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Contractual Risk Transfer and the Pace of Fibre Deployment

Evidence from French Public Initiative Networks

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Abstract

Transferring investment risk to a private delegatee is frequently advocated as a means of accelerating the construction of public infrastructure. Yet its effectiveness remains ambiguous: an operator that finances the asset has a direct interest in completing it quickly, but also bears a financing cost that a publicly funded operator does not. In France, fibre deployment in commercially unattractive territories is entrusted to Public Initiative Networks, whose delegating authorities choose between arrangements that retain investment responsibility and arrangements that transfer it. Using a quarterly panel of 56 networks observed from 2018 to 2026, constructed from the regulator's commune-level open data, we investigate whether this choice is associated with the pace at which networks cover their territories. Our econometric analysis, based on a measure of deployment effort normalised by the remaining stock of connectable premises and on randomisation inference at the network level, yields two key findings. First, networks transferring investment responsibility cover their residual territory 4.6 percentage points faster per quarter, roughly thirty percent above the sample mean and some fifteen months in the date at which a territory is substantially served. Second, the association is concentrated among small and medium-sized networks and attenuates among large regional consortia, suggesting that the layering of governance across tiers of local government dilutes the incentives contractual form is meant to create. These findings provide new insights into the organisational design of subsidised infrastructure programmes, suggesting that the contractual architecture through which public money is spent deserves attention alongside its amount and its targeting.

Keywords: public service delegation; incomplete contracts; fibre broadband; investment risk; infrastructure deployment.

JEL classification: L96, L33, H54, D23, R58.

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1 Introduction

When a public authority delegates the construction and operation of a network infrastructure, one of its first decisions is how much investment risk to transfer to the private party. At one end of the spectrum, the authority finances the asset and delegates only its operation; at the other, the operator finances, builds and recovers its outlay from user revenues over a long horizon. The theory of incomplete contracts suggests that this choice matters. An operator whose remuneration depends on the volume of connected users, and who has sunk the capital required to reach them, has a direct financial interest in completing the network quickly. An operator paid to run an asset it does not own has weaker incentives to accelerate construction, and the authority bears the resulting delay (Williamson, 1985; Grossman and Hart, 1986; Hart, 1995; Engel et al., 2014).

The question is not merely theoretical. Across Europe, the deployment of fibre-to-the-home infrastructure in low-density areas has depended on public subsidy, and the sums involved are considerable: the French *Plan France Très Haut Débit* alone mobilised more than thirteen billion euros of public and private money. How that money is spent – and in particular the contractual architecture through which local authorities delegate construction to private operators – has received far less scholarly attention than whether it reaches the intended territories (Briglauer et al., 2019; Bourreau et al., 2023). Yet the pace of deployment carries real welfare consequences. A body of work documents substantial returns to broadband access in labour markets, firm location and entrepreneurship (Akerman et al., 2015; Kim and Orazem, 2017; Conroy and Low, 2022), returns that were amplified by the pandemic shift to remote work (Isley and Low, 2022; McArthur and Hong, 2023). A delay of several quarters in reaching a rural commune is not a bureaucratic detail; it is a period during which the households and firms of that commune forgo these returns.

The French fibre programme offers an unusually clean setting in which to examine the contractual question. Between 2011 and 2021, roughly sixty *Réseaux d'Initiative Publique* (RIPs) were created to deploy fibre in territories that private operators had declared commercially unattractive. Each is governed by a long-term delegation contract, and the contractual forms used span the full range of investment-risk allocation: direct public management, *affermage* (operation delegated, investment retained), affermo-concessive arrangements, concessions, and public–private partnerships. Because these networks deploy comparable technology under a common regulatory framework and a common national subsidy scheme, they differ from one another primarily in the contractual architecture governing them and in the territories they serve. The regulator, moreover, publishes commune-level deployment data quarterly, which makes it possible to observe the construction process itself rather than only its endpoint.

This paper asks whether the degree of investment-risk transfer is associated with the pace at which these networks cover their territories. We assemble a quarterly panel of 56 RIPs observed between the third quarter of 2018 and the first quarter of 2026, built from ARCEP's commune-level deployment files and linked to contractual information through a verified matching procedure. Deployment effort is measured as the share of the remaining connectable premises that a network covers within a given quarter. This normalisation matters: a network that has already covered ninety percent of its territory cannot mechanically deploy at the same absolute rate as one at twenty percent, and comparing raw increments would confound effort with maturity.

Our central estimate is that networks whose contracts transfer investment risk deploy 4.6 percentage points faster per quarter than those that do not, against a sample mean of roughly 15% – an increase of about thirty percent in quarterly pace. The estimate holds when the analysis is conducted at the commune rather than the network level, when marginal contractual forms are excluded, on the balanced sub-panel of networks observed throughout, and under a dynamic panel estimator addressing the endogeneity of the maturity control. Because the treatment is assigned at the network level and the number of clusters is moderate, we base inference on randomisation of the treatment assignment across networks rather than on asymptotic cluster-robust standard errors (Cameron and Miller, 2015); this returns $p = 0.002$ for our preferred specification. As with any comparison based on a contractual form that authorities choose rather than receive at random, the estimate should be read as a conditional association, and Section 5 reports a sensitivity analysis to selection on unobservables.

The paper makes four contributions. First, it constructs a network-level panel of French RIPs with verified contractual forms observed over 26 quarters, built entirely from open regulatory sources and reproducible from the code we provide; to our knowledge no comparable dataset exists for this sector. Second, it shifts the empirical focus of the broadband literature from deployment coverage to *deployment pace*, a quantity that maps onto investment effort rather than onto its cumulative outcome, and which carries the welfare consequences that the literature on the returns to broadband access has documented.

Third, the paper contributes to the empirical literature on incomplete contracts and public–private partnerships by testing the risk-allocation prediction on the margin that the theory actually addresses. That literature has established that contractual form is associated with prices (Chong et al., 2006; Carpentier et al., 2006) and with ex post service quality, but the mechanism it invokes – the incentive to exert non-contractible investment effort (Hart, 2003; Iossa and Martimort, 2012) – operates during construction, a phase that is rarely observed at sufficient frequency to be studied directly. The quarterly structure of the French regulatory data allows us to bring evidence to bear on the construction margin itself rather than on its downstream correlates.

