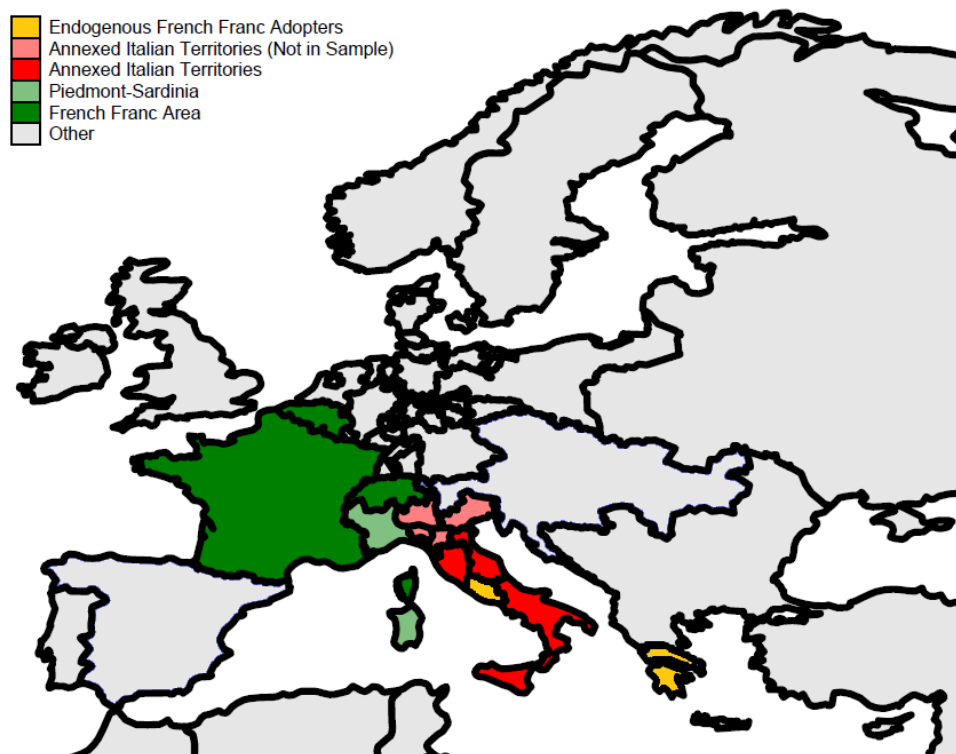
This geopolitical shock makes for an ideal set-up for drawing causal inference. The unexpected character of the Italian unification, and the fact that it involved a mechanical extension of Piedmont-Sardinia's institutions to the annexed states, mean that their policy preferences could not be endogenised. Furthermore, based on standard determinants of foreign-exchange rate regime choice, it would have been highly unlikely for the Italian provinces annexed by Piedmont to endogenously join a common currency with France in a no-unification counter-factual scenario.

I estimate the effect of this exogenous shock to trade costs by merging newly collected data on Italian trade at the regional level around unification with a large bilateral trade data matrix. Relying on structural gravity equations, the estimated common currency effect is large and significant, with a preferred estimate at 35%.

My results are markedly lower than the average point estimate in the literature and around 1/3 of recent estimates carried out with theory consistent gravity equations by [Glick and Rose \(2016\)](#) and [Larch et al. \(2018\)](#).

However, opposite to findings by [Campbell \(2013\)](#) and [Campbell and Chentsov \(2017\)](#), I argue that the common currency effect found in the literature is not completely driven by endogeneity. On the contrary, relying on a credibly causal quasi-experimental

Figure 1: An Exogenous Variation in French Franc Standard Adoption



setting, my findings confirm the original policy implications of the Rose literature.

The paper is organised as follows. Section 2 surveys the literature on the common currency effect on trade, with a focus on the endogeneity issue; Section 3 focuses on the historical context of the Italian unification as an exogenous shock; Section 4 covers the identification strategy; Section 5 details the empirical specification; Section 6 describes the data; Section 7 provides baseline results, robustness checks and looks closely at potential threats to the identification strategy; Section 8 discusses the results with respect to external validity and potential channels and Section 9 concludes.

2. Trade Effect of Monetary Unions and Endogeneity Bias

This paper follows a recent trend in the historical economics literature, relying on historical natural experiments to tackle important empirical questions where contemporary data prevent a rigorous causal inference treatment. Previous efforts in the same vein related to international economics include [Bernhofen and Brown \(2004\)](#) and [Bern-](#)

hofen and Brown (2005) clean identification of the causal effect of openness looking at the exogenous transition of Meiji Japan out of autarky. Juhász (2018) identifies a positive causal relationship between protectionism and industrialization exploiting the heterogeneity in the enforcement of the Napoleonic blockade in early 19th century France. Palma (2021) explores exogenous variations in international flows of precious metals in the early modern period to identify the causal effects of changes in money supply.

The seminal contribution of Rose (2000) - finding that a currency union (CU) triples trade - sparked an intense academic debate. The magnitude of the effect appearing implausible¹, a subsequent literature attempted to "shrink" the CU effect, based on sample selection and data issues². This prompted a response by Glick and Rose (2002) confirming a very substantial CU effect (doubling of trade). Baldwin (2006) provides an interesting methodological critique of this early literature as well as its own estimate of the Euro's trade effect at around 5-10%. In particular, early estimates were performed using gravity equations inconsistent with international trade theory, as they did not take into account unobserved bilateral heterogeneity as well as general equilibrium effects such as multilateral resistance (Anderson and Van Wincoop, 2003). Another issue with early results in the literature lied in the estimate of log-linear equations through OLS, which Silva and Tenreyro (2006) show to be biased in the presence of heteroskedasticity. In a subsequent paper, Silva and Tenreyro (2010) find no statistically significant effect of the Euro on trade through a gravity equation estimated through Poisson-Pseudo Maximum Likelihood (PPML).

Glick and Rose (2016) responded once again to the above criticism by performing the estimate on a larger dataset, confirming the overall message of their early findings: the overall CU effect is about 100% and of 50% in the case of the Euro. In a relatively recent meta-analysis of the literature, Rose (2017) notes that the vast differences in the estimates of the Euro effect lie in sample size. If a large enough sample is considered, the results are consistent with a substantial pro-trade effect of currency unions. A recent response by Larch et al. (2018) has however once again highlighted that the estimated effect for the Euro can be highly sensitive to the econometric specification,

¹Including to Rose himself.

²See Nitsch (2002), among others.

while confirming an effect of 100% for non-EMU currency unions.

A more substantial critique to Glick and Rose's results and their policy implications is represented by endogeneity bias and self-selection into (or out of) currency unions. In this respect, instrumental variable approaches were attempted early on by [Rose \(2000\)](#) and [Alesina et al. \(2002\)](#) but were later abandoned due to unconvincing identification assumptions ([Baldwin, 2006](#)). More convincingly, [Persson \(2001\)](#) first implemented matching techniques to address systematic differences between countries that adopt common currencies and those that do not. Other attempts to partially overcome endogeneity include diff-in-diff-inspired approaches focusing on the selection of relevant control groups ([Micco et al., 2003](#); [Silva and Tenreyro, 2010](#)), on controlling for "third factors", trends ([Berger and Nitsch, 2008](#)) and propensity to treatment and self-selection ([Wolf and Ritschl, 2011](#)).

A key motivation to the present paper lies in the findings by [Campbell \(2013\)](#) and [Campbell and Chentsov \(2017\)](#). They adopt an original critique to the "Rose Effect" literature, by focusing heavily on endogeneity bias. Short of having quasi-experimental evidence at their disposal, they take a close look at the context in which CU switches occur in Rose's dataset. They argue that the literature's results are systematically driven by endogeneity and coincident factors to CU switches, such as decolonisation, warfare, the fall of the Berlin wall or the wider European economic integration processes. They do not find any statistically significant common currency effect once "intuitive" control groups are implemented and CU switches coterminous with war are excluded from the sample.

In this paper, I therefore fill a key gap in the literature and address the endogeneity bias issue by relying on a unique historical quasi-experiment. This is in line with previous attempts to provide causal evidence on the effect of the exchange rate regime, exploiting historical exogenous shocks ([Lopez-Cordova and Meissner, 2003](#); [Mitchener and Voth, 2011](#)). My approach is very close to the one adopted by [Frankel \(2008\)](#), who analyzes the fortuitous adoption of a Euro hard peg by the African countries formerly pegged to France³. To my knowledge, the present paper is however the first attempt to use a natural experiment to specifically investigate the causal effect of common curren-

³[Tenreyro \(2010\)](#) however notes the paper's identification strategy does not necessarily comply with the parallel trend assumption.

cies on trade using the original definition used by [Rose \(2000\)](#) of a 1-on-1 irrevocable exchange rate.

3. The French Franc Zone and the Italian Unification

Following the Italian unification in 1861, several formerly independent Italian states became part of a wider European currency union which, in many respects, represents the closest historical precedent to the Euro. This French franc area was originally formed by France and its neighboring states, Belgium, Switzerland and Piedmont-Sardinia.

I argue that this historical episode provides an ideal environment to estimate the causal effect of currency unions on trade. Section [3.1](#), describes the role of France as an international monetary anchor in the mid-19th century. Section [3.2](#) highlights how the 1861 Italian unification in-itself was a highly unpredictable exogenous shock. On the one hand, it was not driven endogenously by cultural, political or economic ties among Italian states. On the other hand, it unexpectedly materialised following a number of random political and military events.

My identification strategy is built around the unintended consequences of this exogenous shock on bilateral trade costs and is discussed later on in Section [4](#).

3.1. France as an International Monetary Anchor in the 19th Century

The paper's quasi-experimental setting lies in the exogenous adoption of the French franc by several Italian pre-unitary states, following their annexation by the Kingdom of Piedmont-Sardinia, a long term adopter of the French franc standard.

Piedmont-Sardinia's currency had been intrinsically equivalent to the French franc since the beginning of the 19th century, when it had briefly become a French *département* under Napoleonic occupation. After the congress of Vienna and the demise of the First French Empire, both Belgium and Piedmont-Sardinia maintained the French monetary standard: the Sardinian lira, the Belgium franc and the French franc were equivalent to

4.5 grams of pure silver or a fixed equivalent in gold as part of a bimetallic standard. The currency of each country circulated widely in the three states, at times with legal tender status. Switzerland joined the "club" in 1850, when the Swiss franc was introduced as the monetary unit of the Helvetic Confederation. In fact, a *de facto* common currency among Francophone European states predates by a few decades the 1865 International Monetary Convention⁴, better known as the Latin Monetary Union⁵.

In many respects, the French franc currency area I examine in this paper represents the closest historical precedent to the Euro, given both its geographic spread and economic rationale.

The technocrats of the French Second Empire clearly understood monetary fragmentation as a cost to trade and international financial transactions. The key policy maker behind the international expansion of the Franc, Félix Esquirou de Parieu, was in many ways a European federalist *ante litteram*. Heading French monetary diplomacy, he wanted to achieve for international finance transaction costs what Michel Chevalier had achieved for tariff reduction with the 1860 Cobden-Chevalier treaty. He was determined to create a European, or even global, monetary standard around France.

As noted by [Flandreau \(2000\)](#), the key driver for the international spread of the French franc relates to French surging capital exports. Following the establishment of the Second French Empire and the economic reforms of Napoleon III, France became a major supplier of international capital, increasingly competing on equal footing with Great Britain. Even though Esquirou de Parieu's grand project of monetary diplomacy came to a halt following France's defeat in the war against Prussia in 1870, strong momentum built up in the 1860s in favor of harmonisation of the international monetary system around the French Franc. Several European and Latin American nations considered, and even made steps towards, the adoption of the French franc in the late 1860s, including Spain, Austria-Hungary and Romania, while the Papal States and Greece

⁴For a detailed discussion see among others [Einaudi \(2001\)](#) and [Flandreau \(1995\)](#).

⁵The Latin Union arose from the need to coordinate the intrinsic value of lower denomination coins following the change in the relative price of silver and gold, which prompted Switzerland to debase some of their lower denomination coins. The persistence of small variations in the intrinsic value of some denominations does not concern the policy treatment at hand here, as they did not meaningfully increase transaction costs. The fact that small denomination coins circulated across borders shows that they were accepted at face value. This therefore posed a fiscal problem for the French government which undergo Gresham Law-driven influxes of debased small denomination coins from its neighbours ([Einaudi, 2001](#))

formally achieved membership of the Latin Union in 1866 and 1868 respectively.

