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Social Interactions, Network Fluidity and Network Effects

Catherine Tucker
MIT Sloan School of Management

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Abstract

This paper asks how much the strength of network effects depends on the stability and structure of the underlying social network. I answer this using extensive micro-data on all potential adopters of a firm's internal video-messaging system and their subsequent video-messaging. This firm's New York office had to be relocated due to the terrorist attacks of 2001 which lead to a physical re-organization of teams in that city but not in other comparable cities. I study the consequences of this disruption for adoption of video-messaging and the size of network effects. I find evidence that generally network effects are based on direct social interactions. Potential adopters react to adoption only by people they wish to communicate with: They are not affected by adoption by other people. However, when there is a disruption to the social network and communication patterns become less predictable, users become more responsive to adoption by a broader group of users.

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1 Introduction

The main aim of many new technologies is to ease the flow of communication and ideas. However, network technologies like instant messaging or video-conferencing cannot ease communication if no-one else adopts them. Concern that such coordination difficulties mean that useful technologies do not get adopted has prompted a growing body of empirical research documenting network effects.

Researchers have presumed that the social interactions underlying network effects are stable and certain. Our paper, by contrast, examines how network effects change with the certainty of network interactions.

Communications networks, like the social interactions they reflect, can be stable or fluid. It is not clear how the fluidity of the underlying communications network affects adoption. On the one hand, if people communicate reliably with the same handful of people, then this certainty may lead them to adopt more quickly if one of their contacts adopts. On the other hand, if people talk with a large and ever-changing number of people, this may lead them to be more likely to place an option value on the ability to talk with this large set of people. This question matters because of the growing recognition that social networks are not stable but instead constantly evolve (Kossinets and Watts 2006). Firms planning to use social networks to spread network technologies need to know how to adjust their strategy to account for underlying social network fluidity.

This paper uses an extensive dataset on the characteristics of potential adopters of an internal video-messaging system within an investment bank and their subsequent calls to analyze whether network fluidity affects the size of network effects, and therefore the pace of adoption. Deriving this causal link is hard, because there are many unobserved factors that can affect both network fluidity and adoption, confounding analysis. For example, unstable communications among employees could reflect poor management, and that poor management could also explain why the workers do not use a network technology. This research studies an exogenous shock to the stability of the underlying communications network (the terrorist attacks of 2001) that forced physical relocation and reorganization of teams in New York, but not in other comparable cities.

Even with this exogenous shock to the communications network, it would still be problematic to conclude that it is network effects that prompt two employees who talk together to adopt at the same time, rather than because they share similar external impulses to adopt. The video-messaging system's dual use for TV-watching allows an unusually powerful identification strategy that permits the identification of actual, causal network effects. Some employees adopted the technology to watch one-off TV events such as the 2002 Soccer World Cup. This identification strategy has also been exploited in (Tucker 2008) to explore heterogeneity in the size of network effects.

The empirical results suggest that potential adopters react to changes in the installed base only if they wish to communicate with the person adopting. They do not react particularly to the adoption of users with whom they do not communicate but who share characteristics with those that they do communicate with. On the face of it, this suggests that the coordination issues inherent in network effects are less broad than previously supposed. However, closer examination shows that the the 'localness' of network effects depends on the certainty surrounding future communications. This sets up an unexpected result. As the network in which potential adopters communicate becomes less stable, direct network effects decrease in importance. However, broader adoption by employees with whom adopters do not actually communicate starts to have a significant impact.

One plausible explanation of why the broader network matters more during periods of uncertainty is this. In times of uncertainty about how their role in the firm will develop, employees place value on the option of having a broader selection of other people in the network to talk to. Consequently, a potential adopter places an option value on being able hypothetically to talk to someone else even if they never ultimately communicate. The broader network therefore becomes more important when there is less certainty about whom an adopter will talk to. Traditional theories of option-value and strategic investment under uncertainty ((Schwartz and Zozaya-Gorostiza 2003), (Pindyck 1993)) emphasize that periods of uncertainty and instability increase the option-value of waiting and consequently deter aggregate investment in technologies. By contrast, I find that here instability acts to increase the impact of network effects on adoption, by making an adoption decision more responsive to a broader set of adoption decisions.

The paper is organized as follows. Section 2 describes the data and the firm setting. Section 3 describes the conceptual model of adoption behavior that underlies the model. Section 4 describes initial differences in differences results for before and after the terrorist attacks. Section 5 describes the results when I instrument the installed base. Section 6 discusses the broader implications of the research.

