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Identifying Peer Effects Using Gold Rushers

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Abstract

Fishers pay attention to where other fishers are fishing, suggesting the potential for peer effects. But peer effects are difficult to identify without an exogenous shifter of peer group membership. We propose an identification strategy that exploits a shifter of peer group membership: gold rushes of new entrants. Following an exchange-rate-induced gold rush in an American fishery, we find that new entrants are strongly influenced by the location choices of their peers. Over-identification tests suggest that the assumptions underlying identification hold when new entrants are inexperienced but identification is lost as new entrants start to potentially influence their peers.

Key words: Peer Effects, Gold Rushes, Resource Extraction.

JEL classification: *J0, Q0, D8.*

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1 Introduction

"Upon spotting another dive boat, the seasoned skipper immediately takes a look through his glasses, mentally recording the area, jotting it down in his notebook, or punching in the coordinates on his plotter. It's important to know where the competition is diving." - Tom Kendrick, *Blue Water Gold Rush*

Peer effects are notoriously difficult to identify without an exogenous source of peer group variation.[1] Well-known examples of exogenous peer group variation include the random assignment of college roommates (Sacerdote, 2001; Zimmerman, 2003), the quasi-random introduction of different personnel into a work shift (Mas and Moretti, 2009), and school integration/reassignment policies (Angrist and Lang, 2004). Peer effects identification in other contexts has been hampered by a lack of experimental or quasi-experimental sources of peer group variation.

In this paper, we propose an identification strategy for estimating peer effects in a resource extraction setting. Resource extractors (such as fishers) have to decide where to extract resources, often with incomplete knowledge about where the most abundant or valuable resources are located. Extractors typically make these decisions based on private and public information: public information may include the location choices of other extractors. In this paper, we are interested in whether an individual resource extractor's location choice is influenced by the location choice of his or her peers.[2] This is valuable public information, especially when private information is limited. We argue that *gold rushes*, defined as a rapid movement of people to a newly discovered resource, provide a rich source of peer group variation. We use this approach to identify a location choice peer effect in an American fishery that experienced a gold rush due to unprecedented demand for high-end sushi in Japan. The approach only appears to work over a limited time horizon, suggesting that the common hurdles to identification quickly become pervasive. This has important implications for the literature on location choice in environmental and resource economics: it demonstrates that peer effects are a genuine empirical phenomenon but are extremely difficult to estimate.

But why bother to estimate peer effects among resource extractors in the first place? Peer effects matter in resource economics for the same reason that they matter in education and labor economics: policy outcomes may be drastically different in the presence of peer interactions, especially peer learning. For example, policies such as forced desegregation and school voucher programs will have different outcomes depending on the nature of peer effects and whether they generate large social multipliers (Epple and Romano, 1998; Hoxby, 2000). Likewise, the outcomes of many resource management policies depend on assumptions about peer interactions. Examples of natural resources where peer effects can change policy outcomes include oil (Polasky, 1996; Lin, 2009), forests (Robalino and Pfaff, 2010), water (Pfeiffer and Lin, 2010) and fish (Costello and Deacon, 2007).[3]

Why use gold rushes to estimate peer effects? One of the main reasons for the lack of reduced form estimation of peer effects in resource settings has been a difficulty in finding exogenous sources of peer variation (Vignaux, 1996).[4] Gold rushes, which occur during the extraction of many natural resources, create a stream of individuals who have not previously interacted with their peers but now have very strong incentives to pay attention to them. As will be outlined in Section 3, this means that many of the biases (simultaneity, correlated effects, etc.) that hinder identification of peer effects using non-experimental data can be overcome.

The gold rush we study occurred in the Northern California sea urchin fishery in the late 1980s and early 1990s due to a boom in the Japanese economy. Section 2 provides some background and describes the data set, while Section 3 presents the empirical framework. Section 4 presents the results and Section 5 tests the robustness of our identifying assumptions. Section 6 concludes.