Fourth, it contributes to the evaluation literature on public intervention in broadband markets by opening what has largely remained an institutional black box. That literature has concentrated on whether subsidies are well targeted and whether they crowd out private investment (Briglauer et al., 2019; Bourreau et al., 2023; Gerpott and Ahmadi, 2020), treating the delivery mechanism as a neutral conduit. Our results indicate that the organisational design of the subsidy – how the contract allocates investment responsibility between the authority and the operator – is associated with deployment outcomes of a magnitude comparable to those attributed to the subsidy itself, which suggests that evaluations of state aid in network industries would benefit from conditioning on contractual architecture rather than treating recipients as homogeneous.

This paper is organized as follows. Section 2 reviews the relevant literature and describes the institutional setting. Section 3 presents the data and the construction of the panel. Section 4 sets out the empirical strategy. Section 5 reports the results and the sensitivity analysis. Section 6 discusses their interpretation and their implications for the design of delegation contracts. Section 7 concludes.

2 Literature and institutional setting

2.1 Risk allocation in long-term delegation contracts

The economics of public service delegation treats contractual form as an instrument for allocating risk and residual control rights rather than as a technical detail. Two intellectual traditions converge on this view. In the transaction-cost approach initiated by Williamson (1985) and extended to institutional change by North (1990), the appropriate governance structure depends on asset specificity, uncertainty and the frequency of transactions; the more specific the asset and the more uncertain the environment, the greater the exposure to post-contractual hold-up and the stronger the case for integrating rather than contracting. In the property-rights approach of Grossman and Hart (1986) and Hart (1995), ownership determines who receives the residual returns to non-contractible effort, and hence who has an incentive to exert it. Both traditions imply that the same physical asset, built under different contractual architectures, will be built differently.

Applied to infrastructure specifically, these frameworks generate sharper predictions. Hart (2003) shows that bundling construction and operation under a single contract is preferable when the quality of construction is observable through operating costs, and worse when it is not: bundling gives the builder an incentive to internalise the operating consequences of its construction choices, but also an incentive to cut unobservable quality. Engel et al. (2014) analyse the concession as a device transferring demand risk to the party better placed to manage it, at the cost of a risk premium that the authority ultimately pays. Iossa and Martimort (2012) formalise the trade-off: the optimal degree of risk transfer increases with the operator's ability to influence the outcome, and decreases with the operator's risk aversion and the exogeneity of the risk. In a deployment context the first term dominates, because the pace of construction is almost entirely under the operator's control, whereas the demand risk that concessions also transfer is largely exogenous.

A parallel literature examines what happens to these arrangements after signature. Guasch (2004) documents that a large share of infrastructure concessions in Latin America are renegotiated within a few years of award, often because the contractual specifications proved inadequate for the actual operating environment. Athias and Saussier (2010) show that the probability of renegotiation depends systematically on contractual rigidity and on the regulatory environment. The implication is that risk allocation as written need not correspond to risk allocation as realised: a concessionaire facing revenue shortfalls may renegotiate rather than absorb them, which attenuates the incentive differential that contractual form is supposed to create. Any estimate based on the contractual form as signed is therefore likely to be a lower bound on the differential that a fully enforced contract would produce.

Empirically, the literature on French local public services has repeatedly found that contractual form matters for outcomes, and that the choice of form is itself endogenous to the characteristics of the delegating authority. Chong et al. (2006) document systematic price differences between management modes in the water sector. Boyer and Garcia (2008) show that the choice between direct public management and delegation responds to municipal characteristics that also affect prices, so that naive comparisons overstate the delegation effect. Carpentier et al. (2006) reach a similar conclusion using a different identification approach on the same sector, while Martínez-Espiñeira et al. (2009) replicate the exercise on Spanish data. Guelmamen (2025) and Guelmamen et al. (2025) examine how the institutional form of the delegating authority – specifically, inter-municipal cooperation – conditions service performance, and find that the coordination costs of cooperation can dominate its scale economies during transitional periods. The methodological response in this literature has typically been sample selection correction following Heckman (1979), applied for instance by Chen and Williams (2007) to the contracting-out of municipal parks services, or correlated random effects following Mundlak (1978) and Wooldridge (2019).

Our contribution to this strand is to examine an outcome that lies closer to the theoretical mechanism than the price or quality measures usually studied. Prices reflect a bargaining outcome as much as a productive one, and service quality in a mature network is a function of maintenance effort rather than of construction effort. The pace at which an asset is built is, by contrast, a direct measure of the investment effort that risk allocation is supposed to incentivise.

2.2 Public subsidy, deployment, and the returns to connectivity

A second literature evaluates public intervention in broadband markets. The starting point is the observation that private operators underinvest in low-density areas. Briglauer and Gugler (2013) document the lag of European member states in fibre deployment and attribute part of it to regulatory design; Briglauer et al. (2019) show that regulatory incentives alone do not generate investment in rural areas, motivating direct subsidy programmes, and evaluate whether German state aid closed the divide. Gerpott and Ahmadi (2020) synthesise the empirical determinants of next generation access deployment across this literature, and note that the institutional characteristics of the deploying entity are among the least studied. On French data specifically, Bourreau et al. (2023) estimate that public subsidies under the Plan France Très Haut Débit reached municipalities that private investment would not otherwise have covered in the large majority of cases, addressing the crowding-out concern that motivates much of the state aid debate, while Marty (2013) provides a complementary legal and economic analysis of the public–private partnership configurations mobilised in French telecommunications infrastructure and identifies the allocation of construction risk as the principal dimension along which they differ.

A closely related body of work estimates the returns to broadband access, and it is what makes the *timing* of deployment, rather than its eventual completion, the economically relevant outcome. Akerman et al. (2015) exploit a Norwegian programme with staggered municipal rollout and establish that broadband adoption is complementary to skilled labour and substitutable for unskilled labour; the estimate is identified precisely from variation in when connectivity arrives, which is the margin that contractual architecture might influence. Kim and Orazem (2017) show that broadband availability shapes new firm location decisions in rural America, and Conroy and Low (2022) document disproportionate increases in women-owned business formation, indicating that the returns are not uniformly distributed within the receiving population. Briglauer et al. (2021) find that regional benefits spill over beyond the connected county, so that delay in one territory imposes costs on its neighbours. The pandemic sharpened these effects: Isley and Low (2022) show that connectivity conditioned rural employment outcomes during COVID-19, and McArthur and Hong (2023) that slow connections constrained home-working opportunities, disproportionately in the rural areas that public initiative networks are designed to serve.

Two implications follow. Because these returns accrue from the date of connection onwards and are found to be persistent rather than transitory, differences in deployment pace across otherwise similar territories translate into permanent differences in cumulative benefit even where all territories are eventually covered. The convergence of coverage rates that the Plan THD will ultimately achieve therefore does not imply convergence of the benefits captured along the way. And a territory connected in 2021 rather than 2024 captured three additional years of these returns at a moment when they were unusually high.