2 Technology and Data

2.1 Technology

The network benefit of this video-messaging unit relative to the telephone is that the employee can see a television-quality image of the person they are talking to. Theoretically, this could improve the effectiveness of internal firm communication, by adding visual communication cues ((Marlow 1992)). Older video-messaging systems failed because they were based on rarely-used video-conferencing rooms. This research studies a new video-messaging technology attached to an employee's workstation. The end-point technology consists of three elements: Video-messaging software; a media compressor; and a camera fixed on top of the computer's monitor. The technology also had a stand-alone use of transforming the employee's desktop computer into a television.

The video-messaging technology can only be used for internal communication within the firm. This makes it attractive for empirical studies because there are comprehensive data on all potential adopters.

2.2 Firm Setting

After this bank chose this technological standard to conduct internal video-messaging, they invested in an extensive network architecture. To encourage employee buy-in, the bank decided to decentralize installation decisions to each employee. The bank publicized the technology to employees and each employee decided whether and when to order a video-messaging unit from an external sales representative. The bank made employees eligible to adopt the technology if they held a position of Associate or higher (85 percent of full-time employees). The equipment's supplier had excess capacity, so capacity constraints did not affect the timing of individual employee installation decisions.

This decentralization focuses analysis on the private benefits to installation for employees, as opposed to firm-level productivity benefits. Firms find it hard to monitor and reward improved communication (Lazear 2000). Information asymmetries therefore mean that employees' installation benefits may be small relative to firm-level benefits from the video-messaging system. I cannot, unfortunately, quantify these firm-level productivity benefits.

Though the firm bears the monetary costs of installing video-messaging, the employee bears non-monetary costs. These non-monetary costs are represented by the time the employee spends away from their regular work while the desktop video-messaging unit is installed and they learn how to use it. Since these time costs are sunk, I treat installation of video-messaging as irreversible.

2.3 Physical Dislocation

A crucial feature of this firm was that, like many firms based in New York in 2001, they had to physically relocate their offices as a result of the terrorist attacks of September 2001 and the closing down of many financial offices in New York surrounding the World Trade Center due to the potential for structural damage. This shift in physical location was exacerbated by financial uncertainty engendered by the attack and led to a simultaneous change in roles and workgroup dynamics. Confidential interviews with employees confirmed that this uncertainty about both the economy and their future roles as well as the reorganization that resulted from the shift in physical location persisted until the Summer of 2002. This is a similar though slightly longer period than the period of uncertainty documented by (Bloom 2007), but that is plausibly a result of the fact that this firm was directly affected by the attacks while Bloom studies the aggregate response of industry. I therefore use two periods of data (January 2001-August 2001) and (October 2001-August 2002) to look at employee-level installation decisions before and after the physical shock to the bank's Manhattan location.

I use this exogenous shock to the stability and predictability of the communications network to evaluate how the stability of the underlying communication network affects network effects.

2.4 Data

I have complete (anonymized) personnel records for each employee in the investment bank in March 2004. Employees were associated with two main products: Equities and derivatives. There were four broad different functions: Administration, Research, Trading and Sales. There is also information on the precise city location of each employee. I classify these locations into four broad regions: Britain, North America, Europe and Asia/Sub-Equatorial. There were four formal rungs in the hierarchy for employees at the firm. I combine the bottom three rungs "Associate","Vice-President" and “Director” into the “Workers” category and translate “Managing Director” into the “Manager” category. 10.8% of employees were Managers.

A call database recorded the 2.4 million video-messaging calls made within the bank from January 2001 to August 2004. For two-way video-messaging calls, the database records the caller and callee, when the call was made and how long it lasted. For one-way TV calls, the database records who made the call, to which TV channel, when and for how long.

Employees made 1,768,348 two-way user-to-user video-messaging calls. The dataset includes only the 1,052,110 video-messaging calls where the callee accepted the call. Each accepted call lasted on average 5 minutes and 46 seconds. Calls could be made to more than one employee at a time. Multi-party calls (less than 5 percent of calls) were simplified into their pairwise equivalents: A three-way call is treated as three calls between each two of the participants. I use these data both to evaluate when an employee makes a first call and ‘adopts’ the technology and also to reconstruct the communications network within the firm. [1]. A limitation of using these data to reconstruct the communications network is that the call database provides data on whom the 1,294 adopters video-messaged with, but not on whom the 824 non-adopters would video-message with if they adopted. Therefore I exclude non-adopters from my regressions. Consequently my empirical results are representative only for adopters. They should be interpreted as reflecting how network effects affect the adoption timing of adopters, as opposed to the decision to adopt for all employees.[2] Employees also made 741,926 successful one-way caller-to-media-device calls, mostly so they could watch TV.

Using this call data in conjunction with the personal data overcomes (Rohlfs 1974)’s warning that the studying the topology of network effects would be impossible because

[1]It would be ideal to be able to compare the communications networks suggested by this data to alternative communications technologies such as e-mail or telephones, but for legal reasons such data are unobtainable

[2](Tucker 2008) compares predictions from studying adoption only by adopters and also including non-adopters and a predicted communications network. The results from the two exercises were similar.