2 Background and Data

> "The large and unexploited sea urchin biomass in northern California sparked a 'gold rush' as hundreds of new fishermen entered the unregulated fishery." - California Department of Fish and Game, *Annual Status of the Fisheries Report*

The Japanese economy boomed in the late 1980s and started to gradually decline in the 1990s with the Nikkei 225 stock index reaching an all-time high on December 29, 1989 (Figure 1). During this period of growing Japanese affluence, the yen strengthened sharply against the US dollar, reaching an all-time high in April 1995 (Figure 1). Rising demand for luxury goods among Japan's wealthy elite in tandem with a weak dollar caused a rapid appreciation in the price paid for Californian sea urchin; the average price paid for a pound of sea urchins increased roughly 350% from 1988 to 1995. Sea urchin roe is a delicacy served in sushi restaurants where it is listed on menus as *uni*. *Uni* is reputed to be an aphrodisiac and is often the most expensive item available, with Californian urchins particularly sought after. Unprecedented demand for sea urchins, combined with declining stocks in southern California, caused a modern-day Californian gold rush as hundreds of urchin boat captains migrated to Northern California's frigid, shark-infested but urchin-rich waters. Figure 2 shows the extent of the "Blue Water Gold Rush": from a small group of roughly 30 boats in 1987, there was a surge of new entrants in 1988 (84 new entrants), 1989 (71 new entrants) and 1990 (49 new entrants), with the fishery reaching a peak of 154 boats in 1992.[5]

Fishing for sea urchins is a relatively low-tech activity. In the northern California red urchin fishery, urchin divers make single day trips to locations close to shore where they may dive as deep as 60 feet. Nearly all boats are owned by the divers themselves. A diver is normally connected to the surface by a tube that supplies compressed air. On a typical dive, an urchin diver will drag a large rope bag down with him, anchor that to the bottom then set off with a rope cage, fill that up with urchins, come back to the bag, offload the harvested urchins in the bag and then set off again. When done, the diver inflates a buoy attached to the bag with oxygen and it rises to the surface. Visibility and fitness are key determinants of productivity (conditional on there being urchins at a particular site). At the dock, the urchins are unloaded and a processor removes the gonads (urchin roe). About 10% of the weight of a whole urchin is roe.

Aside from the benefit of the gold rush causing an influx of new peers, the Northern California sea urchin fishery has other features that are conducive to the estimation of peer effects. First, the biology of the red sea urchin (*Strongylocentrotus franciscanus*) is an asset: urchins remain in the same geographic locations for long periods of time so information about productive areas remains valuable for much longer than in fisheries where the target species is highly mobile.[6] Urchins are firmly attached to the sea-floor by adhesive tube-like feet so, unlike shrimp or other pelagic species, their spatial distribution is not affected by short term changes in environmental conditions. Second, boats make day trips to spots close to their home port so it is easy to follow and observe other boats. Most dives occur at a depth of 30 feet, implying that boats remain close to shore where they can be easily observed by other boats. Diving at depths greater than 60 feet is both dangerous and less productive. In addition, urchin divers have to see what they're harvesting, thus diving at night to avoid observation by other divers is not an option. Third, visual inspection of the data confirms that the fleet tends to move as a pack: there are long periods where most of the fleet is fishing in the same location. This means that an individual captain frequently receives a clear signal of where his peers believe the best urchin grounds are located.

While it is possible to see where other boats are going, it is difficult to infer how well another vessel is doing. This is because urchin diving is a *quality* fishery not a *quantity* fishery: the value of a vessel's catch does not necessarily depend on the number or size of the urchins harvested but on the quality of the *uni* contained within the urchins.[7] Seeing another boat with a large quantity of big urchins does not necessarily imply that the load is valuable.

In other words, the value of a captain's catch is private and confidential information known only to the captain and the processor who pays for the catch.

Captains therefore have very strong incentives to pay attention to the location choices of other captains.[8] In fact, the decision about where to fish is very similar to an investment under uncertainty decision. No one individual knows with certainty the best action to take but makes a decision based on their private information and any publicly available information. In the fishing context, publicly available information includes the location choices of other fishermen. It is clear that there should be some form of location choice peer effect: fishermen learn by observing the location choices of others and this, in turn, influences their own location choices. The precise form of learning will depend on a number of factors but especially on beliefs about others' beliefs, the timing of decision-making, and the degree to which actions reveal privately held information.