Yet this literature has concentrated on whether public money reaches the intended territories and on the aggregate coverage it produces. The organisational question – how the delegating authority structures the contract through which the money is spent, and whether that structure affects how quickly the territory is served – has received far less attention. The closest antecedent is Beuve et al. (2022), who examine 72 French RIPs and find that concessive and affermo-concessive arrangements outperform SPL-affermage configurations in deployment terms. We depart from their analysis in two respects. We observe networks quarterly rather than cross-sectionally, which allows us to distinguish pace from cumulative coverage. And we normalise the outcome by the remaining stock, which removes the mechanical relationship between maturity and measured deployment: without this normalisation, a network that started earlier will appear to deploy more, irrespective of its effort.

2.3 The French institutional framework

Article L.1425-1 of the *Code Général des Collectivités Territoriales* authorises local authorities to establish and operate electronic communications infrastructure where private provision is insufficient. This provision, introduced in 2004, marked a decisive shift: it allowed departments, regions and inter-municipal bodies to become infrastructure owners in a sector previously reserved to national operators. The *Plan France Très Haut Débit*, launched in 2013, organised this dispersed authority into a national programme mobilising more than thirteen billion euros of public and private investment with the objective of universal fibre coverage.

The programme partitions the national territory into three zones. In densely populated areas, private operators deploy at their own expense and compete infrastructure-to-infrastructure. In areas covered by the private investment-intention procedure (AMII), designated operators committed to deploy without subsidy following a national call. The remainder – the *zone d'initiative publique*, comprising roughly 30,000 of France's 35,000 communes – is served by RIPs: publicly initiated networks financed by local authorities with national and European support, and almost always delegated to a private operator for construction and operation.

The delegating authority is typically a department, a region, or a *syndicat mixte ouvert* pooling several tiers of local government within a single structure. The heterogeneity here is substantial. Some *syndicats* were created exclusively to manage telecommunications infrastructure and possess a narrow, highly specialised mandate; others operate within broader organisations that simultaneously oversee energy distribution or environmental services. Some networks remain under direct departmental management. This institutional variation matters for the contractual choices that follow: the procurement literature has established that the organisational characteristics of public buyers explain a substantial share of the variation in procurement outcomes (Bandiera et al., 2009; Bosio et al., 2022), and that contract-management capacity varies widely across local governments and conditions which services they contract out and on what terms (Brown and Potoski, 2003). On the operator side the sector is concentrated: Orange Concessions, Altitude Infra, Axione and Altice/SFR account for the large majority of networks, and a given operator typically holds contracts of several different forms across different territories.

French delegation law offers a menu of contractual forms differing principally in who finances the asset and who bears the risk that revenues will not cover the outlay. Ordered by increasing transfer of investment risk to the delegatee, the forms observed in our sample are the following.

Under *régie directe*, the authority builds and operates the network with its own resources; no risk is transferred and the arrangement is not a delegation in the legal sense. Under *SPL/affermage*, operation is delegated to a *société publique locale*, a company wholly owned by the participating authorities; the arrangement is formally a delegation but the delegatee is publicly controlled, so risk transfer is nominal. Under *affermage*, the authority finances construction and a private operator is remunerated for operating the asset, bearing commercial but not investment risk. Under *affermo-concessive* delegation, a hybrid form that emerged specifically in the Plan THD context, the delegatee finances part of the deployment, typically the later or more costly phases. Under *concessive* delegation, the delegatee finances, builds and operates the network, recovering its investment from wholesale revenues over the contract term. Under a *public–private partnership*, the delegatee finances and builds the asset against a stream of availability payments from the authority, which retains demand risk while transferring construction and financing risk. Two further forms appear in the data: the CREM (*marché de conception-réalisation-exploitation-maintenance*) and the MPGP (*marché public global de performance*) are global public procurement contracts rather than delegations, in which the operator is paid by the authority rather than by wholesale revenues.

This ordering rests on a proposition that the theoretical literature treats as central: the horizon over which an operator is entitled to recover its outlay increases with the share of that outlay it has advanced. Engel et al. (2014) note that the concession term is determined essentially by the amortisation requirement, and Athias and Saussier (2010) show empirically that contractual duration and contractual rigidity are jointly chosen as a function of the investment the delegatee is required to make. Duration is therefore not an independent contractual dimension but a correlate of risk transfer, and its observed gradient across forms provides a check on whether the legal taxonomy tracks an economic one.

The ordering is nonetheless unlikely to be uniform. The theoretical distinction that Hart (2003) and Iossa and Martimort (2012) draw is between arrangements in which the party exerting effort holds a residual claim on the asset and arrangements in which it does not, which is closer to a binary partition than to a smooth gradient. The intermediate forms of French practice – affermo-concessive delegations in particular – were designed as financial compromises rather than as intermediate incentive structures, and there is no strong theoretical reason to expect them to sit halfway between affermage and concession in incentive terms. We therefore treat the binary partition as our primary specification and report the ordinal version alongside it.

3 Data

3.1 Sources

Deployment. Deployment data come from ARCEP's quarterly open-data release *Le marché du haut et très haut débit fixe (déploiements)*, specifically the commune-level files. Each release reports, for every French commune, the total number of premises, the number connectable to a fibre network, the deployment zone, and the infrastructure operator code. Operators transmit these data to the regulator under a reporting obligation established by regulatory decision, and the regulator aggregates them using INSEE geographic codes. Two features make the series particularly well suited to our purpose. It is uniform across networks by construction, since the reporting obligation and the aggregation procedure are identical for all operators, which eliminates the measurement heterogeneity that would arise from self-reported indicators. And it is quarterly, which allows the construction process to be observed rather than inferred from its endpoint.

We use all quarterly releases from 2018Q3 to 2026Q1. Two vintages are excluded on internal-consistency grounds. The 2019Q3 file is a byte-identical republication of 2019Q2, reporting the same number of communes, the same number of active networks and zero new activations, which is not possible over a six-month interval in a period of rapid deployment. The 2024Q3 file reports every public-initiative commune as having an active operator, a figure the following quarter reverses. The 2025Q2 release is absent from the portal. The estimation panel therefore covers 26 quarters.

The commune files are distributed as shapefiles; we extract the attribute tables only, since the geometry is not required. Column names are not stable across vintages – `locaux` becomes `Locaux`, `INSEE_ARR` becomes `INSEE_AR` – and we normalise them through an explicit crosswalk. Coverage is computed throughout as the ratio of connectable to total premises.