3.2. The Italian Unification as an Exogenous Shock

Even though the Italian unification provides the catalyst for its quasi-experimental setting, the paper is not concerned with the reduction in trade costs between Italian pre-unitary polities. Indeed, it would be impossible to disentangle the common currency effect between, say, Tuscany and Sicily with other features of political unification. The Italian unification is only indirectly of interest here, to the extent it had important unintended consequences: the exogenous adoption of French institutions by the annexed Italian states. It is however crucial to analyze how the Italian unification as a centralised state *in-itself* was an highly unlikely outcome, brought about by random diplomatic and military events. This provides a first layer of exogeneity and the foundation for the paper’s key identification assumptions detailed in Section 4.

3.2.1. Lack of Cultural, Political and Economic Ties among Pre-Unitary Italian States

In 1814, leading Austrian statesman the Prince of Metternich defined Italy as a mere “*geographical expression*”. This definition remained true in 1859, when the outburst of the Second Austro-Sardinian War paved the way, in the space of a few years, to the establishment of a full-fledged political, economic and monetary union as part of a centralised Italian state. This outcome was very much unexpected even a few months before it actually unfolded in 1861⁶.

Prior to unification, Italian states had very loose, if any, cultural, political and, above all, economic ties. Culturally, the Italian language was essentially a literary language limited to a small minority. Prominent Italian linguist [De Mauro \(2017\)](#) estimated that at unification Italian speakers amounted to less than 2% of the population⁷. Piedmontese Prime Minister Cavour himself, often depicted as the hero of the Italian unification by popular history, had at best a weak command of the Italian language, preferring to express himself in French or in the Piedmontese dialect. Despite extensive

⁶Chapter 1 of [Barbagallo \(2017\)](#) extensively reviews this “unforeseen union”.

⁷With an upper bound estimate of 10% if the speakers of central-Italian dialects and the clergy are included.

Figure 2: Italy Before and After the 1861 Unification



journeys across Europe, he had little interest for the Italian peninsula, and allegedly never traveled further South than Florence.

Politically, eight separate sovereign or semi-sovereign states existed (Figure 2), with no particular perception of a common belonging. A process of divergence in terms of political regimes started out in the 1840s, with Piedmont-Sardinia embracing constitutional monarchy and international integration while the rest of the peninsula remained under the absolutist regimes of direct or indirect Austrian rule, the Pope or, in the South, the Bourbon monarchy.

Economically, the lack of pre-unitary ties is also striking. In the 1850s, intra-Italian trade was 15-20% of the total international trade of the Italian states. However, trade across the borders of the Northern Po Valley between Piedmont and Lombardy-Venetia alone accounted for more than half of this figure (Federico and Tena-Junguito, 2014). Lack of regional trade is unsurprising when considering the absence of railways along the peninsula, the poor state of the roads and widespread protectionism. Monetary standards differed in each pre-unitary state⁸, alongside unit of measures and degrees of financial development. This contrasts with the experience of pre-unitary Germany, where gradual monetary integration pre-dates unification by a few decades.

⁸And even within states, as two different monetary standards were in force in the Kingdom of the Two Sicilies, one for the continental part and one for the island of Sicily.

3.2.2. Unintended Consequences of the Austro-Sardinian War of 1859

While there was no endogenous process behind the sudden unification of Italy, the historical literature also shows how random military and diplomatic events actually led to this outcome⁹. To be clear, I am not arguing that some sort of Italian integration process was unlikely to get under way by the mid-19th century. What, based on historical scholarship, and particularly the seminal work of [Mack Smith \(1958\)](#), was however highly unlikely, is the establishment of a fully centralised Italian state spanning the entire peninsula as soon as 1861.

A unification process like the one that would materialise in 1861 was neither expected nor desired by policy makers, both in Italy and abroad. When Cavour and Napoléon negotiated the secret 1858 military agreement against Austria in Plombières, they agreed to a territorial expansion of Piedmont in Lombardy and Venetia and the creation of a loose Italian confederation to be headed by the Pope. Of course, radical Italian nationalists had long unsuccessfully attempted to foster unrest in the peninsula. However, this nationalist sentiment was not widely shared by the Italian elites, particularly by the conservatives ruling Piedmont. The idea of a unified Italy was described, in private correspondence, as "*silly non-sense*" by the Prime Minister of Piedmont-Sardinia Cavour as late as 1856. The closest thing to unification the political mainstream had in mind before the war of 1859 was the creation of some sort of confederation of Italian states that would be better able to counter Austrian influence in the peninsula, which was reflected in the Plombières agreement. This remained the most likely outcome - following Austria's defeat in 1859 - as late as November 1859 when, at the Treaty of Zurich, France and Austria agreed on Piedmont's annexation of Lombardy but not Venetia and reiterated a commitment to an Italian confederation under the Presidency of the Pope.

A key factor explaining the turn of events which, between January 1860 and March 1861, led to the proclamation of the Kingdom of Italy is the threat of revolution in Central and Southern Italy. First, fears of geopolitical instability led France to accept Piedmont's annexation of Tuscany and Romagna by mid-1860. Second, the unexpected

⁹[Romeo \(1984\)](#), [Mack Smith \(1958\)](#) and [Hearder \(2014\)](#) provide detailed accounts of the events. I provide in [Appendix 11](#) an historical summary as well as a timeline.

success of Garibaldi's militias against the army of the Bourbon's monarchy in the Two Sicilies made the establishment of a revolutionary government in Naples a distinct possibility. This pushed both Cavour and the Great Powers to support the annexation of the South by Piedmont in October 1860.

The unexpected character of the unification had important implications for its implementation, which provides the basis for my identification strategy.

4. Identification Strategy

The paper's identification strategy is based on two key assumptions. First, the way the Italian unification was implemented indirectly caused an unexpected, "accidental" reduction in bilateral trade costs between Francophone Europe and the Italian territories annexed by Piedmont, as the latter exogenously adopted the French franc monetary standard. Second, in a no-Italian unification counterfactual, those same annexed Italian regions would have been unlikely to endogenously choose to join the French franc standard in the 1860s in a no-unification counterfactual.

4.1. Implementation of the Italian Unification

The "*Piedmontisation*" process through which the Italian unification was implemented is an important part of the identification strategy.

The possibility of a confederation among Italian states had long been part of the political discourse by the time the Italian unification occurred. High heterogeneity among pre-unitary Italian states would have however warranted a gradual approach and a federal setting, not unlike what Germany experienced in the first half of the 19th century. Nevertheless, the unexpected outcome of the Austro-Sardinian War of 1859 meant that unification took the form of a fully centralised state established in less than five years. This reflected Piedmont-Sardinia's haste in consolidating a relatively weak position, where a tiny - if resourceful - buffer state was suddenly annexing territories with a population more than five times its own, with the looming threat of external aggression from Austria.

The only way this centralised state could be created in such a short time frame, was

through a so-called process of "*Piedmontisation*". Instead of creating new institutions, by 1865 Piedmont-Sardinia's administrative structures and laws were fully transposed - without any amendment of substance - to the rest of the Italian peninsula. This included the adoption of a common Italian currency intrinsically equivalent to the French franc as the sole legal tender.

Crucially for the identification strategy, this blanket, automatic extension of Piedmontese institutions to the rest of Italy ensures that the preferences of the annexed states, particularly in terms of monetary standard, could not have been endogenised.

Table 1: Implementation of the Italian Unification

Date	Event
1859-1860	The annexed territories are ruled by revolutionary "provisional governments", sovereignty is progressively transferred through plebiscites; the Piedmontese tariff system is swiftly extended to the new territories with few temporary exceptions.
1 st of January 1861	The <i>laissez-faire</i> tariffs of Piedmont-Sardinia are applied uniformly in all the annexed territories.
1860-62	The National Bank opens branches in the annexed territories, multiple currencies circulate as legal tender. Trade finance instruments start to be quoted in French-Franc based Piedmontese Lira.
1 st of January 1863	The French-Franc based Piedmontese Lira, now renamed Italian Lira, becomes the only legal tender. The metric system becomes the official system of measurement for all annexed territories.
2 nd of April 1865	A unified Civil Code, based on the Piedmontese code, comes into force in all annexed territories.

Source: [Romeo \(1984\)](#).

Of course, "*Piedmontisation*" implied a number of policy shocks. In particular trade policy changed dramatically in some annexed states, as the tariffs of Piedmont-Sardinia were fully applied in all territories by end of 1860. However, I will show that this is not a concern (Section 5.2) for the purpose of identification. First, the timing of the different steps of "*Piedmontisation*" helps disentangle the trade policy shock from the monetary unification one. While intra-Italian trade barriers were abolished as soon as 1859-1860, as part of an Italian custom union aligned to the Piedmontese tariffs, a more

gradual approach was adopted as far as monetary unification was concerned (Table 1). Second, and more importantly, the trade policy shock is multilateral in nature, as tariffs are reduced equally across all main partners, making the adoption of the French franc standard the only factor to bilaterally affect transaction costs between treated pairs.

4.2. No-Unification Counterfactual

Despite the fact that their adoption of the French franc standard was "accidental" and their own preferences could not have been endogenised, the identification strategy could still be challenged if the Italian states annexed by Piedmont were more likely to endogenously self-select into a common currency with France. It is therefore important to consider what might have happened in a no-Italian unification counterfactual scenario.

It is unlikely that the annexed Italian states would have self-selected into the French franc zone in a no-unification counterfactual. In Table 2, I report a summary of key determinants of FX-regime choice, according to the OCA literature, for selected Italian regions. Sicily, the continental Italian South and, to a lesser degree, Tuscany, had both more volatile nominal shocks and traded substantially less with France compared to Piedmont-Sardinia and other polities in the French sphere of influence.

The predictive power of OCA criteria in the context of the mid-19th century wave of European monetary integration is the focus of separate work in [Vicquéry \(2021\)](#). I find that standard determinants of foreign-exchange regime choice are good predictors of long run patterns of monetary integration. The clear outlier is represented by the Italian unification, with Northern Italian polities exhibiting characteristics closer to the European monetary core.

Based on the historiography surrounding the Austro-Sardinian War of 1858 outlined above, a plausible no-unification counterfactual might have seen materialising a scenario closer to the initial Plombières agreement, kick-starting a loose political integration of the Italian peninsula. This would have possibly entailed economic integration as well, but at a much slower pace and not necessarily along the lines of a closer alignment to France. The example of Germany, where monetary integration followed centuries of gradual political and economic integration is useful in putting into perspective the

Table 2: Selected Italian Polities and FX-Regime Determinants

	Piedmont-Sardinia	Tuscany	Romagna	Two Sicilies
	<i>Bilateral Trade per Capita in 1854-1858 (GBP)</i>			
France	1750.63	804.11	168.28	562.48
Great Britain	441.26	753.78	184.29	625.93
<i>Ratio FR/UK</i>	3.97	1.07	0.91	0.90
	<i>Foreign-Exchange Bills Characteristics (1852-1858)^a</i>			
Volatility ^a	0.13%	0.18%	-	0.44%
Bid-Ask Spread ^b	23bp	24pb	-	56bp

^a Average standard deviation of weekly returns vs. GBP.

^b Average basis points spread between highest and lowest weekly London quote.

Source: [Vicqu ry \(2021\)](#).

unprecedented jump in integration that unfolded in Italy after 1858.

4.3. Nature and Dynamics of the Policy Treatment

The policy treatment I examine is fully consistent with the original definition of a currency union in the Rose literature. The Italian monetary unification effectively introduced an irrevocable 1-on-1 exchange rate on metallic currency between the annexed regions and other adopters of the French monetary standard.

Section 8 will provide more details on the possible channels through which the policy treatment could have reduced bilateral trade costs, as well as more perspective on the external validity of the estimate. It is for now important to stress that commodity-based monetary systems like the ones in use in the mid-19th century are better thought of as currency band regimes fluctuating within a floor and a ceiling, rather than pegs: volatility in the exchange rate exists to the extent that there are transaction costs to the shipping of metal across countries. Commonly considered theoretical channels in the common currency effect literature include a reduction in transaction costs, increase in price transparency and competition, as well as lower exchange rate volatility and trade finance costs ([Baldwin, 2006](#)). The adoption of the French monetary standard could have, in principle, resulted in lower trade costs *vis- -vis* other members of the standard through all those channels.

A recent study of the trade effect of the Latin Monetary Union ([Timini, 2018](#)),

focusing on the post-1860 period and relying on RICA data at the national level only, highlighted how the looseness of the arrangements in place to coordinate the currency area and solve conflicts among members meant that its effect might have been heterogeneous over time and geography. This might particularly apply to the present paper's analysis in the latter part of the treatment period I consider, from 1866 to 1869.

A potential source of heterogeneity in the policy effect is represented by the establishment of the Latin Union in 1865. The Union provided for further harmonisation of smaller denomination "fractional" currency, which were until then characterised by equal face values but might have differed in actual metal content across the zone. An increase in the treatment effect coming from this further step in the harmonisation of the monetary standard should however not be necessarily expected. The Latin Union arose precisely from the already wide circulation of debased non-harmonised lower denomination coins from the smaller members, which posed a fiscal problem for France.