Based on interviews with urchin captains and a general understanding of urchin diving, we hypothesize that the following factors play a key role in the decision about where to go fishing: (i) Where have I made the most money recently? (ii) Which locations do I have more experience diving at? (iii) What is the weather doing today? And, critically, (iv) where have I observed other captains fishing? The motivation for including experience in the decision problem is twofold. Roe content and quality is directly correlated with habitat. Since habitat remains fairly constant over time, locations that have produced high quality roe in the past are very likely to produce good roe again in the future. The experience variable captures the important role of this information, which is separate from recent fluctuations in urchin abundance and quality. Second, there is some location-specific human capital accumulation: if a captain is indifferent between two locations in terms of his expected catch value, he is more likely to go to the location that he has more experience with.

To test for a location choice peer effect, we use data on location choice and catch value obtained from California Department of Fish and Game (DFG) log books and landings tickets for the Northern California sea urchin fishery from 1987 to 1999. We restrict our attention to the Fort Bragg and Point Arena ports for two main reasons. First, the ports are far apart so it is fair to treat captains in the two ports as two distinct peer groups (Figure 3). Captains rarely, if ever, travel far enough that they could observe captains from a port other than their own. Second, 71% of new captains begin their careers fishing out of either Fort Bragg or Point Arena. We use the log books and landings tickets to generate most of our variables of interest: where a captain chooses to go on any given day (this is a dummy variable for north or south, relative to the captain's home port), the average choice of his peer group on that day, his expected catch value in the north relative to the south, the average of his peer group's expected catch value in the north relative to the south, how much of his experience has been in the north relative to the south and how much of his peer group's experience has been in the north relative to the south.

We simplify the location choice as the decision to go north or south out of a vessel's home port for several reasons. First, it is well documented that vessels in this fishery only go to patches that are close to their home port (Smith and Wilen, 2003; Smith, 2005). Second, there are historically productive urchin grounds in close proximity to the north and south of both ports suggesting that, at any given time, either choice is feasible.[9] Third, there are considerable set-up costs involved in preparing for a dive, therefore boats tend to remain in the same area for the duration of the day. Finally, interviews with captains further confirmed that north/south is the most appropriate metric. In Section 5, we test whether our results are robust to relaxing this assumption and allow for a more diverse choice set.

To generate the variable for where a new captain's peers go, we calculate the mean choice of all captains in the same port, excluding the captain under study and any other new captains. An important consideration is how much variability there is among vessels going north or south. The vast majority of vessels (73.5%) take trips to both locations but, importantly for our purposes, the fleet of experienced captains tends to move as a herd: for most of the days in our sample (52%), the entire fleet travels to the same location (Figure 4). This means that new captains frequently observe very strong signals from their peers.

To generate a variable for a captain's expectation of the catch value in the north relative to the south we assume that all captains begin their careers with a prior belief over the catch value difference, c_{N-S}, that is distributed Gaussian with mean zero and zero precision[10], i.e. $f_{i,t}(c_{N-S}) \sim N\left(\theta_{i,t}, \frac{1}{\tau_{i,t}}\right)$ with $\theta_{i,1} = \tau_{i,1} = 0$, where i indexes an individual captain and t indexes days spent fishing. Catch value is simply the dollars earned per diver per day (this is a common standardization). The assumption of a mean zero prior is a strong one but justified given that there are productive urchin grounds to both the north and south of each port and fine-scale information about actual urchin abundance can only be gathered through diving. We treat the value of the catch that a captain records on a given day as a Gaussian distributed signal with an unknown mean but a known variance. This allows us to use a simple Bayesian framework to update the mean and precision of the captain's prior belief:

$$\theta_{i,t+1} = \frac{\tau_{i,t}\theta_{i,t} + r_{i,t}X_{i,t}}{\tau_{i,t} + r_{i,t}},$$

is the update rule used for the mean of the captain's posterior belief and

$$\tau_{i,t+1} = \tau_{i,t} + r_{i,t},$$

is the update rule used for the precision of his posterior belief.[11] In terms of notation, $\theta_{i,t}$ ($\theta_{i,t+1}$) is the mean of the captain's prior (posterior), $\tau_{i,t}$ ($\tau_{i,t+1}$) is the precision of the captain's prior (posterior) belief, $r_{i,t}$ is the precision of the catch value signal received on day t and $X_{i,t}$ is the actual catch value signal received. $X_{i,t}$ is measured in dollars weighted by the location that the catch occurred at. For example, a catch in the north worth \$1,000 is equivalent to $X_{i,t} = 1,000$ and a catch in the south worth \$1,000 is equivalent to $X_{i,t} = -1,000$. We define the precision of the catch value signal to be dependent on its timing ($r_{i,s} = 1, \ \forall s \geq t - 20$ else $r_{i,s} = 0$), thus captains place more weight on more recent catch values. A 20 day time horizon is consistent with both the existing literature (Smith and Wilen, 2003) and interviews with captains; we also experimented with different cutoffs and this did not qualitatively alter our results.[12]