Contractual arrangements. Contractual information comes from a database of French RIP delegation contracts recording, for each network, the delegating authority, the type of contract, its signature date and its duration. This database identifies networks by the name of the delegating authority and of the special-purpose vehicle; ARCEP identifies them by a four-letter operator code assigned under Article L.33-1 of the postal and electronic communications code.

3.2 Panel construction and record linkage

Sample restrictions. We restrict attention to communes in the public-initiative zone, exclude overseas departments – where deployment began after 2024 in Guyane and Mayotte and follows a trajectory not comparable to the metropolitan rollout – and drop communes served by more than one infrastructure operator, for which attribution to a network would be ambiguous. The latter restriction is immaterial: 94% of public-initiative communes have a single operator, and the multi-operator cases are concentrated in border communes where a private network overlaps the public perimeter.

Coding of pre-deployment observations. Until 2020Q4, communes with no active network carry the operator code `ZMD`, a marker for *zone moins dense* rather than an operator identifier; from 2021Q1 the same communes carry a missing value instead. Both encode the same underlying situation – no network yet deployed – and both are recoded to a common inactive status, so that these communes are retained as the pre-deployment observations they represent. Treating either value as informative about operator identity would distort the panel: a naive construction that drops missing operator codes loses 42 percent of communes at the 2021Q1 boundary for reasons unrelated to deployment.

Monotonicity. Because a deployed network does not disappear, we impose monotonicity on the activity indicator and on the stock of connectable premises. This affects 1.8% of observations, in which the reported stock decreases between consecutive quarters as a result of perimeter revisions by the regulator. The adjustment affects a small fraction of observations and is symmetric across treatment groups.

Linkage. Linking the deployment data to contractual information requires connecting two administrative sources that identify networks by different objects: an infrastructure operator in one, a delegating authority in the other. Since names overlap only partially between the two, we rely on an authoritative crosswalk rather than on string comparison, and we validate every link against a dimension external to the matching itself, following the practice recommended in the record-linkage literature (Fellegi and Sunter, 1969; Abramitzky et al., 2021).

The procedure has three steps. First, operator codes are mapped to network names and project sponsors using the nomenclature that ARCEP publishes alongside the deployment files, which also flags whether each network is a RIP and whether it belongs to the Plan France Très Haut Débit; this allows the relevant population to be isolated without relying on the contractual database. Second, each candidate link is validated by a lexical overlap test between the project sponsor recorded by ARCEP and the delegating authority recorded in the contractual database, after removal of terms common to nearly all French local authorities – *syndicat*, *mixte*, *départemental*, *numérique* and the like – whose retention would generate spurious overlap. Third, every link is checked geographically: the department in which ARCEP observes the network's communes must appear among the departments recorded for the delegating authority. Because this geography is reconstructed from ARCEP's own department codes, it constitutes an external validation dimension rather than a product of the matching. Candidate links that fail either test are not resolved automatically; they are verified by hand against primary sources or discarded.

The resulting linkage achieves a geographic consistency rate of 94% among the networks it resolves. A second validation is provided by the contractual data themselves: as Section 2 anticipates, contract duration should increase with risk transfer if the ordering tracks an economic gradient, and in the linked sample it does, with a Spearman rank correlation of 0.45 ($p = 0.003$) between the risk-transfer rank and contract duration. Affermage contracts run for a median of 16.5 years against 25 for concessive delegations, consistent with the amortisation logic set out by Engel et al. (2014).

Resulting panel. The commune panel covers roughly 26,000 communes over 26 quarters. Aggregating to the network level yields a panel of 56 networks with known contractual form, of which 18 retain public investment responsibility (one *régie*, three SPL/affermage, fourteen affermage) and 38 transfer it (eleven affermo-concessive, twenty-six concessive, one PPP).

3.3 Variables and descriptive statistics

Deployment effort is defined as

$$e_{jt} = \frac{C_{jt} - C_{j,t-1}}{L_{j,t-1} - C_{j,t-1}},$$

where C denotes covered premises and L total premises: the share of the remaining stock covered within the quarter. This is the discrete-time analogue of a hazard rate, and it is the quantity a model of costly deployment effort would predict. It has the further advantage of being comparable across networks of very different sizes, which matters in a sample where perimeters range from a few dozen to several thousand communes.

We restrict the estimation sample to observations in which deployment is under way and unfinished, defined as a lagged coverage rate between 0.01 and 0.95. The lower bound removes observations in which effort is zero because deployment has not begun; the upper bound removes observations in which there is nothing left to build. Both restrictions are symmetric across treatment groups and neither depends on the treatment itself.

The treatment variable D_j equals one if the contract transfers investment responsibility to the delegatee (affermo-concessive, concessive, PPP) and zero otherwise (*régie*, SPL/affermage, affermage). We also report an ordinal version taking values zero to five along the ladder described in Section 2. Controls comprise network maturity (lagged coverage) in levels and squares, the log number of premises, and quarter fixed effects.

Table 1 reports descriptive statistics by contractual form. Three patterns are worth noting before turning to the regressions. Concessive delegations display the highest mean effort (0.184) and affermo-concessive delegations one of the lowest (0.140), which sits awkwardly with a smooth risk-transfer gradient and supports the binary partition discussed in Section 2. The single *régie* and the single PPP have extreme values in opposite directions, illustrating why forms represented by one network cannot support inference on their own. And mean maturity varies across forms – 0.714 for concessions against 0.605 for SPL/affermage – confirming that groups are observed at different points in their deployment cycle, so that conditioning on maturity is essential.

Table 1: Descriptive statistics by contractual form

Contractual form	Networks	Obs.	Mean effort	S.D.	Mean maturity
Direct public management	1	1362	0.105	0.210	0.660
SPL / affermage	3	8974	0.174	0.315	0.605
Affermage	14	23301	0.147	0.254	0.678
Affermo-concessive delegation	11	32126	0.140	0.251	0.631
Concessive delegation	26	66911	0.184	0.270	0.714
Public-private partnership	1	9580	0.063	0.155	0.666
Investment delegated	38				
Investment retained	18				

Notes. Commune × quarter panel, 2018Q3–2026Q1. Effort is the share of the remaining connectable premises covered within the quarter. The estimation sample is restricted to communes whose deployment is under way and unfinished (lagged coverage between 0.01 and 0.95). Source: ARCEP, fixed broadband deployment observatory.