"in any practical problem we could never hope to have a complete empirical list of principal contacts."[3]

3 Modeling Technology Adoption

This is a latent variable setting where we observe only installation decisions $inst_{it}$, not installation benefits $inst^*_{it}$, for employee i at time t.

$$inst_{i,t} = \begin{cases} 1 & \text{if } inst^*_{it} > 0 \\ 0 & \text{if } inst^*_{it} \leq 0 \end{cases}$$

where

$$inst^*_{it} = \Delta_1 InstalledContacts_{it} + \Delta_2 InstalledPotentials_{it} + \lambda TV_{r,t} + \beta X_i + \omega T_t + \epsilon_{it} \quad (1)$$

Each month, an employee chooses whether or not to install the technology. This decision maximizes their utility given the set of users in the installed base, the stand-alone TV benefit, their own idiosyncratic net benefits captured by controls X_i, a series of controls for month-by-month changes in their underlying hazard rate T_t, and unobserved heterogeneity (ϵ_{it}). I assume that each employee i takes the adoption choices of other employees as given, but I do not formally model the equilibrium selection mechanism.

It is not easy for the employees to divest themselves of the technology after it is attached to their desk-tops. Therefore I treat adoption decisions as a one-time adoption decision, and include only observations where the employee adopts video-messaging in that month or has not yet adopted. The dependent variable $inst_{it}$ is whether in that

[3] Rohlfs based his pioneering work on the failure of an early video-messaging technology, the AT&T Picturephone, to achieve critical mass.

month the employee used video-messaging technology for the first time. An observation is an employee who did not adopt the technology in the previous months.

3.1 Network Effects from Contacts and Potential Contacts

Employee's adoption decisions may be affected by adoption by those they ultimately talk to. I label the employees in the firm they ultimately talk to as 'contacts'. The installed base of contact adoption decisions is captured by $InstalledContacts_{it}$. This installed base includes all contacts' installation decisions up to and including month t.

Employees could also be affected by adoption of employees who there is a chance they will communicate with (even if they ultimately do not). Theoretically, this broader sense of the installed base could be measured by the aggregate level of adoption in the firm. However, given my use of monthly time dummies to control for firm-wide shocks, I would not be able to identify the separate effect of the installed base. Instead I seek a measure of the installed base that reflects that there will be variation in the likely list of potential contacts for each employee based on their position in the firm. Therefore I define for each employee a list of potential contacts based on whether they share the same product area, function and market specialization. I define $InstalledPotentials_{it}$ as the count of these potential contacts who have adopted the technology, but with whom the employee did not ultimately communicate.

I chose these particular (reasonably broad) characteristics to define potential contacts because according to interviews with investment bankers it is very unusual for employees to switch from investment banking to equity sales or from researching Asian equities to researching European derivatives, since an employee has built up employee-specific expertise and capital in that area. Therefore, even in view of the dislocation and uncertainty caused by the terrorist attacks, it seems likely that an employee would still perceive their product area as being the set of people in the firm they were most likely to communicate with. This is further supported by empirical evidence documented by (Tucker 2008) and (Ryan and Tucker 2008) that employees with similar characteristics are more likely to communicate throughout the time-span covered by the data.

3.2 Controls

It is likely that the net costs of adopting the technology vary considerably across employees. I capture this heterogeneity using a series of controls for each employee X_i. For example, it may be easier for employees in more flexible areas, such as research, to schedule time for their computers to be down, than for employees who work in fast-paced areas such as derivatives trading. Therefore, I include a series of controls for each of the different functions and product groups. I also include a series of dummies for an employee's title to control for differences across in adoption patterns across the hierarchy. Similarly, there may be cross-national differences in technological competence and expected learning costs. To capture this I also include controls for each region.

It is also likely that the net costs of adopting vary across time. Therefore, I include a series of dummies for each month that employees could potentially adopt the technology, T_t. Since these time dummies will also pick up selection and the changing baseline hazard rate, they cannot be interpreted, and instead serve to capture in a flexible manner the varying underlying hazard rate ((Jenkins 1995)).

This technology also had specific benefits that were independent of any network usage. In particular, employees enjoyed being able to watch television on their desktop computer. There were two types of television employees could watch: News TV programming on CNN and CNBC, which covers financial news; and local TV programming (often non-news) broadcast by country-specific channels. While there was little variation across regions in the percentage of adopters watching news programming (implying that this is largely captured by the month dummies), there was large variation in employee interest in local TV programming across regions. For example, employees in UK watched the 2002 Soccer World Cup, while employees in the US did not. Empirically, these local broadcast events were correlated with adoption in the month prior to the month they occur. This suggests that employees adopted the technology in advance to ensure they could watch predictable "must-see" television. I capture these regional shocks to the technology's stand-alone benefit by the variable TV_{rt} which contains the percentage of previous adopters watching "Local TV" in region r in the month following time t.