We also compile data on daily weather conditions (wave height, wind speed and wind direction) using the National Data Buoy Center.[13] We generate a dummy variable for each possible wind direction (north, south, east and west). Interviews with former captains suggest that, all else being equal, the direction of the wind and waves would influence their location choice. Unfortunately, we do not have data on wave direction. Since swells tend to come from the south during the summer (the period during which most of our observations occur), we would expect a tendency to head south during bigger swells. Weather tends to be calmer in the morning so it is better to steer against the prevailing wind and swell in the morning and return with the swell and wind at your back later in the day. The variable for relative experience in the north is simply the fraction of a captain's previous trips that have been to the north. Finally, we define new entrants using three different metrics: any new captain on his first day, any new captain in his first week and any new captain in his first month in the fishery. Summary statistics for the three samples are presented in Tables 1, 2 and 3.

3 Empirical Framework

Why is it so challenging to identify peer effects without a source of exogenous peer group variation? The challenges can be placed in three broad categories: (i) self-selection (ii) simultaneity and (iii) shared unobservables. The first challenge is that similar individuals self-select into neighborhoods, groups, professions, etc., making it difficult to disentangle selection effects from peer effects. The second challenge refers to the fact that an individual affects her peer group and is simultaneously affected by her peers; the simultaneity of the interactions makes separating causal impacts extremely difficult. The third challenge is a variant of the standard endogeneity problem: an individual may imitate her peers because of some other unobserved influence that is correlated with the peer group's behavior. In this section, we will outline the identification strategy we use to overcome these three challenges. The proposed strategy relies critically on using the gold rush as a source of peer group variation.

We start with the issue of self-selection. Clearly, individuals who choose to earn a living by diving for sea urchins have self-selected into the profession. What is critical for identification in our setting is that gold rushers have been incentivized to self-select into a fishery that they would not normally operate in. The Northern California coast is an extremely hostile environment to work in compared to Southern California. The water is cold, murky, and notorious for shark attacks. This is evidenced by the small number of boat captains working in the fishery before and after the Japanese boom. As outlined in Moffitt (2001), identification is possible when interventions offer inducements to alter the composition of peer groups. The Japanese boom clearly offered inducements that altered the composition of peer groups. Without the Japanese boom, very few (if any) of the gold rushers would have self-selected into this fishery. Only one of the new captains was a diver already working in the fishery on another captain's boat before investing in his own boat. He is excluded from the sample of gold rush captains.

We now address the issue of simultaneity. Consider the simple linear regression model $y = \alpha + \beta x + \epsilon$ where y is the behavior of an individual and x is the behavior of her peer group. The other variables have the usual interpretations. The error term, ϵ, captures any variation in y that is not explained by variation in x and α is an intercept. Now, if the peer group's behavior x is directly influenced by the individual's behavior y then this reverse causality provides an indirect pathway for the error term ϵ to be correlated with the regressor, because unobserved influences on y become unobserved influences on x. If this correlation is present, then ordinary least squares (OLS) estimation of β will be inconsistent. This is one of the types of *reflection problems* referred to in seminal work by Manski (1993, p. 532): "the problem is similar to that of interpreting the almost simultaneous movements of a person and his reflection in a mirror. Does the mirror image cause the person's movements or reflect them? An observer who does not understand something of optics and human behaviour would not be able to tell." A gold rush allows us to break this simultaneous reflection of behavior because the social interaction is clearly unidirectional: new entrants (gold rushers) can learn from observing the location choices of existing captains but existing captains do not learn anything from observing new entrants.