4 Empirical strategy

Our baseline specification is estimated at the network-quarter level:

$$e_{jt} = \beta D_j + \gamma_1 m_{j,t-1} + \gamma_2 m_{j,t-1}^2 + \mu_t + \varepsilon_{jt}, \tag{1}$$

where e_{jt} is deployment effort as defined in Section 3, D_j the indicator for delegated investment, $m_{j,t-1}$ lagged coverage, and μ_t quarter fixed effects. Standard errors are clustered at the network level. Five features of the design deserve comment.

The outcome. Normalising the quarterly increment by the remaining stock is the modelling choice on which everything else rests. A network's feasible absolute increment is bounded above by what remains to be built, so a specification in levels or in first differences would attribute to contractual form what is in fact a difference in position along the deployment path. The normalised measure is scale-free and bounded in $[0, 1]$ by construction, and it corresponds to the conditional probability that a still-unconnected premise becomes connectable within the quarter – the quantity that a model of costly effort with a convex cost function delivers. An unnormalised increment would be dominated by network size and by position along the deployment path, which is precisely what the normalisation is designed to remove.

The maturity control. Even after normalisation, effort is not independent of maturity. The first premises to be connected in any perimeter are the cheapest – dense communes near existing backbone – and the last are the most expensive. We therefore condition on lagged coverage in levels and squares, allowing the relationship to be non-monotonic. Including maturity in lagged rather than contemporaneous form avoids the mechanical simultaneity that would arise from conditioning on a variable determined jointly with the outcome, and Section 5 reports a dynamic panel specification that instruments the lagged control by its own deeper lags to address the residual correlation with past shocks.

Fixed effects. Quarter fixed effects absorb the common component of deployment: the national supply chain for fibre installation, the availability of certified subcontractors, the successive lockdown periods, and the national subsidy disbursement calendar all affect every network in the same quarter. Network fixed effects are not included, since the treatment is constant within a network – no RIP in our sample changes contractual form during the observation window – and would be absorbed by them. For the same reason the correlated random effects approach of Mundlak (1978) and Wooldridge (2019) does not apply here: the within-network mean of a time-invariant regressor is the regressor itself. Identification comes from comparisons across networks, conditional on maturity, size and time.

Level of estimation and weighting. We estimate at the network level in the baseline. Commune-level estimation implies an implicit weighting scheme in which a network with three thousand communes contributes three thousand times more observations than one with a single commune; since the treatment varies only across networks, this weighting reflects no corresponding gain in information about β. Estimating at the network level gives each network equal weight. Because the two levels are not equivalent, we report the commune-level specification and an explicitly size-weighted network-level specification alongside the baseline, and discuss the differences between the three.

Inference. With a treatment assigned at the network level and 56 clusters, the cluster-robust variance estimator is downward-biased (Cameron and Miller, 2015). We therefore complement it with randomisation inference: the assignment of D is permuted across networks 2,000 times, equation (1) is re-estimated at each draw, and the observed coefficient is located in the resulting distribution. This yields an exact test under the sharp null of no effect for any network, valid irrespective of the number of clusters, and it is the inference we report as our preferred one. The two procedures need not agree, and where they diverge we treat the randomisation p-value as authoritative.

5 Results

5.1 Main estimates

Table 2 reports the baseline specifications.

Column (1) regresses effort on the treatment with quarter fixed effects alone. The coefficient is 0.039 and significant, with a randomisation p-value of 0.004. Column (2) introduces maturity in levels and squares, raising the coefficient to 0.046 and improving precision. The increase is consistent with the descriptive pattern noted in Section 3: networks transferring investment risk are on average further advanced in their deployment cycle, and since effort declines at high maturity, failing to condition on the remaining stock attenuates the estimate rather than inflating it.

The maturity terms take the expected shape. The linear term is positive (0.57) and the quadratic negative (-0.46), so effort rises with coverage at low maturity and falls at high levels, with an implied turning point around 60% coverage. This inverted-U is consistent with a

Table 2: Contractual risk transfer and deployment pace

	(1)	(2)	(3)	(4)
Investment delegated	0.0391***	0.0458***	0.0460***	
	(0.0118)	(0.0121)	(0.0124)	
Degree of risk transfer				0.0216***
				(0.0067)
Maturity		0.5682***	0.5955***	0.5943***
		(0.1347)	(0.1381)	(0.1377)
Maturity squared		−0.4609***	−0.4758***	−0.4891***
		(0.1150)	(0.1136)	(0.1192)
log(premises)			0.0061	0.0027
			(0.0101)	(0.0098)
Quarter fixed effects	Yes	Yes	Yes	Yes
Observations	1,290	1,290	1,290	1,290
Networks	56	56	56	56
Adjusted R^2	0.079	0.112	0.112	0.116
p-value (randomisation)	0.0040	0.0015	0.0025	0.0005

Notes. Network × quarter panel. The dependent variable is the share of the remaining connectable premises covered within the quarter. Columns (1) to (3) use the binary indicator for delegated investment; column (4) uses the ordinal risk-transfer rank. Standard errors clustered at the network level in parentheses. The randomisation p-value is based on 2,000 permutations of the treatment assignment across networks; it is valid irrespective of the number of clusters and constitutes our preferred inference. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

deployment process that accelerates once the backbone infrastructure is in place and the marginal commune requires only distribution work, then decelerates as the residual becomes progressively more expensive to reach – isolated premises, difficult terrain, unresolved rights of way.

Column (3) adds network size, measured by the log number of premises, and leaves the treatment coefficient unchanged at 0.046. Column (4) replaces the binary treatment with the ordinal risk-transfer rank, yielding 0.022 per step. Since the binary contrast spans roughly two steps of the ladder, the two specifications are quantitatively consistent, and we retain the binary version as our preferred specification.

5.2 Locating the contractual threshold

An average gradient of 0.022 per step is consistent with very different underlying patterns, and distinguishing between them matters both for interpretation and for the design of successor contracts. Table 3 therefore reports a categorical specification taking *affermage* – the modal form among networks retaining investment responsibility – as the omitted category.

The pattern is not graduated. Affermo-concessive delegations, which occupy the middle of the contractual ladder, are statistically indistinguishable from affermage: the coefficient is 0.021 with a p-value of 0.227. Concessive delegations, by contrast, differ sharply from affermage (0.065, $p < 0.001$) and also from affermo-concessive delegations ($p = 0.005$). The discontinuity therefore

Table 3: Categorical specification: where does the incentive threshold lie?