Table 1 gives summary statistics for the dependent and independent variables described in this section.

Table 1: Description of all variables used in regressions

Variable	**Description**	**Mean**	**Std. Dev.**
	Dependent Variables		
$FirstAdoption_{it}$	Indicator Variable for first month an employee makes outward video-messaging call	0.08	0.274
$FirstAdoption_{it}$	Indicator Variable for first month a worker makes outward video-messaging call	0.07	0.258
$FirstAdoption_{it}$	Indicator Variable for first month a manager makes outward video-messaging call	0.17	0.372
	RHS Variables		
Variable	**Description**	**Mean**	**Std. Dev.**
$InstalledContacts_{it}$	Sum of cumulative adoption by employee i's contacts by month t	8.292	9.581
$InstalledPotentialConctacts_{it}$	Sum of cumulative adoption by employee i's potential contacts who work in the same function and global market area but whom the employee did not ultimately communicate with by month t	88.606	44.10
TV_{rt}	Proportion of adopters in the employee's region r who have adopted prior to month t who watch local television channels in month $t+1$	0.336	0.359
Controls for Regions	Dummies for Europe, Asia, US and UK		
Controls for Month	Dummies for each month from February 2001 to August 2002		
Controls for Function	Dummies for working in administration, research, trading and sales		
Total Observations	12723		

4 Differences in Differences

There is a potential for the underlying process represented by equation (1) to be affected by the stability of the network and expectations about future communications. Often in firms, communication patterns are relatively stable and predictable. However, in the time-span covered by my data there was a shock to the New York office of the firm due to the terrorist attacks of 2001 which forced them to relocate their offices. This physical dislocation led to uncertainty about how teams based in New York would be located and work together in the future. To investigate the effects on different locations of this shock to the certainty of communications, I use a familiar difference-in-differences specification where I use the non-New York offices as a control for changes in behavior caused by the shock in New York. As with any diff-in-diff, the identifying assumption is that the New York office in the absence of the physical relocation would have had a similar time trend and responsiveness to the installed base as employees in other countries. One novelty is that I do not use a traditional diff-in-diff, where the variable of interest would be $NY_i^{*}Unstable_t$, which would capture the level effect of the aftermath of the terrorist attacks on New York employees' adoption decisions. My main variables of interest are the three-way interactions $InstalledPotentials_{it}^{*}NY_i^{*}Unstable_t$ and $InstalledPotentials_{it}^{*}NY_i^{*}Unstable_t$. These measure the differential effect on the installed base of the relocation on the New York offices. Therefore the differences-in-differences specification of equation (1) becomes

$$\begin{aligned} inst_{it}^{*} &= \Delta_1 InstalledContacts_{it} + \Delta_2 InstalledPotentials_{it} \\ +\Delta 3 InstalledContacts_{it}^{*}NY_i^{*}Unstable_t &+ \Delta_4 InstalledPotentials_{it}^{*}NY_i^{*}Unstable_t \\ \gamma(Unstable_t^{*}NY_i + NY_i^{*}InstalledContacts_{it} &+ NY_i^{*}InstalledPotentials_{it} \\ +Unstable_t^{*}InstalledContacts_{it} &+ Unstable_t^{*}InstalledPotentials_{it}) \\ &+\lambda TV_{r,t} + \beta X_i + \omega T_t + \epsilon_{it} \end{aligned} \tag{2}$$

The level effects of $Unstable_t$ and NY_i are captured by the vector of time dummies and location dummies.

Based on initial work by (Prentice and Gloeckler 1978) and formalized by (Allison 1982) and later (Jenkins 1995), the discrete time hazard model has been proposed as an attractive alternative to continuous time hazard models for survival time data. The key insight of this literature is that if survival time data is reordered so that, for each person, there are as many data rows as there are time intervals at risk of the event occurring for each person, then standard binary choice methods can be used to approximate a survival time model. I estimate equation (1) using a probit specification with i.i.d errors using maximum likelihood.

I do not put a causal interpretation any of the results in this specification. In particular, I do not interpret the coefficients on $InstalledContacts_{it}$ and $InstalledPotentials_{it}$. As documented by (Manski 1993) these measures are plagued by the reflection problem, making any causal interpretation of correlations in adoption problematic. Instead, I merely use the three-way interactions $InstalledContacts^*_{it}NY^*_iUnstable_t$ and $InstalledPotentials^*_{it}NY^*_iUnstable_t$ to identify the sign of the change in adoption correlations in New York relative to other cities after the terrorist attacks. In section 5, I use instrumental variables to identify actual network effects.