A second potential source of heterogeneity lies in the impact of the 1866 financial crisis on Italy. Following another war with Austria and a fiscal crisis, the Italian government introduced some capital controls and a parallel paper currency, inconvertible into and trading at a discount with respect to the metallic Lira and its Latin Union equivalents. This implied substantial monetary instability but not necessarily a weakening of the treatment effect. The disruption to trade finance during the crisis meant that international transactions were likely - at least in the period this paper is concerned with - to be invoiced and settled in metallic currency, possibly increasing the relative transaction cost of trading with non-Latin countries with respect to Latin ones.

Finally, recent research by [Chen and Novy \(2019\)](#) has highlighted the underlying heterogeneity to the estimates of the common currency effect in the literature. While data availability prevents me from adopting their theory-consistent framework to investigate variable trade cost elasticity, I discuss heterogeneity in my results with respect to their findings in [Section 7.3](#).

5. Empirical Strategy

5.1. Gravity Equation Specification

I estimate a theory consistent structural gravity equation, accounting for multilateral resistance terms ([Anderson and Van Wincoop, 2003](#)) and time-invariant unobservables

$$\ln X_{ijt} = \lambda_{it} + \psi_{jt} + \mu_{ij} + \beta \mathbf{z}_{ijt} + \gamma \text{Franc}_{ijt} + \epsilon_{ijt} \quad (1)$$

where X_{ijt} denotes flows to importer i from exporter j at time t ; λ_{it} and ψ_{jt} denote time-varying importer-time and exporter-time fixed effect respectively, controlling for time-varying exporter (importer) specific factors, including relative price changes; μ_{ij} is a pair fixed-effects absorbing any time-invariant pair-specific factor; \mathbf{z}_{ijt} and \mathbf{w}_{ij} denote respectively time-varying and time-unvarying pair specific factors; Franc_{ijt} is a dummy variable taking value of 1 once both i and j share the franc as a common currency and η_{ijt} is a residual error.

The same equation can be written in multiplicative form and estimated using PPML, which is shown to be preferable to estimate log-linear elasticities ([Silva and Tenreyro, 2006](#))

$$X_{ijt} = \exp(\lambda_{it} + \psi_{jt} + \mu_{ij} + \beta \mathbf{z}_{ijt} + \gamma \text{Franc}_{ijt}) + \epsilon_{ijt} \quad (2)$$

Consistently with the literature [Larch et al. \(2018\)](#) I rely on Equation 2 as my preferred specification and report multi-way clustering standard errors at the exporter, importer, and year levels. In some specifications, I allow for bilateral pairs μ_{ij} to trend, following [Campbell \(2013\)](#) and [Campbell and Chentsov \(2017\)](#).

5.2. Coincident and Confounding Factors

Third factors correlated with switches into or out of common currencies have been shown to represent a potentially important bias in the estimation of the "Rose Effect" ([Campbell, 2013](#)). As outlined above, the Italian unification shock implied a number of policy changes that could potentially bias my estimate of the common currency

effect. Despite the low ex-ante probability of treated regions to self-select into the French Franc, other confounding factors could also have been at play. I provide a preliminary discussion of those potential threats below and address each of those threats in Section 7.2.

Trade Policy

The period at hand witnessed to a general trend of liberalisation of trade, which has been traditionally understood to take effect following the signature of the Cobden-Chevalier treaty by Britain and France in 1860, a few years before the start of my treatment period. There is disagreement in the literature over whether Cobden-Chevalier actually had pro-trade effects (Becuwe et al., 2018), particularly for targeted products (Lampe, 2009) or occurred once trade liberalisation was already well under way (Tena-Junguito et al., 2012). What is however a clear feature of trade liberalisation in the 1860s, confirmed by gravity analysis looking at aggregated trade (Accominotti and Flandreau, 2008), is its multilateral character, particularly as Most Favoured Nation (MFN) clauses became widespread. This means that as long as one controls for the presence of a FTAs, changes in trade liberalisation in aggregate should be absorbed by importer-year fixed effects.

This is particularly true for the trade policy shock implied by the Italian unification. While the adoption of a French franc standard led in principle to a bilateral reduction in trade cost, the extension of Piedmont's tariff implied a multilateral, symmetric reduction of trade costs across all partners at the same time, both at the time of unification and successively when existing treaties are deepened in the wave of Cobden-Chevalier. By the early 1850s, Piedmont-Sardinia had signed FTAs, mostly containing conditional MFN clauses, with all the major European economies. These treaties are extended either shortly after *de facto* military occupation or at the latest by end of 1860 to the annexed Italian territories. As the conditionality of MFN clauses would leave open the possibility of discriminatory trade policy, I provide evidence that the assumption of multilateral tariff changes is verified in the data for the post-unification tariff shock, by computing effective post-unification tariff changes for the annexed Italian states vis-à-vis France and Great Britain. Post-unification, the deepening of the existing pre-unitary treaties implies a contemporaneous adoption of unconditional MFN clauses vis-à-vis all

the main European partners which strengthen even further the multilateral character of trade policy.

I also run several robustness checks, exploiting the lag between the extension of the tariff system and monetary unification and testing for an heterogeneous effect of the trade treaties coming into force in the Italian states following annexation.

French Capital Exports

A major development throughout the period is the rise of France as a capital exporter. In this respect, one might be worried that the estimated common currency trade effect could simply reflect a recycling of capital exports from France, regardless of the adoption of a common currency. I make sure this is not the driver of my results by running a French franc "placebo" on peripheral countries that made formal steps towards but did not eventually adopt a French franc standard in the 1860s.

Adoption of the Metric System

An unintended consequence of the Italian unification which might bias the common currency effect estimate is represented by the adoption of the metric system's units of measure by the annexed Italian territories. I address this potential bias by testing whether I can detect any effect between Italian states and non-French franc adopters of the metric system.

Napoleonic Occupation

Finally, French occupation during the Napoleonic period could represent a potential confounder, as most of the Italian French franc adopters were at some point directly or indirectly part of the French Empire before 1812, with common "French institutions" possibly being correlated with trade. It is important to note that path-dependency and self-selection into French institutions driven by the Napoleonic period, which can explain the behavior of the original, partly French speaking, members of the French franc zone, had very likely subsided by the 1850s. Among the countries that endogenously made formal steps towards adoption of the French monetary standard in the 1860s, only the Papal States experienced a significant period of Napoleonic occupation. At the same time, within-Italy experiences under Napoleonic occupation varied substantially in terms of length and institutional legacies. Even though, time-invariant confounders are absorbed by high-dimensional fixed effects in my preferred specifica-

tion I run two distinct robustness check to ensure my results are not confounded. First, I explicitly control for the duration of Napoleonic occupation in some specifications, excluding pair-specific fixed-effects. As a further robustness check, I show that balancing the duration of Napoleonic occupation across treated and control pairs using a propensity score also confirm my baseline results.

6. Data

6.1. Trade Data

6.1.1. Reconstructing Coherent Pre and Post-Unification Italian Trade Data

A key contribution of this paper is to provide a first reconstruction of coherent trade data for each major pre-unitary Italian states by destination before and after unification, between 1852 and 1869. While official statistics on trade by pre-unitary state exist and were recently surveyed by [Federico and Tena-Junguito \(2014\)](#), they rarely provide flows by destination. More importantly, no official statistics on international trade at the pre-unitary state level were collected post-unification by the Kingdom of Italy.

I get around this problem by relying on statistics compiled by foreign consulates in Italy, drawn from archival and printed sources (See Appendix 12). French consulates around the world collected an impressive amount of data since the beginning of the 19th century. In 1841, a ministerial circular by Foreign Affairs minister Guizot required French consuls abroad to compile standardised tables on the *Etats de Commerce et de Navigation* of their catchment area, to be sent to Paris annually. The standardised tables included a matrix of trade value by destination/provenance and - even if inconsistently - product. Crucially, recording of detailed trade statistics at the pre-unitary Italian state level continued well into the unification period. British consulates performed a similar statistical exercise, albeit with little consistency across different diplomatic representations.

I am therefore able to construct a bilateral trade matrix for four major pre-unification

regions, Tuscany, Naples, Sicily¹⁰, Romagna¹¹ as well as for continental Piedmont-Sardinia for most years between 1852 and 1869.

Appendix 12 discusses in more details the sources of the data as well as their shortcomings and how I address them. It is however important to stress three features of the data.

First, I stop observing trade between Italian pre-unitary states post-unification. As discussed above, this is of largely irrelevant to identification as it would have been in any case impossible to disentangle an intra-Italian common currency effect from the different aspects of political unification occurring at the same time.

Second, I do not observe overland trade for pre-unitary Italian states. This is in any case of little relevance to the trade of Central-Southern Italy in this period, as the vast majority of its trade occurs by sea. Also this implies, if anything, a downward bias on the treatment effect. Consider the case of a relative increase of overland trade vs. trade by sea: this would cause a spurious decrease in trade in my data that is very likely to disproportionately affect French franc bordering countries such as France and Switzerland. I however test in some specifications whether excluding other likely affected countries - Austria and the Zollverein - changes my results.

Third, I address missing reporting in some years and measurement error by estimating gravity equations both on a yearly dataset and on a "collapsed" dataset, where I average bilateral trade over eight periods of three or two years, so that all pre-unitary states are reporting within each period.

6.1.2. Other Trade Data

The estimation is carried out on a relatively large bilateral trade matrix for nineteen polities. The matrix is obtained by merging my newly collected data at the Italian regional level with the bilateral trade data of the RICardo project database ([Dedinger and Girard, 2017](#)) for fourteen countries: Great Britain, France, Belgium, the Netherlands, the United States, Sweden, Russia, Spain, the German Zollverein¹², Portugal,

¹⁰While part of the same polity, the continental South and Sicily had two different custom administrations.

¹¹The Eastern part of the Papal States annexed by Piedmont-Sardinia in 1860.

¹²When not reported at the Zollverein level, I sum the trade of all the reported members of the custom union.

Austria, Turkey, the Papal States¹³ and Greece¹⁴.

6.2. Common Currency Treatment Dummy

- **Franc:** The treatment dummy variable is coded to be equal to 1 when the importer (exporter) is an Italian state adopting Piedmont-Sardinia's currency following annexation and the exporter (importer) is one of France, Belgium or Switzerland. The adoption of the Franc for international transactions - as signaled by the change in currency denomination of quoted trade finance instruments to Piedmontese or Italian Liras - occurs in 1861 for Tuscany, 1862 for Romagna and 1863 for the continental South and Sicily. In some specifications, I also estimate the franc effect including trade flows between annexed Italian states and the endogenous new-comers to the franc area in the 1860s, Greece (1868) and the Papal States (1866).
- **Franc (Endogenous):** In some specifications I also estimate a common currency effect for pairs that endogenously adopted the French franc with a dummy that is coded to be equal to 1 for pairs formed by France, Belgium, Switzerland, Piedmont, Greece and the Papal States. Only the addition of the latter two makes the estimate possible as the other pairs are time-invariant.

In some specifications, I code both Franc dummies so as to include trade flows with incoming adopters of the French Franc, the Papal States after 1866 and Greece after 1868. Figure 1 provides a visual summary of the nature of French franc adoption per polity/region.

6.3. Time-Varying Pair-Specific Controls

Given the high definition fixed-effects specification of my preferred gravity equation, only time-varying pair-specific trade costs can be estimated.

¹³To which I subtract the trade flows of the Eastern region of Romagna in the pre-unification period.

¹⁴All of these polities are reporters in the dataset with the exception of Austria, Turkey, the Papal States and Switzerland, which means that the bilateral trade among those pairs is not observed in the dataset.

- **FTA** is a dummy that takes value of 1 once a country pair signs a free trade agreement, allowing for an explicit reduction in tariffs with or without most favored nations clauses, according to the extensive Trade Agreements Database compiled by [Pahre \(2012\)](#).
- **War (Allied)** are dummy variables that takes value of 1 in the years where a country pair is at war with each other (allied with each other and actively engaged in a war), according to the Correlates of War dataset by [Sarkees and Wayman \(2010\)](#).
- **Metric System** is a dummy that takes value of 1 once if both trade partners have adopted the French Metric System of measurements.

6.4. Pair Time-Unvarying and Multilateral Time-varying Variables

Pair time-unvarying as well as multilateral time-varying variables, including bilateral distance, common language and border dummies and population size are not included in the gravity equation as they are fully absorbed by fixed effects. However, I report them in order to assess observable differences between French franc adopters and non-adopters and look at potential time-invariant confounders. In this respect, Napoleon is a variable equal to the product of the respective years of Napoleonic occupation experienced by the country pair, based on [Boudon \(2003\)](#). Polity2 is the sum of the Polity2 score of political autocracy vs. liberty, according to the [Polity \(2012\)](#) project, for a country pair. A higher score means a higher combined level of political liberty¹⁵. Using the sum of the years of occupation or dichotomous variables equal to 1 if both countries have been occupied does not meaningfully alter the results. Population data are from [Mitchell \(1998\)](#).