We now tackle the third challenge of shared unobservables. Manski (1993) identifies three reasons why agents may imitate the behavior of their peers: correlated effects, exogenous effects and endogenous effects. Correlated effects refer to agents taking the same action as their peers because they face a similar environment or have similar characteristics. For example, all the fish are aggregated in one location so every captain decides to go there, regardless of what their peers are doing. Exogenous effects refer to a direct causal relationship between the characteristics of a peer group and the agent's behavior. For example, a captain's peer group may be very macho, causing a self-conscious captain to go to more dangerous locations. In other words, a captain is influenced by the *characteristics* of his peer group not by their *actions*. Endogenous effects are "true" peer effects: an agent's behavior literally varies with the behavior of the peer group. For example, observing a large number of boats in a particular area directly causes a captain to go to that area. True peer effects are difficult to identify because of the possibility of correlated or exogenous effects. This is akin to the standard endogeneity problem faced in analysis of non-experimental data.

A common approach to overcoming endogeneity is to find appropriate instrumental variables for the endogenous regressor (the peer group's location choice). Good instruments would be variables that influence the peer group but we can be certain that they are not correlated with unobserved influences on the individual under study. Since individuals and their peers are typically exposed to the same influences, this makes finding appropriate instruments almost impossible. This is again where the gold rush is critical. The gold rush introduces new individuals to each fishing port. These individuals do not know what existing fishermen have been catching and where they have been catching it. Thus, we can use information accrued by existing fishermen in the past, namely catch and historical experience at different locations, as instrumental variables. This is information that predicts where existing fishermen go but should be orthogonal to unobserved influences on new entrants because new entrants are completely unaware of this information. It is, of course, possible that existing fishermen and new entrants have the same beliefs about where to go, despite having very different information sets. This would be an example of correlated effects that our instrumental variables approach could not overcome. However, this is only likely to be true in two very extreme situations. The first situation is one where the beliefs of existing captains and new entrants are highly correlated *and* the private information of existing captains does not influence their beliefs. This would imply that the information received from going fishing is essentially worthless. As we will soon see, this is not borne out by the data. Captains respond rationally to changes in their private information sets, strongly suggesting that new information is changing beliefs. This is essentially the exogenous variation that we are using to identify a peer effect. Even if new entrants and existing captains have highly correlated beliefs, as long as the private information of existing captains is affecting the beliefs of existing captains but not *directly* affecting the beliefs of new entrants then the endogeneity problem is overcome. The second situation that might be of concern is one where the information that existing captains observe causes them to believe the same thing as new entrants who have not been exposed to the same information. For example, an existing captain believes A, a new entrant believes B, the existing captain updates on new information and now believes B, the same belief as the new entrant who has a very limited information set. This seems unlikely but it remains a possibility. To summarize, gold rushes allow for the identification of peer effects because they induce self-selection that would not occur otherwise, they break the simultaneity of decision-making and they create potential (but not necessarily valid) instrumental variables.

We now outline the model we are going to estimate using the gold rushers as the backbone of our identification strategy. We wish to estimate the following as our main (i.e. second-stage) regression:

$$\underbrace{y_{i,p,t}}_{\substack{Gold\ rusher's \\ location\ choice}} = \mathbf{fe_{i,p}} + \underbrace{\beta\bar{y}_{-i,p,t}}_{\substack{Average\ location\ choice \\ of\ existing\ captains}} + \mathbf{x'_{i,p,t}}\gamma + \epsilon_{i,p,t}. \tag{1}$$

This tests for a peer effect from the existing captains to the new captains (the gold rushers). For new captain i, his peer group of existing captains excludes any other new captains who have entered the fishery at approximately the same time. The dependent variable $y_{i,p,t}$ is a binary variable for whether new captain i goes to the north or south of his home port p on day t, where a value of 1 represents a trip to the north. The location choice $y_{i,p,t}$ is conditional on both choosing to enter the fishery and choosing to go fishing on day t. The key explanatory variable $\bar{y}_{-i,p,t}$ is the average location choice of all the existing captains from the same port on the same day. In line with the literature on peer effects in other settings (Manski, 1993, 2000; Duflo and Saez, 2002; Mas and Moretti, 2009), we assume a linear peer effect as a best approximation to the true underlying response function.[14] Other explanatory variables (the vector $\mathbf{x_{i,p,t}}$) include the new captain's personal catch value history and experience (if they exist) and a vector of weather variables, namely wind speed and wave height. The regression includes captain and port fixed effects ($\mathbf{fe_{i,p}}$).[15] As mentioned earlier, the time horizon, t, ranges from 1 to 20 depending on whether the sample is restricted to a new captain's first day, second day, first week, etc.[16] Note that $t = 1$ is a different calendar date for each new captain unless they start fishing from the same port on the exact same day.