	(1) Categorical	(2) Concession vs. rest
Direct public management (rank 0)	-0.0649^{***}	
	(0.0103)	
SPL / affermage (rank 1)	0.0345	
	(0.0250)	
Affermo-concessive delegation (rank 3)	0.0206	
	(0.0169)	
Concessive delegation (rank 4)	0.0646^{***}	
	(0.0124)	
Public–private partnership (rank 5)	-0.0499^{***}	
	(0.0106)	
Concessive delegation		0.0561^{***}
		(0.0111)
Maturity	0.7010^{***}	0.6850^{***}
	(0.1123)	(0.1084)
Maturity squared	-0.5789^{***}	-0.5649^{***}
	(0.0950)	(0.0987)
log(premises)	0.0044	
	(0.0100)	
Tests of contractual thresholds (column 1)		
H_0: rank 3 = rank 2 (affermage)	$p = 0.227$	
H_0: rank 4 = rank 2 (affermage)	$p < 0.001$	
H_0: rank 3 = rank 4	$p = 0.005$	
Quarter fixed effects	Yes	Yes
Observations	1,290	1,182
Networks	56	51
Adjusted R^2	0.171	0.142

Notes. Network × quarter panel. The dependent variable is the share of the remaining connectable premises covered within the quarter. In column (1) the omitted category is *affermage* (rank 2), the modal form among networks retaining investment responsibility. Column (2) restricts the sample to ranks 2 to 4 and compares concessive delegations to affermage and affermo-concessive delegations pooled. Standard errors clustered at the network level in parentheses. Ranks 0 and 5 comprise a single network each; their coefficients are reported for completeness but are estimated from one cluster and should not be interpreted. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

falls between partial and full transfer of investment responsibility rather than between retention and transfer. Restricting the sample to the three central forms and comparing concessions to affermage and affermo-concessive delegations pooled yields a differential of 0.056 (column 2), larger than our baseline estimate.

Two consequences follow for the reading of Table 2. Pooling affermo-concessive delegations with concessions attenuates the baseline coefficient, since eleven of the thirty-eight treated networks behave like the control group; the 0.046 we report is therefore conservative relative to the concession-specific differential. And the significant ordinal coefficient of column (4) should not be read as evidence of a smooth progression across ranks: it reflects a single discontinuity rather than an even gradient. We retain the binary treatment as the primary specification nonetheless, since it corresponds to the partition stated ex ante in Section 2, and we return to the interpretation of this threshold in Section 6.

Coefficients for ranks 0 and 5 are reported for completeness but are estimated from a single network each and should not be interpreted; with one cluster per category, the clustered standard error is not informative.

Magnitude. The mean quarterly effort in the estimation sample is 0.15. A coefficient of 0.046 therefore corresponds to a pace differential of roughly thirty percent. Put differently, a network covering ten percent of its residual stock per quarter under an affermage arrangement would cover about thirteen percent under a concession, other things equal. Compounded over the five to eight years of a typical deployment phase, differentials of this order translate into completion dates separated by several quarters – which, given the returns to broadband access documented in Section 2, is not a negligible welfare quantity.

Inference. The randomisation p-values reported in the final row range from 0.0005 to 0.004 across specifications, and agree here with the cluster-robust p-values.

5.3 Robustness

Table 4 collects the robustness checks.

Table 4: Robustness

Specification	Coefficient	Std. error	Observations	Networks
Baseline (network level)	0.0458***	(0.0121)	1290	56
Commune level, department FE	0.0757***	(0.0156)	142254	56
Weighted by number of communes	0.0039	(0.0178)	1290	56
Excluding direct management and PPP	0.0455***	(0.0117)	1238	54
Balanced sub-panel (OLS)	0.0413**	(0.0161)	754	29
System GMM (mean of valid specifications)	0.0459	(0.0367)	1298	29

Notes. Coefficient on the indicator for delegated investment. All specifications include quarter fixed effects and control for maturity in levels and squares. Standard errors clustered at the network level. The balanced sub-panel retains the 29 networks observed in all 26 quarters. See Table 5 for the full set of GMM specifications. $^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Level of estimation. Estimating at the commune level with department fixed effects yields a larger coefficient, 0.076. Two factors drive the difference. The first is weighting: at the commune level, large networks dominate the estimation. The second is that 63 of the 81 departments in the sample contain a single network, so department fixed effects absorb a substantial share of the treatment variation, leaving identification to the minority of departments hosting several networks. We regard the network-level estimate as the more interpretable of the two and note that both point in the same direction, though the cluster-robust and randomisation p-values diverge for this specification (< 0.0001 against 0.066), which is a further reason to prefer the network-level baseline.

Weighting. Weighting network-quarter observations by the number of communes served reduces the coefficient to 0.004. The association is therefore carried by the smaller and medium-sized networks in the sample, while the large regional networks – Losange in the Grand Est, Nouvelle-Aquitaine THD, THD Bretagne, which together serve several thousand communes – do not display it as clearly. This is consistent with the theoretical mechanism operating more strongly where the delegatee's investment is concentrated in a single perimeter and the delegating authority's monitoring is correspondingly more direct. Large regional networks are typically governed by consortium arrangements involving a region and several departments, a multi-level structure that the inter-municipal cooperation literature associates with higher coordination costs (Guelmamen, 2025; Guelmamen et al., 2025) and that plausibly dilutes the incentive structure a given contractual form is meant to create.

Marginal contractual forms. Excluding the single *régie* and the two PPPs – forms represented by one or two networks and therefore incapable of supporting inference on their own – leaves the coefficient essentially unchanged at 0.046. The result is driven by the contrast between affermage and concessive delegations, which together account for 40 of the 56 networks, rather than by the extremes of the ladder.

Balanced sub-panel. Restricting to the 29 networks observed in every quarter reduces the coefficient slightly, to 0.041, and it remains significant at the 5% level.

Dynamic specification. The maturity control is mechanically correlated with lagged deployment shocks, which biases its coefficient and, through it, the treatment coefficient. We address this with a system GMM estimator (Blundell and Bond, 1998) instrumenting lagged maturity by its own deeper lags. Because the specification space in dynamic panel estimation is notoriously permissive, we defined the full grid of eighteen combinations of lag depth and instrument collapsing, and report all of them in Table 5 rather than selecting among them.