Table 2 describes the results from probit estimation of equation (2). The first column presents the results for all employees. They key variables of interest are the three way interactions $InstalledContacts^*_{it}NY^*_iUnstable_t$ and $InstalledPotentials^*_{it}NY^*_iUnstable_t$. While New York showed no change in responsiveness to the installed base of their contacts that I can measure precisely, the adoption of employees in New York became increasingly correlated with the measure of the installed base of potential contacts in the firm after the crash. The second and third column of Table 2 breaks down the results by managers. These results suggests that most of the change in responsiveness was due to changing behavior by those with a title of Director or below. This is unsurprising since managing directors have on average the most diverse list of existing contacts (they often communicate outside of their workgroup and function). Therefore my definition of potential contacts probably applies less well to them than to other

Table 2: Probit Specification for Differences in Differences

	All Employees	**Managers**	**Workers**
Unstable*NY*InstalledContacts	-0.0041	-0.0083	-0.0039
	(0.0038)	(0.0056)	(0.0045)
Unstable*NY*InstalledPotentials	0.0039***	0.0029	0.0047***
	(0.0006)	(0.0022)	(0.0007)
InstalledPotentials	0.0006	0.0028	-0.0001
	(0.0014)	(0.0058)	(0.0014)
InstalledContacts	0.0332***	0.0334***	0.0330***
	(0.0028)	(0.0103)	(0.0020)
Unstable*NY	-0.4976***	-0.1248	-0.6560***
	(0.0802)	(0.2439)	(0.0935)
NY*InstalledContacts	0.0056**	0.0081	0.0053**
	(0.0023)	(0.0066)	(0.0025)
Unstable*InstalledContacts	-0.0153***	-0.0268***	-0.0119***
	(0.0033)	(0.0054)	(0.0040)
NY*InstalledPotentials	-0.0006	-0.0000	-0.0007
	(0.0006)	(0.0012)	(0.0006)
Unstable*InstalledPotentials	-0.0001	-0.0022	0.0004
	(0.0012)	(0.0047)	(0.0013)
TV-Watching	0.2915***	-0.3591	0.3315***
	(0.0960)	(0.2916)	(0.0998)
Observations	10217	1033	9184
Month Dummies	Yes	Yes	Yes
Region, Title, Function Dummies	Yes	Yes	Yes

Dependent Variable: Indicator for when an employee first makes an outward videomessaging call
Sample: Adopters who have not yet made a videomessaging call
Probit Specification. Clustered Standard Errors at City level. * p<0.10, ** p<0.05, *** p<0.01

employees further down the hierarchy.

5 Measuring the Network Effects

Though the results in Table 2 are suggestive that responsiveness to the installed base changed for New York, they do not measure causal network effects. I want, however, to quantify the effect of an employee adopting on the adoption decisions of other employees. Correlation between adoption behavior overstates the extent of network effects, however, if all the employees receive similar unobserved shocks to their benefits from adopting (Manski (1993)). Consider two employees who are both instructed to install the technology by their boss; I need a clear identification strategy to avoid interpreting the subsequent correlation in their adoption decisions as a causal network effect.

I use shocks to the technology TV-watching benefit as an instrumental variable to identify how changes in the installed base causally affect an employee's adoption. This exploits three types of variation in the data: Regional variation in the benefit of watching TV; time variation in the benefit of watching TV; and variation in which regions employees have contacts or potential contacts in.

Instrumental variables have been used to identify empirically network effects before - see for example Rysman (2004). What makes this instrumentation strategy unusual is that the instrument is calculated each month for each employee, which allows employee-level consideration of network effects. The TV benefit ($TV_{r,t}$) for each employee's contacts, weighted to reflect the region of each contact, is the instrument for the number of contacts who have installed the technology. Similarly, the instrument for each employee's installed base of contacts of contacts is the TV benefit ($TV_{r,t}$) for that employee's potential contacts, weighted to reflect the region of each potential contact.

Both the installed base of direct contacts ($InstalledContacts_{it}$) and the installed base of contacts's contacts ($InstalledPotentials_{it}$) are instrumented. The instruments are the average TV benefit $TV_{r,t}$ for each employee's contacts and contacts' contacts.

This varies by the which regions these contacts and contacts' contacts work in. In all specifications the installed base measures were precisely identified and the first-stage regressions were significant at the 0.01 level.

To simplify both interpretation and implementation, I present stratified results for equation (1) by period and by city that replicate the differences in differences strategy represented by equation (2). Table 3 displays the estimates for equation (1). The first two columns display results for the "stable" pre-period before September 2001. They suggest that for both New York and other financial centers, the effect of direct contacts adopting was significant while the effect of potential contacts was insignificant. I interpret this to suggest the broader swathe of potential contacts had negligible effects on the adoption of video-messaging by employees. One interpretation of this is that users had predefined social networks and did value the option of calling outside these networks.