Although the dependent variable is binary, we present results from estimating Equation (1) using both ordinary least squares and a probit estimator. The advantage to using OLS is that we can deal with potential unobservables by using instrumental variables and removing captain-specific fixed effects by demeaning the data. The obvious drawback is that the error terms in a linear probability model are distributed Bernoulli and performing hypothesis tests based on assuming an underlying Gaussian distribution is not technically correct. This approach has been adopted elsewhere for the simple reason that, "the estimation of dynamic panel data models with a discrete dependent variable is essentially an unsolved problem in the classical statistics literature" (Klaassen and Magnus, 2001, ; p. 501). The advantage to using an instrumental variables probit estimator is that it is appropriate to assume an underlying Gaussian distribution and test statistics have the usual interpretation. The disadvantage is that we can no longer simply difference out time-invariant unobservables when using a nonlinear model.[17]

As described above, we need to instrument for the average location choice of the existing captains to deal with the possibility of correlated or exogenous effects. Our first-stage regression uses the historical catch and experience of existing captains to predict their average location choice:

$$\underbrace{\bar{y}_{-i,p,t}}_{\substack{\textit{Average location choice}\\ \textit{of existing captains}}} = \mathbf{fe_{i,p}} + \underbrace{\begin{bmatrix} c\bar{a}tch_{-i,p,t} \\ exp\bar{e}rience_{-i,p,t} \end{bmatrix}' \lambda}_{\substack{\textit{Average catch and experience}\\ \textit{of existing captains}}} + \mathbf{x_{i,p,t}}'\rho + u_{i,p,t}, \quad (2)$$

where $c\bar{a}tch_{-i,p,t}$ is the average catch of existing captains and $exp\bar{e}rience_{-i,p,t}$ is their average experience in the north relative to the south. Our identification strategy rests on the assumptions that existing captains are not influenced by new captains, that new captains are unaware of the location of the past catch and experience of existing captains, and consequently this information is statistically independent of any unobserved influences on new captains.

The instrumental variables approach also provides indirect evidence of whether any identified peer effect is social learning. We are testing whether the location choice of new captains is correlated with the part of the peer group's location choice that is correlated with important information about urchin abundance (i.e. where urchin captains have gone and what they have caught there). Another way of saying this is that we're testing whether new entrants are behaving in a way that is consistent with information they can not directly observe but would like to learn. This is not to say that we can disentangle whether new captains learn from the behavior of existing captains or simply conform with their behavior but it does indirectly test whether following the herd is a good idea. We do find that those captains who followed the herd (i.e. made the same choice as the majority of existing captains) were more likely to have made the right choice on that day.[18] For the First Day sample, 82.67% of the new captains who followed the herd made the right choice, compared to 16.98% of the captains who did not follow the herd. For the First Week sample, 81.85% of the new captains who followed the herd made the right choice compared to 22.94% of the captains who did not follow the herd. For the First Month sample, 83.32% of the new captains who followed the herd made the right choice, compared to 23.34% of those who chose to not follow the herd. All of these proportions are statistically different from each other at better than the 1% level.

An important question for our identification strategy is: at what point do new entrants become existing captains? This presents both an obstacle and an opportunity. It presents an obstacle in that, as new entrants become more experienced, the assumptions underlying identification must break down. Our identification strategy is therefore temporally limited to the period over which existing captains are not influenced by new captains. It presents an opportunity in that our model is over-identified and we can test whether we lose identification as new entrants start to influence existing captains. We therefore estimate our regression model and perform over-identification tests for a range of time horizons: new captains on their first day in the fishery, their first two days, their first week, their first month, and so on. The drawback to restricting the time horizon to each new captain's very first day is a small sample size (180 observations) and an inability to control for time-invariant differences between captains. The benefit, however, is that we can be certain the private information of existing captains has not been influenced by the past actions of new entrants because the new entrants have not taken any actions yet. Conversely, the week and month samples allow for a larger sample size and the ability to control for unobserved differences between captains. However, we cannot be completely certain that the catch and experience of existing captains is not beginning to be influenced by the new entrants.