The pattern is instructive. The nine specifications without instrument collapsing all yield coefficients between 0.075 and 0.114, significant at conventional levels – but all carry Sargan statistics between 0.98 and 1.00. This is the signature of instrument proliferation, which deprives the overi-

Table 5: Full set of System GMM specifications

Lags	Collapsed	Coefficient	p	Sargan	AR(2)	Valid
2:2	Yes	0.0395	0.477	0.010	0.266	
2:2	No	0.1135	0.008	0.987	0.258	
2:3	Yes	0.0017	0.972	0.008	0.270	
2:3	No	0.1017	0.017	1.000	0.260	
2:4	Yes	-0.0074	0.879	0.009	0.270	
2:4	No	0.0908	0.026	1.000	0.262	
3:3	Yes	0.0177	0.815	0.003	0.232	
3:3	No	0.0747	0.032	0.984	0.228	
3:4	Yes	-0.0244	0.683	0.004	0.234	
3:4	No	0.0852	0.024	1.000	0.227	
3:5	Yes	-0.0247	0.674	0.005	0.234	
3:5	No	0.0838	0.033	1.000	0.228	
4:4	Yes	0.0473	0.212	0.215	0.242	✓
4:4	No	0.0783	0.031	0.990	0.240	
4:5	Yes	0.0467	0.209	0.263	0.242	✓
4:5	No	0.0828	0.023	1.000	0.239	
4:6	Yes	0.0436	0.216	0.182	0.242	✓
4:6	No	0.0810	0.041	1.000	0.239	

Notes. Eighteen specifications, defined ex ante by crossing the depth of the lags used as instruments with the collapsing option. A specification is deemed valid if it jointly satisfies three criteria set before estimation: absence of second-order autocorrelation, a Sargan statistic between 0.10 and 0.80, and an instrument count below half the number of units. The upper bound on the Sargan statistic discards specifications in which instrument proliferation deprives the test of power: the nine uncollapsed specifications display p-values between 0.98 and 1.00 and are not interpretable. Estimated on the balanced sub-panel of 29 networks.

dentification test of power rather than confirming instrument validity (Roodman, 2009). The nine collapsed specifications yield smaller coefficients and, in six cases, Sargan statistics that reject validity.

Of the eighteen specifications, three satisfy the validity criteria set in advance; the remaining fifteen are excluded on instrumentation grounds rather than on the basis of their estimates, nine because instrument proliferation renders the overidentification test uninformative and six because it rejects. Across the three valid specifications the coefficient is 0.047, 0.047 and 0.044 – a dispersion of 0.002 – and coincides with the least-squares estimate obtained on the same sub-sample. The dynamic estimator therefore confirms the magnitude while, with five to seven moment conditions on twenty-nine networks, lacking the precision to sharpen it.

5.4 Selection on unobservables

The contractual form is chosen by the delegating authority rather than assigned, and the procurement literature reviewed in Section 2 identifies administrative capacity as a determinant of both contractual choice and implementation performance. We address this by testing directly for selection using an exclusion restriction and by estimating a control function. Table 6 reports our results.

Table 6: Selection on Unobservables

First stage (probit, network level)	
Specialised delegating authority	-1.546
	(0.525)
z-statistic	-2.95
Networks	52
Falsification test: instrument in the outcome equation	
Investment retained	0.019
	($p = 0.140$)
Investment delegated	0.004
	($p = 0.795$)
Endogeneity test: inverse Mills ratio	
Coefficient	0.0028
Bootstrapped s.e.	(0.0195)
95% confidence interval	$[-0.0357,\ 0.0408]$
p-value	0.831

Notes. The instrument is an indicator for a delegating authority created exclusively to manage digital infrastructure. The endogeneity test adds the inverse Mills ratio constructed from the first-stage equation to the outcome equation reported in Table 2, column (2). Under the null hypothesis of no endogenous selection, the coefficient on the inverse Mills ratio equals zero. Standard errors are obtained by bootstrap resampling of networks stratified by treatment status, with both stages re-estimated at each of 2,000 bootstrap replications; all replications converged. The estimation sample comprises the 52 networks for which the instrument is observed. A selection-corrected treatment coefficient is not reported because, with a single instrument, the inverse Mills ratio is nearly collinear with the treatment indicator.

An exclusion restriction. We exploit the specialisation of the delegating authority. Roughly two thirds of the networks in our sample are delegated by a *syndicat mixte* created exclusively to manage digital infrastructure. The remainder are delegated by departmental councils or by general-purpose structures for which fibre is one competence among many. A dedicated *syndicat* holds earmarked resources and its own borrowing capacity, and can therefore carry the investment itself, whereas a departmental council arbitrates between digital infrastructure and its other statutory obligations and is correspondingly more likely to transfer the burden.

The instrument is strong in the first stage. In a probit of the treatment on specialisation, mean maturity and network size, the coefficient on specialisation is -1.55 with a z-statistic of -2.95 ($p = 0.003$): specialised authorities are substantially less likely to delegate investment. It also passes falsification. Regressing effort on the instrument separately within each regime returns coefficients of 0.019 ($p = 0.140$) among networks retaining investment responsibility and 0.004 ($p = 0.795$) among those delegating it. We note that the first of these tests has limited power, since only one of the seventeen networks in that regime is delegated by a non-specialised authority.

Testing for selection. Table 6 reports the outcome of a two-step control function (Heckman, 1979; Wooldridge, 2010), in which the inverse Mills ratio constructed from the first stage is added to the outcome equation. Its coefficient constitutes a direct test of endogeneity: under the null that contractual form is uncorrelated with the performance shock conditional on the covariates, it is zero. We estimate it at 0.003 with a bootstrapped standard error of 0.019, a 95% confidence interval of $[-0.036; 0.041]$ and $p = 0.831$. Standard errors are obtained by resampling networks stratified on treatment status, both stages being re-estimated at each of 2,000 draws so that the sampling error of the first stage propagates into the second. All draws converge, and the bootstrapped standard error is within eight percent of its analytical counterpart, which indicates that first-stage estimation contributes little additional uncertainty. What the exercise establishes is that no confounding is detected along the dimension that specialisation of the delegating authority allows us to probe.

6 Discussion

6.1 From quarterly pace to deployment schedules

The coefficient of 0.046 is a quarterly hazard differential, and its economic content is clearer once translated into the completion horizon it implies. Consider two networks with identical perimeters, both beginning deployment from a standing start, one covering ten percent of its residual stock per quarter and the other thirteen. Under a constant hazard, the first reaches ninety percent coverage after twenty-two quarters and the second after seventeen: a difference of five quarters, or fifteen months, in the date at which the territory is substantially served. Allowing the hazard to follow the inverted-U profile we estimate rather than remaining constant lengthens both horizons and compresses the gap somewhat, but the order of magnitude is unchanged.