¹⁵Technically, this variable is time-variant at the pair level. However, low time variation means it is most of the time collinear with the gravity equation's fixed effects and thus discarded from regressions.

7. Results

7.1. Baseline Results

Table 3 shows baseline results, estimating Equations 1 and 2 using the whole dataset or a "collapsed" sample. The estimated common currency effect is large and precisely estimated, with OLS producing higher and less precise estimates compared to PPML. My preferred specification, PPML with bidirectional pair, exporter-year and importer-year fixed-effects and multi-way clustering in column (3) and (4), point to a common currency effect between 35% and 45%. Reset tests (Silva and Tenreyro, 2006) confirm PPML should be preferred, as misspecification is rejected. Results are also robust to the bias correction procedure for three-way PPML gravity equations developed by Weidner and Zylkin (2021).

Table 3: Baseline Results

	(1)	(2)	(3)	(4)
	OLS	OLS	PPML	PPML
Franc	0.414*	0.379**	0.355***	0.302***
	(0.204)	(0.147)	(0.0756)	(0.0772)
FTA	0.116	0.0177	0.0288	-0.0691*
	(0.0749)	(0.108)	(0.0305)	(0.0192)
Observations	3,801	1,882	4,350	2,074
R-squared	0.932	0.985	0.938	0.988
Sample	All Years	Collapsed	All Years	Collapsed
Implied Effect	0.513	0.427	0.460	0.353
RESET Test (P-Value)	-	-	0.0245	0.108
Bias Correction	-	-	0.348**	0.283*

Dependent variable: log of bilateral trade (OLS), bilateral trade (PPML). Bidirectional pair, importer-time and exporter-time fixed effects, war and allied dummies included in all specifications but not shown. Multi-way clustered standard errors reported in parenthesis. ***, ** and * denote statistical significance at the 0.01, 0.05 and 0.1 levels respectively. Bias correction reports point estimates and level of statistical significance according to the PPML bias correction procedure designed by Weidner and Zylkin (2021) with pair-level clustering standard errors

Table 12 tests the robustness of my preferred estimate to variations in the speci-

cation of the gravity equation¹⁶. As shown by [Campbell \(2013\)](#) and the literature he surveys, positively estimated effects on trade policy variables can hide secular trends at the pair level. Column 1 in [Table 12](#) allows for pair-specific trends in the gravity equation and yields again an estimate close to 35%. The same is true in Columns 2 and 3, introducing unidirectional pair-fixed effects, with and without a pair-specific trend, and in Columns 5 and 6 looking at the sensitivity to the treatment of missing data.

In [Table 13](#) I test the sensitivity of my baseline results to changes in the "control" trade flows. As shown by [Rose \(2017\)](#) this can widely affect the estimates of the common currency effect. Excluding large non-European countries (USA, Russia, Turkey) from the sample as they might be less comparable to treated countries. This tends to lower somewhat the estimated effect to 25%. I also try excluding from the sample non-French franc countries that are more likely to trade with Italy overland (Austria-Hungary and the Zollverein), as given the nature of my trade data this might be a source of bias in the effect¹⁷. There is no sign of such bias as estimates are substantially higher, between 45% and 55%.

I also verify that my results are not driven by any specific country. As in [Micco et al. \(2003\)](#), I drop in turn every country in the sample. Coefficients estimated while dropping one country at a time are plotted in [Table 14](#). The estimated coefficient remains precisely estimated and mostly close to 0.3. This exercise however highlights patterns of heterogeneity that will be examined more closely below.

[Table 4](#) introduces a Franc dummy for pairs that endogenously selected into the French franc area. This is only possible when coding the dummy including the 1860s newcomers to the franc area, the Papal States and Greece, making the variable time-variant and non collinear with pair fixed-effects¹⁸. The estimated coefficient for the endogenous franc effect is remarkably similar to the exogenous one, when using my preferred specification in columns 1 and 3. However, the effect turns negative and statistically insignificant once I introduce pair-specific trends in columns 2 and 4. This suggests progressive integration with the French bloc as a possibly important source of

¹⁶The same estimates performed on the complete dataset as opposed to the "collapsed" one yield similar results.

¹⁷Even though, as noted in [Section 6](#), if there is a bias, its sign is very likely to be negative overall, as most of Italian overland trade is likely to have French franc countries as destination.

¹⁸This is because legacy members of the franc area, Belgium, Piedmont-Sardinia and Switzerland, entered into a common currency with France years before the start of my sample period.

Table 4: Adding New Joiners and Endogenous Franc Pairs

	(1) PPML	(2) PPML	(3) PPML	(4) PPML
Franc (Exog. incl. New Members)	0.305*** (0.0784)	0.309*** (0.119)		
Franc (Exog. excl. New Members)			0.304*** (0.0752)	0.314** (0.137)
Franc (Italy-New Members)			0.338* (0.192)	0.206 (0.192)
Franc (Endogenous)	0.301*** (0.116)	-0.0791 (0.171)	0.309*** (0.119)	-0.0964 (0.151)
FTA	-0.0685*** (0.0200)	-0.0280 (0.0458)	-0.0685*** (0.0206)	-0.0279 (0.0693)
Observations	2,074	2,074	2,074	2,074
R-squared	0.988	0.993	0.988	0.993
Sample	Collapsed	Collapsed	Collapsed	Collapsed
Pair-specific Trend	NO	YES	NO	YES
Implied Effects:				
Exogenous incl. New Members	0.356	0.362	-	-
Exogenous excl. New Members	-	-	0.355	0.369
Annexed Italy and New Members	-	-	0.402	0.228
Endogenous	0.351	-0.0761	0.362	-0.0919

Dependent variable: bilateral trade. Bidirectional pair, importer-time and exporter-time fixed effects, war and allied dummies included in all specifications but not reported. Columns 2 and 4 allow for a pair-specific trend. Multi-way clustered standard errors reported in parenthesis. ***, ** and * denote statistical significance at the 0.01, 0.05 and 0.1 levels respectively.

endogeneity bias, comforting the identification strategy.

Adding new-comers to the exogenous Franc dummy yields similar results to the baseline. Separating out the effect between annexed Italian states and new-comers in columns 3 and 4 however signals an interesting difference with respect to the endogenous Franc dummy. While the introduction of pair-specific trends in column 4 makes the estimate statistically insignificant the coefficient remains large. This might be related to the reduction in trade costs between the annexed Italian states and the new-comers being more exogenous compared to the larger members of the French franc area.

7.2. Parallel Trends

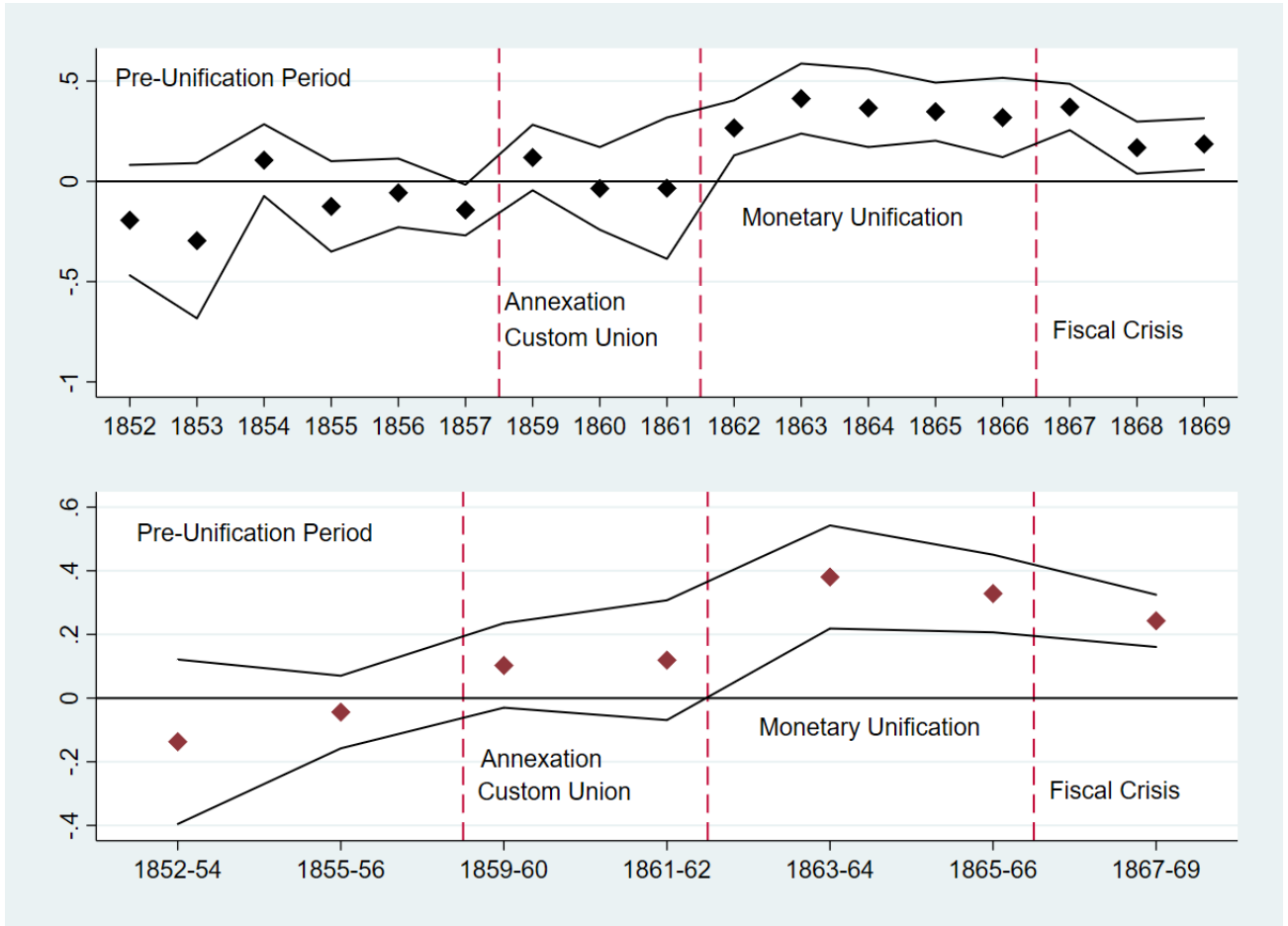
It is crucial to my identification strategy that the parallel trends assumptions between treated and control trade flows is fulfilled. Figure 3 provides reassuring evidence that this is the case by plotting leads and lags of the estimated coefficient. No common currency effect can be detected prior to the start of actual circulation of the French franc monetary standard in the annexed Italian territories. It might also be reassuring for the identification strategy that no change in the estimated effect can be detected between 1859 and 1861 when the new currency is still to be circulated but new trade tariffs are already in full operation in the annexed territories. I will turn more closely to the issue of the coincident shock to trade policy below.

7.3. Heterogeneity

Figure 3 also allows to explore heterogeneity in the estimated effect along the time dimension. As highlighted above, two sources of potential heterogeneity might be at play after 1866, with further harmonisation of coinage with the Latin Union convention on the one hand, and the Italian fiscal crisis of 1866 introducing a parallel in-convertible currency on the other hand. It is crucial to note that only the bottom panel in Figure 3 should be analyzed in this view¹⁹. The slight decline observed in the coefficient for 1867-1869 might signal an increase in relative trade costs related to the payment-system implications of the Italian fiscal crisis. However, this is not clearly backed by

¹⁹The top panel does not accurately portray heterogeneity over time as it does not correct for missing reporting by treated Italian states, which are more frequent after 1865 (See Table 17).

Figure 3: Leads and Lags of the Common Currency Effect



This figure shows the leads and lags of the common currency effect estimated with PPML, bidirectional pair, exporter-year and importer-year fixed-effects, using the whole sample (Top Panel, equivalent to Column 3 in Table 7.1) and the collapsed sample (Bottom panel, equivalent to Column 4 in Table 7.1). The first period highlighted by the red dotted line denotes the pre-treatment period before the Second Austro-Sardinian War in 1858 and the military events that will lead to the Italian unification. The second period corresponds to the annexation of Italian territories by Piedmont-Sardinia and the implementation of a custom union, leading to the 1861 proclamation of the Kingdom of Italy. The third period corresponds to the full implementation of the Italian monetary unification. The fourth period denotes the Third Austro-Sardinian War and the Italian fiscal crisis of 1866.

contemporaneous evidence in the consular reports I rely on to build my dataset. While the disruption is clearly mentioned as an increase in trade costs, it seem to be qualitatively portrayed as decreasing the trade costs between Italy and the French franc area relative to non-French franc countries. I find several references to shipment of physical currency to France as part of Consulates' trade reports in Italy after 1866²⁰.