After September 2001, the "unstable" period, we observe an entirely different pattern for New York compared to employees in other locations. In both cases estimated network effects for the influence of direct contacts decrease (the estimate for New York becomes statistically insignificant). However, the drop in the point estimate for New York is far larger. Of great interest is the change in the pattern for 'potential contacts'. While for non-New York employees their adoption is still negligible, we see that for New York employees there is now a small but positive influence from their adoption.

Any basic differences in differences specification relies on the identifying assumption that the control group would have had a similar time-trend of behavior to the treated group if there had not been the treatment. One concern, for example, is that employees in Asia may have had a different adoption pattern than their colleagues in New York, due to different cultural biases and concerns for consensus. To alleviate such concerns, I reran my regressions using a more tightly defined control group of employees from the London office. These were the closest employees to the New York office, both in terms of the economy of the country they operated in and the cultural practices of the firm.

Table 3: Probit Specification with Instrumental Variables

	Stable Period		**Instable Period**	
	NYC	**Not NYC**	**NYC**	**Not NYC**
InstalledContacts	0.0389***	0.0325***	0.0162	0.0174***
	(0.0077)	(0.0040)	(0.0100)	(0.0045)
InstalledPotentials	0.0016	-0.0003	0.0060*	-0.0004
	(0.0037)	(0.0021)	(0.0036)	(0.0016)
TV-Watching	-0.3807	0.0676	-0.0430	0.7402***
	(0.2984)	(0.1558)	(0.3977)	(0.2126)
Observations	1383	4017	1276	3541
Month Dummies	Yes	Yes	Yes	Yes
Region, Title, Function Dummies	Yes	Yes	Yes	Yes

Dependent Variable: Indicator for when an employee first makes an outward videomessaging call

Sample: Adopters who have not yet made a videomessaging call

Probit Two-Step Newey GMM estimates: * p<0.10, ** p<0.05, *** p<0.01

Instruments for the different installed base measures are the TV valuation of each employee's actual and potential contacts.

TV valuation is measured by the % of prior adopters who watch local TV in that contact's region in the next month.

First stage regressions significant at 1 percent level. Regression is exactly identified.

Table 6 displays the results of this more limited control group. The results are similar to before though the only significant effect in the unstable period that I measure is the positive effect for New York employees from the installed base.

To address concerns about the reliability of the Probit-IV model when there are multiple dummy variables, I also estimated a linear probability model. The results for this specification displayed in Table 7 in the appendix. The estimates are very similar, with a slight increase in significance, especially for the installed base of actual contacts.

I calculated some rough back-of-the-envelope figures for the implied impact of the relative shock to the New York employees. An average employee has 88 potential contacts. The marginal effect of the installed base of around 0.0004 therefore suggests that the average employee received a boost in their adoption propensity of around 3.5 percent from their potential contacts. To put this boost in context, the average adoption propensity is 8 percent each month. It is important to emphasize that this is a marginal boost to adoption. As suggested by the negative coefficient on $Unstable^{*}NY$ in table 2, on average the level of adoption in New York after the terrorist attacks was slightly lower (around 1 percent in the raw data). However, what changed was the extent to which adoption propensities were *influenced* by the broader network of potential contacts.

6 Implications

Using an extensive dataset on the adoption of an internal video-messaging system within one firm, this paper finds that the empirical structure of network effects reflects interaction patterns within networks. When the network structure was stable, adoption cascades were confined to small subsets of people who interact with each other. However, when the the expected communications network became less stable, potential adopters looked more broadly at the network and were more responsive to adoption by a broader circle of potential contacts.

One plausible explanation of why the broader network matters more during periods of uncertainty is that, in times of uncertainty about how their role in the firm will develop, employees place value on the option of having a broader selection of other people in the network to talk to. Consequently, a potential adopter places an option value on being able hypothetically to talk to someone else even if they never do ultimately communicate. The broader network therefore becomes more important when there is less certainty about whom an adopter will talk to. Traditional theories of option-value and strategic investment under uncertainty emphasize that periods of uncertainty and instability increase the option-value of waiting and consequently deter aggregate investment in technologies. By contrast I find that here instability acts to increase the impact from network effects on adoption by making an adoption decision more responsive to a broader set of adoption decisions. In this case the 'option' of communication with a broader swathe of adopters gives a marginal boost to adoption propensities of 3.5 percent.

My research underscores the fact that network effects are very closely linked to the underlying social network. This is the first empirical study of network effects to include network topology and stability directly in its estimation. The empirical results that highlight the 'localness' of network effects in the stable period support recent theoretical models such as Sundararajan (2004)'s model of multiple equilibria with local network effects and Gale and Kariv (2003)'s models of social learning with myopia in social networks. It also echoes the findings of Mobius (2001) on early small niche telephone markets. There is no research (that I know of) that investigates how network fluidity affects network, effects but the results in this paper suggest that the stability of networks and social interactions is an important moderator of how large or small network effects are.