Fifteen months is a material quantity in this context. The returns to broadband access documented in Section 2 accrue from the date of connection onwards, and the literature finds them to be persistent rather than transitory. Two territories that both reach full coverage by the end of the Plan THD therefore do not capture the same cumulative benefit if one is served a year earlier than the other, and the difference is permanent. This is the sense in which deployment pace, rather than terminal coverage, is the outcome on which the contractual architecture of a subsidised rollout should be assessed.

The magnitude also bears comparison with the effects attributed to the subsidy itself. Bourreau et al. (2023) estimate that Plan THD support brought fibre to territories that private investment would not have covered in the large majority of cases – an extensive-margin effect on whether deployment occurs at all. Our estimate concerns the intensive margin, holding the decision to deploy fixed, and is large enough that the two are of comparable policy relevance. A programme that reaches the right territories but through contractual arrangements that slow construction achieves less than its budget would suggest.

6.2 Where the association operates: perimeter concentration and governance layering

The most informative feature of our results is not the average coefficient but its distribution across network sizes. The association is carried by the small and medium-sized networks in the sample and attenuates among the large regional consortia – Losange, Nouvelle-Aquitaine THD, THD Bretagne – which together account for a substantial share of the communes covered.

Two features of large RIPs plausibly account for this. The first is the dilution of the delegatee's stake. In a departmental perimeter, a concessionaire's entire investment and its entire revenue stream depend on one network reaching completion; in a regional consortium spanning seven or eight departments, the same operator holds a portfolio in which delay in one area can be offset elsewhere, and the sharpness of the incentive is correspondingly reduced. The second is the layering of the delegating authority. Large RIPs are typically governed by a region acting jointly with several departments, sometimes through a dedicated *syndicat mixte* whose decisions require assent from multiple tiers. The literature on inter-municipal cooperation has documented that such arrangements generate coordination costs capable of dominating the scale economies they are meant to realise, particularly during transitional periods (Guelmamen, 2025; Guelmamen et al., 2025), and the same logic applies to the monitoring of a delegation contract: an authority that must convene several assemblies to press its delegatee presses it less effectively than one that need not.

Read this way, the size heterogeneity is not a weakness of the result but a statement about its mechanism. Contractual form sets an incentive structure; whether that structure transmits into construction effort depends on the institutional environment in which the contract is administered. This is precisely the complementarity between contractual and institutional arrangements that the transaction-cost tradition anticipates (Williamson, 1985), and it suggests that the contractual choice and the governance structure of the delegating authority should be analysed jointly rather than as separate margins.

6.3 Why partial risk transfer does not change behaviour

The categorical estimates of Section 5 locate the discontinuity between partial and full transfer of investment responsibility: affermo-concessive delegations behave like affermage, concessions differ from both. This is what the property-rights framework predicts, and the mechanism is worth stating precisely.

The distinction Hart (2003) and Iossa and Martimort (2012) draw is between arrangements in which the party exerting effort holds a residual claim on the asset and arrangements in which it does not. An operator financing part of a deployment does not thereby acquire a residual claim: it acquires a receivable against a counterparty that retains ownership of the residual. Partial financing therefore alters the size of the delegatee's exposure without altering the nature of its entitlement, and on this reading should not be expected to change behaviour. Our estimates are consistent with that prediction and inconsistent with a model in which incentives scale smoothly with the share of the investment borne.

The finding also bears on how the French intermediate forms should be understood. Affermo-concessive delegations emerged in the Plan THD context as a response to a financing constraint on the public side: authorities unable to fund the entire deployment sought operators willing to advance part of it, typically the later and more costly phases. They were not designed as intermediate incentive structures, and the evidence suggests they do not function as such. The design of these contracts was a budgetary compromise, and it appears to have been treated as one by the operators who signed them.

The practical implication for successor contracts is that the relevant decision is discrete rather than calibrated. An authority unable to transfer the full investment burden gains little in incentive terms from transferring part of it, and would do better to concentrate its financing capacity on a smaller perimeter delegated in full than to spread it across a larger one delegated in part. This runs against the logic that produced the affermo-concessive form in the first place, which sought to extend coverage by sharing the financing burden as widely as possible, and it suggests that the trade-off between perimeter size and contractual completeness deserves explicit attention when delegations are renewed.

7 Conclusion

This paper has examined whether the allocation of investment risk in delegation contracts is associated with the speed at which French public initiative networks deploy fibre. Using a newly constructed panel of 56 networks observed quarterly over 2018–2026, built from the regulator's commune-level open data and linked to contractual information through a verified matching procedure, we find that networks whose contracts place investment responsibility on the delegatee cover their residual territory about 4.6 percentage points faster per quarter – roughly a thirty percent differential, and some fifteen months in the date at which a territory is substantially served.

The contribution is threefold. Empirically, the paper brings evidence to bear on the margin that the incomplete contracts framework actually identifies. The prediction that risk allocation shapes non-contractible investment effort has largely been tested through its downstream correlates – prices, ex post service quality – because construction is rarely observed at sufficient frequency to be studied directly. The quarterly structure of the French regulatory data allows the construction phase itself to be examined, and the pattern we document is the one the theory anticipates: the party that finances the asset builds it faster.

Methodologically, the paper proposes a measure of deployment effort – the share of the remaining stock covered within the period – that separates construction intensity from network maturity, and which we hope will be useful beyond the present application. Any evaluation comparing infrastructure projects at different stages of completion faces the same mechanical problem, and normalising by the residual stock addresses it without requiring the projects to be observed from a common starting date.

For policy, the results suggest that the organisational design of a subsidy programme deserves attention alongside its amount and its targeting. Among territories that Plan THD support reached, the contractual architecture through which the money was spent is associated with differences in deployment pace of a magnitude comparable to the effects usually attributed to the subsidy itself, and the association is strongest where the perimeter is concentrated and the delegating authority's oversight most direct. Evaluations that treat subsidy recipients as homogeneous may therefore attribute to the transfer what belongs in part to the contract. As the first Plan THD delegations approach renewal in the 2030–2040 horizon, the terms on which they are re-tendered become a policy question in their own right, and our results indicate that the relevant choice is a discrete one between retaining and transferring investment responsibility rather than a calibration among intermediate forms.

The natural next step is to bring data on the procurement process itself to bear on the question – the number and identity of bidders, the technical assessments preceding the choice of form, the reasons recorded in the deliberations of the delegating authority. Such data would allow the contractual choice to be modelled rather than conditioned on. The renewal wave now approaching offers a further opportunity, since networks reaching the end of their initial contract will re-tender under a regulatory environment common to all of them, generating variation in contractual form that is plausibly less confounded with the characteristics of the authority than the original choice was.

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