Heterogeneity in the common currency effect along the cross-sectional dimension plays an important role in the most recent reassessment²¹ of the Latin Monetary Union trade effect by [Timini \(2018\)](#). He finds the Latin Monetary Union effect to be driven by hub-to-spoke flows between France and the other members, with no role for spoke-to-spoke flows. Table 5 shows this is not the case when looking at the exogenous French franc pairs, as, if anything, estimated effects are significantly stronger for trade flows between Belgium and Switzerland on one side and the annexed Italian states on the other side.

Asymmetric effects are also shown to be a crucial feature of the common currency effect in recent work by [Chen and Novy \(2019\)](#). Relying on a theoretical framework allowing for variable trade cost elasticity, they find the common currency effect to be strongly heterogeneous across and within country pairs, with higher effects found for smaller import shares. Both dis-aggregated estimates by the direction of flows in Table 5 and within pairs effects in Table 15 seem to be broadly consistent with their theoretical predictions, particularly when looking at France. The effect tends to be significantly higher for small import shares, or, in other words, trade flowing to France from the smaller treated countries, compared to large import shares. [Chen and Novy \(2019\)](#) show similar bilateral asymmetries can be found looking at Euro area members.

7.4. Trade Policy Shock

In principle, the adoption by the annexed Italian states of Piedmont-Sardinia's *laissez-faire* trade policy could represent a concern. However, as highlighted above, given the multilateral nature of trade liberalisation in the period I consider, changes in

²⁰See various comments related to bullion and currency shipping in [Ministère du Commerce \(1878\)](#), "Royaume d'Italie".

²¹Which focuses on what I characterise in this paper as the "endogenous" members of the French franc area and relying on national level data.

Table 5: Heterogeneity by Trade Flow Direction

	(1)	(2)	(3)	(4)
	PPML	PPML	PPML	PPML
Franc (France Imports)	0.371*** (0.123)	0.658*** (0.241)	0.303*** (0.114)	0.523*** (0.167)
Franc (France Exports)	0.269** (0.132)	0.0418 (0.139)	0.215* (0.125)	0.00520 (0.132)
Franc (Belgium Imports)	0.575*** (0.128)	0.882** (0.358)	0.460*** (0.157)	0.448 (0.315)
Franc (Belgium Exports)	0.800*** (0.170)	0.869*** (0.186)	0.792*** (0.140)	0.866*** (0.175)
Franc (Switzerland Imports)	-0.0321 (0.334)	1.279*** (0.443)	-0.0665 (0.232)	1.362*** (0.432)
Franc (Switzerland Exports)	1.316** (0.519)	2.394*** (0.447)	0.671 (0.443)	3.269*** (0.508)
Observations	4,350	4,350	2,074	2,074
R-squared	0.985	0.990	0.988	0.993
Sample	All Years	All Years	Collapsed	Collapsed
Pair-specific trend	NO	YES	NO	YES

Dependent variable: bilateral trade. Bidirectional pair, importer-time and exporter-time fixed effects, FTA war and allied dummies included in all specifications but not reported. Multi-way clustered standard errors reported in parenthesis. ***, ** and * denote statistical significance at the 0.01, 0.05 and 0.1 levels respectively. The estimates separate out the estimated franc effect depending on the direction of trade flows of legacy members. As in the rest of the paper, only exogenous pairs involving a legacy member and an annexed Italian state are considered.

trade policy should be systematically absorbed by exporter-year and importer-year fixed effects, particularly after 1860 with the proliferation of MFNs clauses. This assumption is consistent with estimated effects of the FTA dummy in all the specifications shown so far, as no pro-trade effect of bilateral trade treaty can be found. Furthermore, looking at the leads and lags of the franc effect reassuringly suggests that no effect can be detected in the few years where the new tariff regime was already in place but the French monetary standard was being gradually implemented. If the common currency effect I uncover was driven by a contemporaneous tariff shock dis-proportionally favoring French Franc countries one should expect this to appear as soon as 1858-1861, when the custom union is implemented.

I further test the robustness of my assumption regarding the multilateral nature of the trade policy shock. First, by testing test whether the change in bilateral trade costs following the adoption of Piedmont-Sardinia’s tariffs in 1858-61 was symmetric for France and Great Britain *vis-à-vis* all annexed Italian states individually. I compute a measure of post-unification effective tariff change T between each annexed Italian region i a partner j for n products k as

$$T_{ij} = \sum_{k=1}^n W_{ijk} S_{ik} \quad (3)$$

where W_{ijk} is the share of product k in the the total export of partner j to Italian region i and S_{ik} is the post-unification percentage change in tariff for product k in Italian region i . I am able to calculate T for all Italian regions in my sample *vis-à-vis* France and Britain across 19 products, relying on the archival data sources discussed in Appendix 12 and the data on pre and post-unification tariffs by product and region compiled by [Stringher \(1889\)](#).

As shown in Table 6, the difference in the post-unification effective change in tariffs vs. France and Great Britain is statistically insignificant for all Italian annexed regions. This confirms the assumption of a symmetric change in trade costs coming from the post-unification tariff shock, which should be therefore absorbed by fixed effects. If anything, the computed weighted tariff changes would indicate a downward bias in my estimates. Indeed, the tariff shock seems to have marginally favored Britain, compared

Table 6: Product-Mix Weighted Post-Unification Change in Tariffs

	Tuscany	Romagna	Naples	Sicily
Weighted Tariff Change				
<i>France</i>	3%	-14%	-46%	-21%
<i>Great Britain</i>	-4%	-19%	-53%	-43%
Equal Mean T-Test (P-Value)	0.684	0.843	0.837	0.374
Imports Coverage	79%	70%	57%	51%

Source: [Stringher \(1889\)](#), [Ministère du Commerce \(1878\)](#).

The first two rows compute the change in tariffs brought by the adoption of Piedmont’s tariff across 19 products, weighted by the share of each product in the total bilateral imports between each region and France/Great Britain. The third row reports the p-value of a equal mean T-test between weighted tariff reduction for France and Great Britain by region. Imports coverage denotes the share of imports covered by the 19 products considered.

to France. As shown in Figure 7, this was driven by larger tariff reductions in products disproportionately exported to Italy by Britain relative to France, such as iron, wool and, to a lesser extent, cotton.

Second, Columns 5 and 6 of Table 7 further explore any differential effects coming from the FTA treaties exogenously extended to the annexed territories by Piedmont, compared to the average FTA effect. I estimate separately from the Treaty variable a Piedmont-treaties dummy turning to 1 when an annexed territory adopts a trade treaty as part of the Italian unification. No statistically significant effect can be detected, while the franc effect remains stable.

7.5. Other Coincident Factors

Other important coincident factors include the rise of France as a capital exporters and the adoption of the metric system by the annexed Italian states.

Regarding the former, one could speculate that my treatment effect is in fact related to France’s rising role as a capital exporter, regardless of the adoption of the French franc monetary standard. I address this by running a ”placebo French Franc” estimate, where I test whether I can detect an effect for trade flows between the original members of the French franc bloc and Spain and Austria after 1862. Those countries were peripheral capital importers and made formal steps towards the adoption of a French monetary

Table 7: Coincident Factors

	(1)	(2)	(3)	(4)	(5)	(6)
	PPML	PPML	PPML	PPML	PPML	PPML
Placebo Franc	-0.0573 (0.124)	-0.0929 (0.138)				
Metric System			-0.0252 (0)	0.157 (0.166)		
Franc					0.350*** (0.0769)	0.292*** (0.0774)
FTA Annexed					0.0823 (0.101)	0.0271 (0.100)
FTA Excl. Annexed					0.0282 (0.0358)	-0.0701*** (0.0239)
FTA	0.0253 (0.0299)	-0.0744*** (0.0188)	0.0243 (0)	-0.0763*** (0.0261)		
Observations	4,350	2,074	4,350	2,074	4,350	2,074
R-squared	0.985	0.988	0.985	0.988	0.985	0.988
Sample	All Years	Collapsed	All Years	Collapsed	All Years	Collapsed
Implied Effect	-	-	-	-	0.419	0.339

Dependent variable: bilateral trade. Bidirectional pair, importer-time and exporter-time fixed effects, war and allied dummies included in all specifications but not shown. Multi-way clustered standard errors reported in parenthesis. ***, ** and * denote statistical significance at the 0.01, 0.05 and 0.1 levels respectively. Columns 1 and 2 perform a placebo franc test using Austria and Spain as placebo-treated. Columns 3 and 4 test whether the franc effect could be actually driven by the adoption of the metric system. Columns 5 and 6 separate out the FTA dummy for FTAs extended to Italian annexed territories.

standard and Latin Union membership, without ultimately joining. As can be seen in columns 1 and 2 in Table 7 no "placebo Franc" effect can be detected.

With respect to the metric system, I exploit the fact that several countries were metric system adopters but did not have a French franc monetary standard. I therefore test whether I can detect any trade effect during the treatment period between the annexed Italian states and the Netherlands, Portugal and Spain. Columns 2 and 3 in Table 7 show that such effect is either negative or not statistically significant.

7.6. Confounding Factors and Balancing

Finally, I address the possibility that confounding factors are biasing my results. Propensity score based methods are helpful in this respect, as they allow to build a control group with similar probability of treatment to the treatment group in observational studies. They were previously employed in the context of the common currency effect literature by [Chintrakarn \(2008\)](#) and [Wolf and Ritschl \(2011\)](#). While the treatment is arguably exogenous, and is shown to be with respect to classical determinants of exchange rate choice the control group exhibits significantly different characteristics across several dimensions, including distance, degree of political autocracy, population size and, importantly, years of Napoleonic occupation, a possible confounder. Column 1 in Table 8 controls directly for such time-invariant factors by estimating the gravity equation without pair fixed-effects. Furthermore, I implement an Inverse Probability of Treatment Weighting estimation to balance co-variates across these dimensions and provide a control group closer to the treatment one. To do so, I first intuitively drop from the control group, in order for it to become closer to the treated pairs' characteristics, non-European countries, bilateral flows that already shared the French franc before the beginning of the sample and bilateral flows among very large countries (the sample's top 5% in terms of product of population).

I then estimate a propensity score P as the fitted values of a probit model of French franc adoption

$$P(\text{Franc}_{ij} \mid Z_{ij}\theta) = 1 - \Phi(-Z'_{ij}\theta) \quad (4)$$

where Z is a set of potential confounders to be balanced, including distance, the

quadratic distance, the minimum and maximum population in 1858 within the pair i and j and variables Polity2 and Napoleon. I then weight the sample by $1/P$ for treated pairs and $1/(1-P)$ for control pairs. The balancing of covariates obtained through this weighting procedure is shown in Figure 9: the standardised difference in most covariates is substantially reduced, particularly for the years of Napoleonic occupation, with the difference between treated and control groups becoming statistically insignificant.

Table 8: Estimates Controlling for Time-invariant Co-variates

	(1) No Pair FE PPML	(2) PPML - IPT Weighted	(3) PPML - IPT Weighted
Franc	0.3427** (0.162)	0.402*** (0.134)	0.339* (0.184)
FTA	-0.157* (0.942)	0.173** (0.0769)	0.126* (0.0696)
Napoleon	0.0249 (0.0368)		
Observations	2,323	1,101	1,091
R-squared	0.9084	0.9898	0.9898
Sample	Collapsed	Collapsed IPTW	Collapsed IPTW
Pair FE	NO	YES	YES
Pair-specific Trend	NO	NO	YES
Implied Effect	0.408	0.494	0.404
RESET	0.083	0.756	0.715

Dependent variable: bilateral trade. Importer-time and exporter-time fixed effects, war and allied dummies included in all specifications but not reported. Controls for distance, common language and border, population size, polity2 score included in column 1 but not reported. Column 2 and 3 are estimated on a reduced sample, balancing average Napoleonic occupation, distance, polity2 score and bilateral population size across treated and non-treated pairs. Multi-way clustered standard errors reported in parenthesis. ***, ** and * denote statistical significance at the 0.01, 0.05 and 0.1 levels respectively.

Estimating the gravity equation on a sample balanced on key covariates, as shown in columns 2 and 3 of Table 8, provides treatment effects above the baseline estimate at around 40%.

Table 9: Balancing of Co-Variates Before and After IPT Weighting

	Original Collapsed Dataset			Inverse Propensity Weighted Dataset			
	Franc	Non-Franc	P-Value	Franc	Non-Franc	P-Value	St. Diff.
Napoleon	242	59.34	0.000	101.41	91.08	0.489	0.076
Distance	1065.445	1966.106	0.000	1307.67	1259.79	0.257	0.099
Polity2	-4.023	-7.657	0.000	-9.13	-8.57	0.568	-0.075
Population	47,700	220,000	0.000	49,678	51,850	1	-0.035

The table report the average of selected co-variates across franc and non-franc bilateral pairs, before and after the sample is weighted as described in Section 7.6.