Often policy assumes that network effects for communications and network technologies depend on the total number of subscribers. My estimates suggest that only the smaller subset of people with whom a potential adopter interacts, play a significant part in the adoption decision. The exception to this rule is when networks are unstable and fluid. If these results hold for other technologies, then this suggests that when communication networks are stable, the scope for network effects in network technologies may be relatively small, reducing the need for policy intervention. However when people are uncertain about how they will use a technology in the future, network technologies are more likely to be be vulnerable to coordination failure as adopters become more responsive to a broader swathe of adopters.

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A TV Watching

I focus on the viewing habits for local television as my instrument. Local channels for Europe were ZDF (German), ARD (German), Kanal (Swedish), ORF (Austria) and Eurosport. Local channels for Britain were ITV, SkySports, Channel 4 and BBC. Local channels for the US were CSPAN, FOX, NBC and CBS. Local channels for Asia were NTV (Nippon TV), CATS (Japanese), TV-Asia, and BBC 24 World Service.

The Soccer World Cup in June 2002 illustrates my identification strategy. Figure 1 in the appendix shows how the percentage of employees who watch local TV programming varies across the US and UK in 2002. Far more employees in the UK watched the Soccer World Cup in June 2002 than in the US. There was a spike in installations in the UK at the time of the World Cup and also a smaller spike in installations in the US. Figure 2 in the appendix shows that the spike in installations in the US in June 2002 consists of employees in the US reacting to TV-inspired installation by their contacts in the UK. This anecdote illustrates the identification strategy. I do not count all earlier adoption by i's contacts as necessarily causing i's installation. Instead I use variation in adoption by i's contacts or contacts' contacts that can be predicted by variation in the stand-alone (TV) benefit.

The video-messaging unit's TV use led to a less systematic pattern of adoption than is common for communication technologies. Table 4 shows that there is no monotonic relationship between adoption timing and the post-adoption intensity of usage of the technology.

B Linear Probability Model Specifications

Figure 1: Relationship between New Installations and TV-watching in the US and UK, 2002

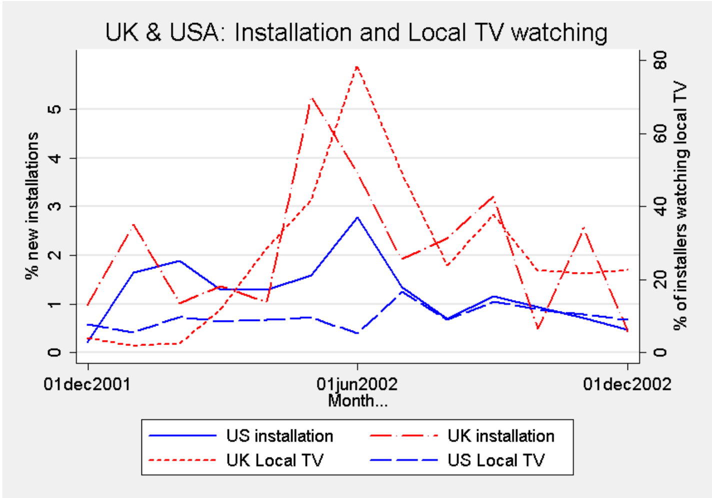

Figure 2: Relationship between New Installations and US employees having any UK contacts

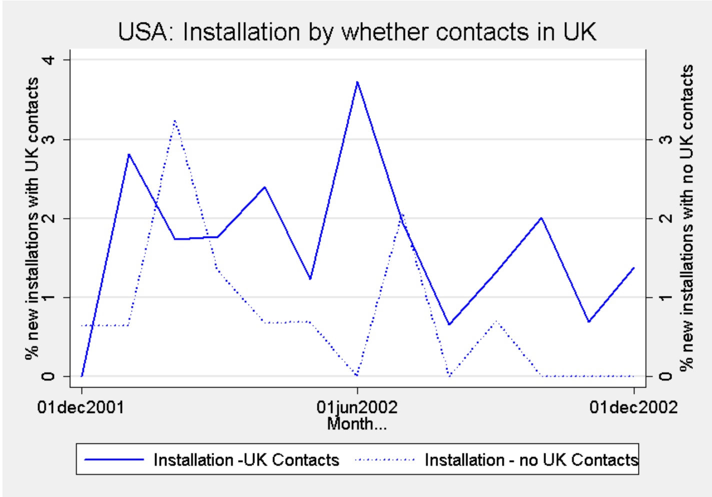

Table 4: **Relationship between Adoption timing and Usage intensity**

Year-Month of Adoption	**Average total calls in last 12 months**	**Average time spent video-messaging each month (one unit is one day**
200102	287.11	1.03
200103	136.75	0.57
200104	101.89	0.40
200105	242.19	0.82
200106	168.32	0.88
200107	87.27	0.24
200108	194.78	0.60
200109	260.59	0.96
200110	144.32	0.44
200111	57.48	0.19
200112	114.93	0.30
200201	77.94	0.26
200202	113.09	0.33
200203	52.94	0.19
200204	277.73	0.86
200205	96.74	0.38
200206	197.17	0.95
200207	186.55	0.56
200208	118.59	0.49