4 Results

We start with the results from running OLS and probit estimation of Equation (1) without instruments for the three different samples (Tables 4 and 5). The magnitude of the peer effect is similar across all three samples. If everyone in a new captain's peer group decides to go north then this increases the probability that the new captain will go north by 20 to 29 percentage points. The estimated peer effect is not statistically different from zero in the First Day sample, perhaps due to the small sample size, and is statistically significant at the 5% level or better in the First Week and First Month samples.[19] In addition, OLS estimation suggests that the strength of the peer effect does not decline as new captains become more experienced. The coefficients on the other variables are generally consistent with our expectations, except for the negative coefficient on the relative experience variable in the first week sample, which is suggestive of strategic experimentation.

Table 6 presents the results from estimating the first stage of the instrumental variables model for the three different samples. We can see that the peer group responds rationally to their catch history and experience. The peer group is more likely to go north when the value of their recent catch in the north is greater than the value of their recent catch in the south. In addition, if all of the peer group's experience has been in the north then they are between 47 and 73 percentage points more likely to go to the north. In all three samples, at least one of the instruments is statistically significant at the 1 percent level. In terms of evaluating the overall strength of the chosen instruments, the F statistic on the joint significance of the instruments is high as well as the partial R-squared of the instruments (relative to the overall R-squared), confirming that the instruments are strongly correlated with the endogenous regressor (Bound et al., 1995). The negative signs on the coefficients for wind speed and wave height align with information obtained from interviews with captains: if all else is equal, there is a tendency to go south if strong southerly winds or swells are expected and *vice versa*.

Results from estimating the second stage regression are presented in Table 7. In addition, the results obtained from using a probit estimator instead of ordinary least squares are presented in Table 8. For all samples, there is a strong peer effect. Not only is the peer effect strong in a statistical sense, it is very strong in terms of its implications. As an example, looking at the First Day sample in Column (1) of Table 7, if all of a new captain's peer group switch from going south to going north then this is associated with a 73 percentage points increase in the probability that the new captain will go north. For the First Week sample the associated change is 89 percentage points but this declines to 50 percentage points for the First Month sample. This attenuation of the peer effect over time is indirectly supportive of our earlier assumption that captains have a mean-zero prior over catch distribution when first entering the fishery.

It is interesting to note how strong the peer effect is relative to a new captain's private influences. New captains appear to ignore their personal catch and simply follow the herd. This is in contrast to results from laboratory experiments which have found that subjects place too much weight on their private information relative to publicly available sources of information (Celen and Kariv, 2004). Also, for their first week in the fishery, we observe a negative correlation between where new captains have gone in the past and where they go on the day in question (this negative correlation was also a feature of the OLS results in Table 4). We could speculate that this is evidence of strategic experimentation, especially since the coefficient on this variable has the expected positive sign for the first month sample, but we cannot rule out other explanations.

5 Robustness Checks

5.1 Over-identification Tests

We now indirectly test the validity of our instruments. A standard approach to testing this assumption when the endogenous regressor is over-identified is to use Hansen's J test (Hansen, 1982). Over-identification allows us to test (conditional on one of the instruments being exogenous) the null hypothesis that both of the instruments are exogenous. The alternative hypothesis is that at least one of the instruments is endogenous. It is important to note that failure to reject the null does not imply that all of the instruments are valid, since the test is based on the assumption that at least one of the instruments is valid. Nevertheless, table 9 confirms that our specifications pass this standard over-identification test. In all three samples (first day, first week, first month), we fail to reject the null that the instruments (past catch and experience of existing captains) are uncorrelated with any unobserved influences on new captains, although we can reject the null at the 10% confidence level for the first month sample.[20]

We now go a step further and hypothesize that if our instruments are indeed valid, then the calculated Hansen's J statistic should move in the direction of rejecting the null hypothesis as we dilute the validity of our instruments. In particular, the assumption that existing captains are not influenced by new captains is likely to be less valid as we increase the time horizon of our sample size. It is rational for an existing captain to ignore a novice captain on his first few days on the job. However, any captain with more than a few weeks experience is probably worth paying attention to. Thus, we should expect the reflection problem to emerge: existing captains begin to be influenced by new captains so their catch history and experience become correlated with unobserved influences on new captains implying that catch history and experience are no longer valid instruments. The results presented in Table 9 support this hypothesis: as we increase the time horizon, we start to reject the null hypothesis that both instruments are exogenous. The p-value on Hansen's J test declines monotonically from 0.4 for the first day sample to 0.29 for the first two days sample to 0.1 for the first week sample to 0.09 for the first month sample.