8. Discussion

I now turn to a short discussion on the relevance of the paper’s results in the context of the wider literature on the common currency trade effect. I also tentatively highlight some channels that could explain the common currency pro-trade effect I find.

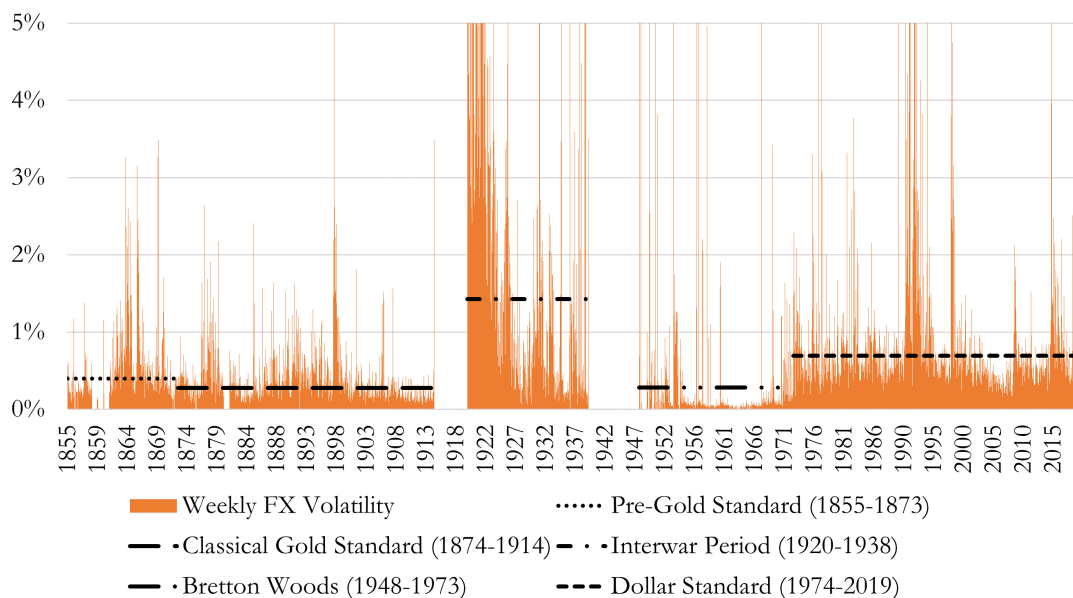
8.1. Comparison with Existing Estimates

The historical setting of the analysis raises the issue of results’ comparability with existing estimates of the common currency effect. In particular, one might wonder about the external validity of the treatment effect I examine. Indeed, the latter occurs in a context of commodity-based currencies linked to a metallic parity, where the role of FX volatility - a key plausible mechanism for a pro-trade common currency effect - might not come as intuitive. On the contrary, I will show below how transaction costs related to FX volatility in the period I examine are in line with those experienced during most of the common currency observations in the Rose literature.

Far from suppressing volatility, the behavior of the FX market during this period closely resembled the one of ”bands” exchange rate regimes²². Metallic currencies’ exchange rates typically fluctuated between upper and lower bounds, represented by

²²Given technological obstacles to the existence of a spot market, FX transactions in the 19th century were carried using a short-term negotiable instrument, the ”bill of exchange”.

Figure 4: FX Volatility Now and Then



Source: The Economist, The Banker’s Almanac, Bank for International Settlements, Global Financial Data and author’s calculations. Annual average of the standard deviation of weekly log-returns for eleven European currencies against the British Pound until WWII and the US Dollar afterward.

transaction costs to arbitraging²³, according to money demand conditions.

Figure 4 quantifies FX market volatility for key currencies over one and a half century, providing a snapshot of different volatility regimes. It notably shows how FX volatility in the pre-Gold Standard period is actually higher than during the Bretton Woods period, with a standard-deviation of weekly FX log-returns at around 0.4% vs. 0.28% for the 1945-1973 period. This analysis context in terms of trade cost stemming from FX volatility is then remarkably similar to the setting where the majority of currency unions observations considered by the literature occurs. Based on the computation of currency unions switches by De Sousa (2012), the Bretton Woods period represents 35% of the sample but 55% of currency union observations (65% excluding the Euro) in the Rose dataset commonly employed by the comon currency effect literature.

Furthermore, it is important to stress how the spread of the French franc throughout Europe in the mid-19th century was well understood by its promoters as a mean to

²³If the metallic parity is credible, arbitrageurs would self-stabilise the exchange rate around the metallic parity, but arbitrage would only kick-in once the deviation from parity is close to the cost of physically shipping metals.

reduce international transaction costs in a context of trade globalisation and European integration (See Section 3.1), with striking parallels with the recent EMU process.

Table 10 provides some more elements of comparison between the present paper’s results and the wider literature, as I replicate the key estimate by Glick and Rose (2016) for different sub-periods using PPML and high-definition fixed-effects²⁴. Estimates of the common currency effect in columns (1), (2) and (3) might be suggestive of heterogeneity in the effect related to different regimes in FX transaction costs.

The estimated coefficient in the moderately volatile pre-Gold Standard and Bretton Woods periods are very similar at around 30%, while the effect is much higher (above 100%) once the more volatile post-1973 period is included. However, the picture is significantly more blurred once slight changes to the sample are made in columns (4) and (5).

Table 10: The Common Currency Effect Now and Then

	(1)	(2)	(3)	(4)	(5)
	Pre-GS	Bretton Woods	Post-WWII	Post-1973	Post-WWII
Period	1852-1869	1948-1972	1948-2013	1973-2013	1948-2013
CU Considered	French Franc	All	All - No EMU	All - No EMU	All
% of CU Dyads	3.2%	2.5%	1.6%	1.1%	1.6%
Sample	Collapsed	G&R (2016)	G&R (2016)	G&R (2016)	G&R (2016)
Estimated Effect	0.302***	0.274***	0.716***	0.215*	0.155***
FX Volatility	0.4%	0.28%	0.5%	0.7%	0.5%

Column (1) reproduces the estimate in Table 1 Column 4. Estimates in Columns (2) to (5) are performed on the dataset by Glick and Rose (2016) using PPML, controlling for FTAs, colonial relationships and importer-year, exporter-year and importer-exporter fixed-effects and standard errors clustered at the exporter, importer and year levels. Source for average FX volatility is detailed below Figure 4.

A key reason to compare my results with the wider literature would be to gather some evidence on the size (and direction) of the endogeneity bias in the latter. It would be tempting to conclude, looking at meta-analyses as well as recent state-of-the-art estimates by Glick and Rose (2016) and Larch et al. (2018) - putting the common currency effect at close to 100% - that this bias is sizable. Nevertheless, as shown in

²⁴The estimate in column (3) therefore replicates column (5) in Table 1 of Larch et al. (2018).

Table 10, estimates based on data by [Glick and Rose \(2016\)](#) can be fairly sensitive to slight changes in specification and dis-aggregation of various currency unions. This could indicate that the common currency effect I obtain in quasi-experimental setting in the present paper might actually not be as low, in relative terms, as it might be suggested by a comparison with recent *preferred* estimates in the literature. As recalled above, [Chen and Novy \(2019\)](#) show the crucial role of heterogeneity driven by trade share in this respect.

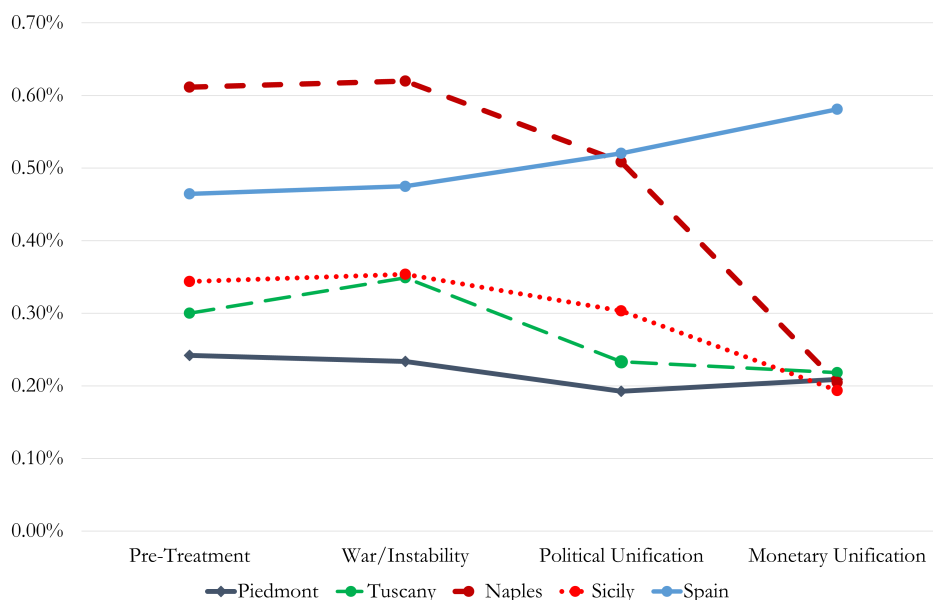
8.2. Channels

Channels commonly considered in the literature ([Baldwin, 2006](#)) include FX volatility, transaction costs related to the use of different currencies, as well as an increase in market transparency and efficiency. Thanks to original high frequency foreign-exchange data I examine in further detail in [Vicqu ery \(2021\)](#), I am able to provide some quantification for the first two factors for three of the treated Italian regions I consider: Tuscany, Naples and Sicily. I can compare their developments around the monetary unification shock with those of Piedmont, a longstanding member of the French franc zone, and Spain, a comparable peripheral country which does not adopt the French Franc.

Overall, the shock had a strong impact on both volatility and transaction costs but with some heterogeneity: it was significantly stronger in the more peripheral Southern regions of Italy compared to Tuscany. This is the mirror image of those regions having a much lower ex-ante probability of joining the French franc zone based on foreign-exchange regime determinants as discussed above. Figure 5, shows bid-ask spreads on trade-finance FX instruments quoted in London. Transaction costs for treated regions converged to the levels of Piedmont, but only after monetary unification and from different levels: while convergence for Tuscany and Sicily reflected a decrease of about 10bp in the spread, this figure was closer to 40bp for the continental South.

Data on FX and trade-finance instruments also show how the treatment specifically reduced transaction costs between the annexed Italian regions and the French franc area. Figure 6 shows how the correlation of returns of trade-finance instruments quoted in London between the same countries and France dramatically increased following

Figure 5: Bid-Ask Spread on Trade-Finance Instruments Quoted in London



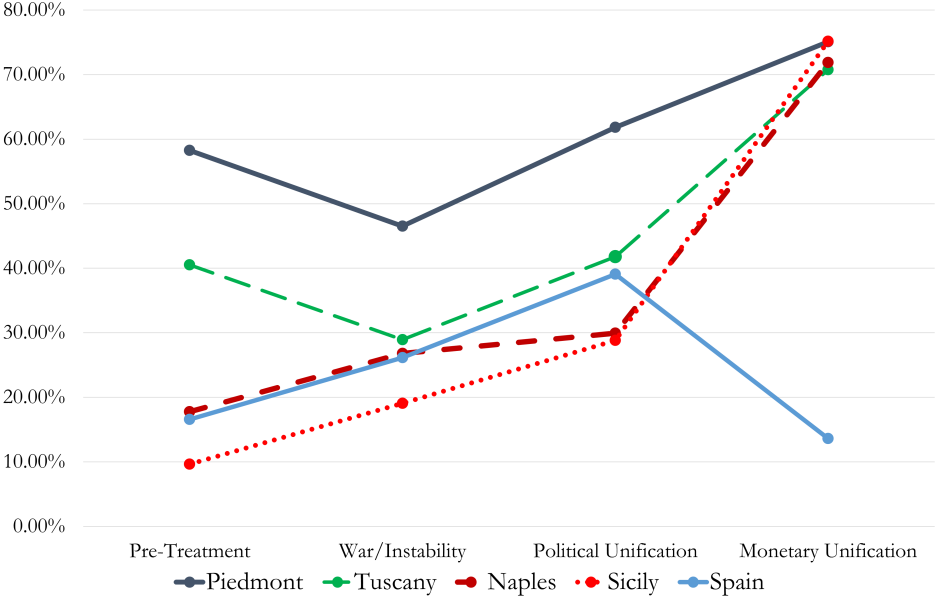
Source: The Economist, author’s calculations. Bid-Ask Spread on 3-Months Bills of Exchange quoted in The Economist averaged across key periods. Pre-Treatment: 1952-1958. War/Instability: 1859-1860. Political Unification: 1861-1862. Monetary Unification: 1863-1865. Period definition for Tuscany reflects a slightly different timing of events described in Section 6.

their adoption of Piedmont’s currency. The monetary unification shock is sizable for Tuscany, where co-movement with France increase by about 30pp following treatment, but dramatically larger for Southern Italian regions, where co-movement with France was orders of magnitude lower pre-treatment than post-treatment.