Table 5: Linear Probability Model Specification with Differences in Differences

	All Employees	**Managers**	**Workers**
Unstable*NY*InstalledContacts	-0.0017**	-0.0019	-0.0016**
	(0.0007)	(0.0012)	(0.0007)
Unstable*NY*InstalledPotentials	0.0003***	0.0001	0.0002***
	(0.0001)	(0.0003)	(0.0001)
InstalledPotentials	0.0002	0.0010	0.0001
	(0.0002)	(0.0010)	(0.0002)
InstalledContacts	0.0072***	0.0091***	0.0064***
	(0.0006)	(0.0026)	(0.0005)
Unstable*NY	-0.0275***	0.0015	-0.0273***
	(0.0070)	(0.0332)	(0.0079)
NY*InstalledContacts	0.0013**	0.0016	0.0011**
	(0.0005)	(0.0018)	(0.0005)
Unstable*InstalledContacts	-0.0051***	-0.0081***	-0.0041***
	(0.0006)	(0.0018)	(0.0007)
NY*InstalledPotentials	-0.0001	0.0002	-0.0001
	(0.0001)	(0.0002)	(0.0001)
Unstable*InstalledPotentials	-0.0002	-0.0008	-0.0001
	(0.0002)	(0.0007)	(0.0002)
TV-Watching	0.0209	-0.0844*	0.0240
	(0.0155)	(0.0471)	(0.0162)
Observations	10217	1033	9184
Month Dummies	Yes	Yes	Yes
Region, Title, Function Dummies	Yes	Yes	Yes

Dependent Variable: Indicator for when an employee first makes an outward videomessaging call
Sample: Adopters who have not yet made a videomessaging call
Linear probability model: * p<0.10, ** p<0.05, *** p<0.01

Table 6: Probit Specification with Instrumental Variables using London as a comparison to New York

	Stable Period		Instable Period	
	NYC	**London**	**NYC**	**London**
InstalledContacts	0.0389***	0.0273***	0.0162	0.0114
	(0.0077)	(0.0063)	(0.0100)	(0.0071)
InstalledPotentials	0.0016	-0.0020	0.0060*	-0.0007
	(0.0037)	(0.0035)	(0.0036)	(0.0027)
Observations	1383	1617	1276	1557
Month Dummies	Yes	Yes	Yes	Yes
Region, Title, Function Dummies	Yes	Yes	Yes	Yes

Dependent Variable: Indicator for when an employee first makes an outward videomessaging call
Sample: Adopters who have not yet made a videomessaging call
Probit Two-Step Newey GMM estimates: * $p<0.10$, ** $p<0.05$, *** $p<0.01$
Instruments for the different installed base measures are the TV valuation of each employee's actual and potential contacts.
TV valuation is measured by the % of prior adopters who watch local TV in that contact's region in the next month.
First stage regressions significant at 1 percent level. Regression is exactly identified.

Table 7: Linear Probability Model Specification with Instrumental Variables

	Stable Period		**Instable Period**	
	NYC	**Not NYC**	**NYC**	**Not NYC**
InstalledContacts	0.0077***	0.0070***	0.0012	0.0021***
	(0.0013)	(0.0007)	(0.0008)	(0.0005)
InstalledPotentials	0.0002	-0.0001	0.0004*	-0.0000
	(0.0006)	(0.0003)	(0.0002)	(0.0002)
TV-Watching	-0.1113**	-0.0218	-0.0032	0.0756***
	(0.0529)	(0.0289)	(0.0325)	(0.0204)
Observations	1383	4017	1276	3541
Month Dummies	Yes	Yes	Yes	Yes
Region, Title, Function Dummies	Yes	Yes	Yes	Yes

Dependent Variable: Indicator for when an employee first makes an outward videomessaging call
Sample: Adopters who have not yet made a videomessaging call
Linear Probability Model with instrumental variables MLE estimates: * $p<0.10$, ** $p<0.05$, *** $p<0.01$
Instruments for the different installed base measures are the TV valuation of each employee's actual and potential contacts.
TV valuation is measured by the % of prior adopters who watch local TV in that contact's region in the next month.
First stage regressions significant at 1 percent level. Regression is exactly identified.