All in all, the above data can be interpreted as evidence of alternative channels through which a pro-trade common currency effect might materialise, beside reduction in volatility and increase in market transparency. In particular, the Italian monetary unification shock allowed annexed regions to benefit from significantly easier access to French trade finance, as signaled by a marked increased of money market co-movement.

Furthermore, the pattern of heterogeneity in the magnitude of the monetary unification shock shown by financial data fits well with the heterogeneity in the estimated effect shown in Table 14. Tuscany experienced milder shifts in the currency and money markets following monetary unification and is shown to increase the treatment effect once dropped from the dataset.

Figure 6: Co-movement of Returns vs. France - Trade-Finance Instruments Quoted in London



Source: The Economist, author's calculations. Correlation of 3-Months Bills of Exchange's return vs. France as quoted in The Economist across key periods. Pre-Treatment: 1952-1958. War/Instability: 1859-1860. Political Unification: 1861-1862. Monetary Unification: 1863-1865. Period definition for Tuscany reflects a slightly different timing of events described in Section 6.

9. Conclusion

This paper fills an important gap in the literature on the trade effect of common currency by providing for the first time an estimate of the common currency relying on a quasi-experiment which can be interpreted as causal.

The estimated effect is large and significant, around 35%. The identification strategy, which relies on an exogenous variation in the membership of the 19th century French franc monetary bloc, is resilient to a wide range of robustness checks. Importantly, coincident factors are shown not to be driving my findings.

The effect is significantly smaller than the average one found in the common currency literature according to recent meta-analyses ([Head and Mayer, 2014](#); [Rose, 2017](#)), suggesting that endogeneity bias might have been playing a role in previous work. Nevertheless, my results are relatively close to the lower bound of recent correctly specified estimates such as in [Glick and Rose \(2016\)](#).

All in all, my results corroborate the broader policy implications of Rose's seminal paper in a quasi-experimental setting. A common currency is found to have large - if heterogeneous - pro-trade effect unbiased by endogeneity and self-selection.

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10 Results Appendix

Table 11: Descriptive Statistics - Collapsed Sample

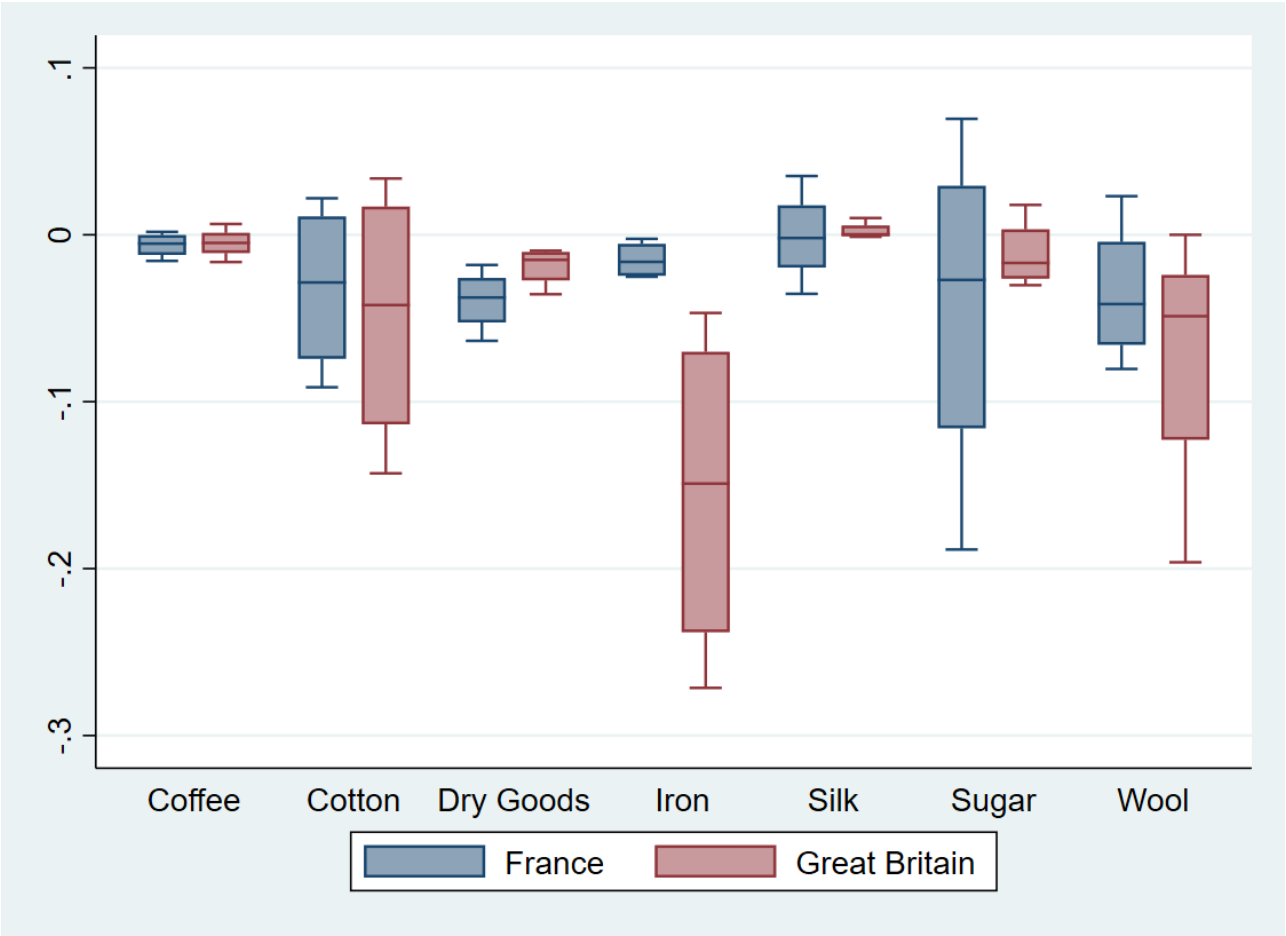
Variables	Non-Treated Flows			Treated Flows			All Flows		
	N	Mean	S.D.	N	Mean	S.D.	N	Mean	S.D.
Imports	4695	33,500	101,000	275	7,877	11,000	4970	32,100	98,600
Distance	6408	1,966.11	1,847.02	432	1,065.45	313.02	6840	1,909.22	1,802.83
Border	6408	0.06	0.23	432	-	-	6840	0.05	0.22
Language	6408	0.10	0.29	432	-	-	6840	0.09	0.29
Population.i	6408	15,679	16,725	432	9,046	12,753	6840	15,260	16,580
Franc	6408	-	-	432	0.43	0.50	6840	0.03	0.16
Franc (End.)	6408	0.03	0.18	432	-	-	6840	0.03	0.17
FTA	6408	0.22	0.41	432	0.50	0.50	6840	0.24	0.42
Napoleon	6408	59.34	111.99	432	242.00	161.73	6840	70.88	123.98
Polity2	6408	- 7.66	8.45	432	- 4.02	8.06	6840	- 7.43	8.47
War	6408	0.01	0.09	432	-	-	6840	0.01	0.08
Allied	6408	0.01	0.10	432	-	-	6840	0.01	0.09

Table 12: Robustness to Different Specifications

	(1)	(2)	(3)
	PPML	PPML	PPML
Franc	0.316** (0.131)	0.291*** (0.0504)	0.326*** (0.121)
FTA	-0.0278 (0.0424)	-0.0781*** (0.0172)	-0.0409 (0.0267)
Observations	2,071	2,088	2,088
Estimator	PPML	PPML	PPML
Sample	Collapsed	Collapsed	Collapsed
Pair-specific Trend	YES	NO	YES
Symmetric Pair FE	NO	YES	YES
Implied Effect	0.371	0.337	0.385
RESET Test (P-Value)	0.000	0.00215	0.000
R-squared	0.9877	0.972	0.977

Dependent variable: bilateral trade. Importer-time and exporter-time fixed effects, war and allied dummies included in all specifications but not reported. Columns 1 include bidirectional pair fixed-effects. Columns 1 and 2 include unidirectional (symmetric) pair fixed-effects. Columns 1 and 3 allow for a pair-specific trend. Multi-way clustered standard errors reported in parenthesis. ***, ** and * denote statistical significance at the 0.01, 0.05 and 0.1 levels respectively.

Figure 7: Weighted Tariff Reduction Across Italian Regions by Major Product and Partner



Source: [Stringher \(1889\)](#), [Ministère du Commerce \(1878\)](#).

The chart plots the range of the reduction achieved by the annexed regions for the seven main import products weighted by their importance in the import share of France and Britain in each region.

Table 13: Estimates Across Different Control Groups

	(1)	(2)	(3)	(4)	(5)	(6)
	Only Europe	No Overland	Both	Only Europe	No Overland	Both
Franc	0.220** (0.106)	0.449*** (0.0782)	0.327** (0.136)	0.204** (0.101)	0.369*** (0.0901)	0.272** (0.127)
FTA	-0.0137 (0.0671)	0.0155 (0.0307)	0.0823 (0.0738)	-0.0678 (0.0854)	-0.0818* (0.0489)	0.0117 (0.0739)
Observations	2,944	3,484	2,248	1,422	1,654	1,082
R-squared	0.990	0.993	0.996	0.992	0.994	0.996
Sample	All Years	All Years	All Years	Collapsed	Collapsed	Collapsed
Implied Effect	0.246	0.567	0.387	0.226	0.446	0.312
RESET	0.0104	0.395	0.0398	0.0157	0.483	0.499

Dependent variable: bilateral trade. PPML estimator. Bidirectional pair, importer-time and exporter-time fixed effects, war and allied dummies included in all specifications but not reported. Multi-way clustered standard errors reported in parenthesis. ***, ** and * denote statistical significance at the 0.01, 0.05 and 0.1 levels respectively. Columns 1, 3, 4 and 6 exclude non European partners (USA, Russia, Turkey) from the control group. Columns 2, 3, 5 and 6 exclude partners more exposed to overland trade with Italy (Austria, Zollverein) from the control group.

Table 14: Estimates Obtained Dropping One Country at a Time

Country	Franc	Country	Franc
Austria	0.291***	Russia	0.357***
Belgium	0.254**	Sardinia	0.326***
Britain	0.282**	Sicily	0.294***
France	0.640***	Spain	0.285***
Greece	0.302***	Sweden and Norway	0.311***
Naples	0.290***	Switzerland	0.310***
Netherlands	0.337***	Turkey	0.277***
Portugal	0.306***	Tuscany	0.392***
Romagna	0.260***	USA	0.202**
Papal States	0.296***	Zollverein	0.352***

Dependent variable: bilateral trade. PPML estimator. Bidirectional pair, importer-time and exporter-time fixed effects, FTA, war and allied dummies included in all specifications but not reported. Multi-way clustered standard errors reported in parenthesis. ***, ** and * denote statistical significance at the 0.01, 0.05 and 0.1 levels respectively. The coefficients shown correspond to estimates obtained dropping one country at a time from the sample and can be compared to the overall coefficient obtained in column 4, Table 3.

Table 15: Bilateral Asymmetries

Exporter-Importer	Franc Estimate	Exporter-Importer	Franc Estimate
France-Tuscany	0.0538 (0.148)	Tuscany-France	0.229 (0.153)
France-Romagna	1.576*** (0.266)	Romagna-France	2.680*** (0.318)
France-Naples	0.0978 (0.199)	Naples-France	0.718*** (0.233)
France-Sicily	0.298 (0.183)	Sicily-France	0.566*** (0.136)
Belgium-Tuscany	1.266*** (0.158)	Tuscany-Belgium	0.0631 (0.232)
Belgium-Naples	1.255*** (0.296)	Naples-Belgium	0.500** (0.226)
Belgium-Sicily	0.252* (0.139)	Sicily-Belgium	0.707*** (0.119)
Switzerland-Naples	-0.450 (0.299)	Naples-Switzerland	1.625*** (0.215)

Dependent variable: bilateral trade. PPML estimator. Bidirectional pair, importer-time and exporter-time fixed effects, FTA war and allied dummies included in all specifications but not reported. Multi-way clustered standard errors reported in parenthesis. ***, ** and * denote statistical significance at the 0.01, 0.05 and 0.1 levels respectively. The table compares bilateral asymmetries in the estimated effect when separate coefficients can be estimated without incurring in colinearity issues.

11 Historical Appendix

Piedmontese Prime Minister Cavour, an influential figure in European diplomatic circles, is without doubt the key character behind the events that unfolded in the Italian peninsula around the unification. Nevertheless, Cavour never planned for Italy to become unified nor pursued territorial expansion towards the South of the peninsula before events forced him in that direction ([Mack Smith, 1958](#)). He described as "silly non-sense" the mere idea of Italian unification in a letter to a close adviser as late as 1856. The goals of Cavour were, more practically, territorial expansion into Lombardy-Venetia and a weakening of Austrian influence in the peninsula.

Conservative elites in Northern Italy might have been attracted to the idea of an anti-Austrian Italian confederation put forward by moderate intellectuals such as Vincenzo Gioberti. However, they largely opposed Italian nationalism as a dangerous, radical proposition. Their views and interests contrasted markedly with the ones of the small extremist groups of conspirators who supported the Italian national cause. Unification and Italian nationalism were largely championed by secret networks of far-left republicans such as Giuseppe Garibaldi and Giuseppe Mazzini, often resorting to terrorism and unrest. Several attempts to foster popular revolts in favor of Italian nationalism failed miserably in the fifty years preceding unification, due to lack of broad-based support.

Paradoxically, that same fear of political instability, which was inherently associated with Italian nationalism in the first part of the 19th century, ultimately pushed the Piedmontese elite to support the creation of a centralized Italian state, in the aftermath of the war against Austria in 1859.

In 1858 Cavour and Napoléon III met in Plombières to negotiate France's military support against an imminent Austrian attack on Piedmont-Sardinia. The two statesman agreed, in case of victory against Austria, on Piedmont territorial expansion into Lombardy and Venetia and on the creation of a loose Italian confederation. This was very far from the centralized political, economic and monetary union that unfolded only a couple of years later.

The success of the French-Piedmontese armies in the first stage of the 1859 campaign

led, as expected, to the annexation of Lombardy by Piedmont-Sardinia. However, several unintended developments tilted the course of events towards Italian unification.

First, Napoléon III dropped his support for Piedmontese expansion into Venetia, frustrating Cavour's ambitions. Central and Eastern Italy are to remain under Austrian influence and an Italian confederation presided by the Pope is to be created. This partial disavowal of the original French-Piedmontese pact made the conservative Piedmontese elite keener on bolder initiatives against the status quo.

Second, anti-Austrian riots took place in several cities in central Italy in the second half of 1859. Faced with the risk of revolution in neighboring states and the opportunity of further territorial expansion, Cavour skilfully secured the annexation of the smaller Italian duchies as well as the Granduchy of Tuscany and the Romagna territories by March 1860.

Third, in May 1860, Garibaldi's private militias landed in Sicily. Cavour secretly opposed the expedition and unsuccessfully attempted to stop it by requesting the intervention of France. However, the Sicilian and Neapolitan army unexpectedly collapsed against an out-numbered Garibaldi. As Garibaldi's expedition was able to march on Naples by August 1860, Cavour was once again forced to adapt. The threat of a revolutionary government in Naples pushed Cavour and the great powers to support the annexation of the Two Sicilies by Piedmont-Sardinia in October 1860. As a result, in March 1861 the Kingdom of Italy was officially formed, paving the way for the unification process.

Table 16: Timeline of the Italian Unification

Date	Event
July 1858	Secret French-Piedmontese pact negotiated by Napoléon III and Prime Minister Cavour, supporting Piedmont's expansion into Lombardy and Venetia against Austria.
April 1859	French-Piedmontese war on Austria.
May 1859	Anti-Austrian unrest in central Italy; pro-Piedmont revolutionary governments take power in Tuscany and Romagna.
June 1859	Austria evacuates Milan.
July 1859	Peace of Villafranca: France partially reneges on its promises. Lombardy is to be annexed by Piedmont but not Venetia. Tuscany and Romagna should remain under Austrian influence. An Italian confederation presided by the Pope is to be created. Cavour resigns in protestation to the missed annexation of Venetia.
November 1859	Treaty of Zurich: France and Austria reiterates their commitment to the principles of the Peace of Villafranca, while Piedmont is largely excluded from the negotiation table. Lombardy is nominally ceded to France and transferred to Piedmont.
December 1859	Diplomatic crisis, as pro-Piedmont governments in central Italy refuse to hand power back to Austria. Cavour is recalled to head the cabinet. He exploits French-British rivalries to overrule the Peace of Villafranca and obtains the annexation of Tuscany and Romagna through plebiscites, in exchange for French annexation of Savoy and Nice.
May 1860	Revolutionary militias led by Garibaldi land in Sicily. Cavour opposes the expedition.
August 1860	The unexpected collapse of the Two Sicilies Kingdom against Garibaldi forces Cavour to adapt; the threat of a revolutionary republic in Naples is defused by the annexation of the South by Piedmont.
March 1861	The Kingdom of Italy is proclaimed.

Source: [Romeo \(1984\)](#).

12 Data Appendix

12.1 Sources

As the most detailed original source compiled by French diplomats is sometimes missing from the Archives of the French Foreign Office, I rely on three distinct sources:

1. Where available, I employ the original *Etats de Commerce et de Navigation*'s statistical tables from the French Foreign Ministry Archives in Paris. Most of them are filed in the *Affaires Diverses Commerciales* series of the French *Archives Diplomatiques*, under "Varia", *Cartons* 654-660. Others are filed with the *Correspondance Consulaire et Commerciale* series, within each Consulate's correspondence. The original tables offer the highest level of details but were filed inconsistently and are missing for various years/countries.
2. Fortunately, a high number of the trade reports received by the Foreign Ministry were later published by the [Ministère du Commerce \(1878\)](#) as part of the *Annales du Commerce Extérieur*, a printed publication aimed at a business audience. The statistical tables contained in the *Annales* were at times condensed and provided less details in terms of trade partners but, combined with the original tables, they still allow to build a bilateral trade matrix for most of the years and partners I consider.
3. Finally, I employ equivalent reports from the British consulates in Italy to fill some of the reporting gaps in the dataset. Those reports were very similar in nature to the French ones, likely relying on the same local sources, and were published inconsistently in the Parliamentary Papers²⁵.

12.2 Shortcomings of the data

My dataset has, by construction, two key limitations. First, intra-Italian trade stops being recorded post-unification and is therefore discarded from the dataset. In any case, this is of little relevance for the paper's objective, as it would have been

²⁵For a detailed survey of the British consular reports on Italian trade see [Marks \(1959\)](#).

difficult to disentangle an intra-Italian common currency effect from the reduction in trade costs coming from other aspects of unification.

Second, Italian trade data in my dataset only refer to trade by sea. This is due to the nature of the consular trade data, which were recorded at the ports. The ports considered are Genoa for Piedmont-Sardinia, Livorno for the Granduchy of Tuscany and Ancona for the Romagna. Total maritime trade is reported in both French and British consular statistics for both the continental South and Sicily.

Not being able to observe overland trade could potentially introduce a bias in my estimates, as the development of a national railway system starting in 1861 is likely to have increased overland international trade: I could then see trade with some partners spuriously declining as it shifts from maritime to overland transportation. Trade patterns for Piedmont-Sardinia could also be affected by unification-induced border changes with polities that continue to exist. However, both the territories gained from Austria and the region of Savoy ceded to France were unlikely to be significantly involved in maritime trade to and from Piedmont-Sardinia²⁶.

More importantly, overland trade is only marginally relevant for the Central-Southern regions of Italy I consider. Overland trade within the Italian peninsula South of the Po Valley was close non-existent pre-unification. Post-unification, the Italian railway system was developed with a military, rather than economic, rationale. It remained substantially costlier than sea shipping and is normally considered to have been unable to foster the rise of an integrated national market in the first decades of unification (Fenoaltea, 2011). In any case, an increase in overland international trade is likely to imply a downward bias to the estimate of the common currency effect. Indeed, France and Switzerland being bordering countries, they should be proportionally more affected by this potential shift from sea to overland trade compared to the majority of "control" trade partners in the sample. As a robustness check, in some specifications I exclude from the sample non-treated countries, such as Austria and Germany, that are more exposed to an increase in overland trade from Italy and could bias the estimated effect upward.

²⁶Nice was also ceded to France by Piedmont and might have been involved in maritime trade to Genoa after 1861, but it represented less than 0.4% of French population in 1860.

12.3 Missing Observations and Data Aggregation

I address missing reporting of trade and measurement errors in my original data aggregating up trade flows in a "collapsed" dataset. The latter is sampled by averaging trade at the ij level over two or three years periods. This not only ensures that within each period every treated Italian state reports at least once but also likely reduce measurement errors that are bound to exist in historical sources. This sample leaves me with three pre-treatment periods between 1852-1858, one period corresponding to the wars and annexation process (1859-1860), one period post-unification but before monetary unification, and three post-unification treatment periods.

Truncated reporting in my Italian sources (the smallest partners are not always reported) means that some of the missing values in the trade matrix are not actually zero. Furthermore, some bilateral flows are not observed by construction, either because they concern intra-Italian flows post-unification or pairs of countries that are both not reporting trade (eg. Turkey and the Papal States). When this is the case, I follow [Glick and Rose \(2002\)](#) and exclude missing values from the sample. In the full sample, this leads to 4970 actually recorded flows out of 6840 theoretical trade flows.

Table 17: Sampling and Missing Reporters by Year

Year	Period	All Italian States Reporting^a	Collapsed Sample^b
1852	Pre-Unification	NO (Naples)	
1853	Pre-Unification	YES	1852-1854
1854	Pre-Unification	YES	
1855	Pre-Unification	YES	
1856	Pre-Unification	YES	1855-1856
1857	Pre-Unification	YES	
1858	Pre-Unification	YES	1857-1858
1859	Annexation (North)	NO (Sicily)	1859-1860
1860	Annexation (South)	NO (Naples)	
1861	Unification	YES	1861-1862
1862	Unification	NO (Naples)	
1863	Monetary Unification	YES	1863-1864
1864	Monetary Unification	YES	
1865	Monetary Unification	YES	
1866	Monetary Unification	NO (Romagna)	1865-1866
1867	Monetary Unification	NO (Tuscany)	
1868	Monetary Unification	NO (Sicily)	1867-1869
1869	Monetary Unification	NO (Sicily)	

^a Missing State reported in parenthesis.

^b Trade data are collapsed over three or two years period so that in every period all annexed Italian states report at least once.

Figure 8: Statistical Tables - *Etats de Commerce et de Navigation*

Exportations des Provinces Napoléoniennes pour l'année 1864.

<i>Pays de Destination</i>	<i>Amandes et Noisettes</i>	<i>Blés</i>	<i>Bois</i>	<i>Chaux et Lins</i>	<i>Crème de Tartre</i>	<i>Coton</i>	<i>Fruits secs et légumes</i>	<i>Porcelaine</i>	<i>Seraines de Lin</i>	<i>Huiles</i>	<i>Laines</i>
<i>Angleterre</i>	450,000	700,000	300,000	1,600,000	380,000	4,800,000	280,000	1,000,000	100,000	5,000,000	700,000
<i>Amérique</i>	250,000	"	"	"	"	"	300,000	"	"	500,000	"
<i>Autriche</i>	500,000	"	"	600,000	100,000	2,000,000	150,000	"	200,000	2,000,000	400,000
<i>Belgique</i>	100,000	"	"	500,000	50,000	1,500,000	100,000	150,000	200,000	100,000	50,000
<i>Espagne</i>	"	"	350,000	"	"	"	"	"	"	"	"
<i>France</i>	750,000	1,800,000	800,000	2,100,000	200,000	8,200,000	700,000	2,000,000	800,000	3,500,000	800,000
<i>Hollande & Diles lles</i>	100,000	"	"	200,000	50,000	"	50,000	"	200,000	1,000,000	"
<i>Italie du Nord</i>	300,000	600,000	200,000	700,000	200,000	3,500,000	400,000	200,000	500,000	1,500,000	100,000
<i>Prusse</i>	"	"	"	"	"	"	"	"	"	1,000,000	"
<i>Grèce & Turquie</i>	"	"	"	"	"	"	"	"	"	"	"
<i>Russie</i>	"	"	"	"	"	"	"	"	"	"	"
<i>Suisse</i>	"	"	200,000	"	"	1,100,000	"	"	100,000	200,000	"
<i>Total</i>	2,450,000	3,100,000	1,850,000	5,900,000	980,000	21,100,000	1,980,000	3,300,000	2,400,000	17,600,000	1,000,000
<i>Total de l'année antérieure</i>	2,300,000	3,100,000	2,300,000	5,700,000	1,000,000	19,500,000	2,000,000	4,200,000	2,000,000	18,100,000	1,000,000
<i>Différence</i>	<i>En plus</i> 150,000	"	"	200,000	"	1,600,000	"	"	400,000	"	"
	<i>En moins</i> "	"	"	"	20,000	"	20,000	850,000	"	500,000	"