An acute observer might initially think that the monotonic decline in the p-value of the over-identification test statistic is merely due to the power of the test improving as the sample size increases, i.e. we fail to reject a false null hypothesis too often with small sample sizes. To address this concern, we take our largest sample (this is the First Month sample, which has 2,721 observations) and create 1,000 bootstrapped samples of equal size to our smallest sample (the 1 day sample with 180 observations). We then perform Hansen's test of over-identification on all 1,000 sub-samples and record the associated p-values. A histogram of the p-values is presented in Figure 5. Although the mean p-value is quite high (0.21), the median value is low (0.13) as the distribution of values is clearly skewed to the left. This suggests that sample size plays some role in the decline of the estimated p-value as we increase the number of days included in the sample but that the main driving force is the loss of identification.

A similar critique of the conclusion implied by Table 9 is that the test is undersized in small samples. However, as established in Monte Carlo simulations by Hall and Horowitz (1996), the standard over-identification test is over-sized in small samples, implying that we tend to reject a true null too often. In terms of the size of the test, the p-values in Table 9 should therefore be interpreted as being underestimated, not over-estimated, for the smaller sample sizes. In short, Hansen's test confirms the intuition that our identification assumptions are most credible when new captains are brand new (their first and second days on the job). Many of the social interactions that make it so hard to cleanly identify peer effects have not had a chance to happen yet. As new captains become more experienced and presumably start interacting with existing captains, it appears that the case for identification starts to break down.[21]

5.2 Increasing the Spatial Resolution of the Key Variables

Another potential critique of the analysis presented in this paper is that by simplifying the location choice of each captain into the decision to go north or south we are biasing our results in favor of finding a peer effect. For example, if we were to go one step further and simplify the location choice decision as the decision to go fishing in the ocean or on land then it would always be true that new captains make the same location choice as their peer group. Despite the fact that previous research (Smith and Wilen, 2003; Smith, 2005) and interviews with captains supports the model specification we have assumed, it is important to address this concern. To this end, we divide the coastline in our study area into eight discrete but ordinal zones, each of roughly 6 nautical miles in length. Two of the zones are located directly south of Point Arena, two are located directly north of Point Arena, two are located directly south of Fort Bragg and two are located directly north of Fort Bragg. Our choice variable is now a discrete but ordinal variable (0 for the south zone that is farthest from a captain's home port, 1 for the south zone that is closest to port, 2 for the north zone that is closest to port, and 3 for the north zone that is farthest from port). We re-calculate all of our key variables in terms of the redefined choice set. For example, the catch value and relative experience variables are now weighted by how far north or south a captain went. It is extremely important to note that this is a crude and imperfect way to increase the spatial resolution of our analysis. The aim is perform a basic robustness check and not to develop an entire new model. Weighting the catch value by how far north or south a captain went will tend to overweight catch values in the far north and far south but it approximately captures how beliefs should change as captains experience different catch values in different locations.[22]

Summary statistics are presented in Table 10. The location choice now ranges from 0 to 3 and the experience variable still ranges from 0 to 1 but a value of 0 indicates that all of a captain's experience has been in the location to the far south of his home port and a value of 1 indicates that all of a captain's experience has been in the far north. The results from estimating our main instrumental variables regression model, Equation (1), with this new data set are presented in Table 11. Although the coefficients are not directly interpretable, we observe a statistically significant peer effect in all three samples. This suggests that our earlier results are not merely a consequence of aggregating location choices into a binary variable. However, as noted above, this robustness check is imperfect and these results should be interpreted with caution. For the First Week sample, the coefficient on wave height is no longer significant. This may be because wave height strongly influences the decision to go north or south in the morning but has less of an impact on the decision of how far north or south to go.

5.3 Alternative Functional Forms for the Peer Effect

We have assumed a linear relationship between the average choice of the peer group and an individual captain's choice. However, this may not be realistic. It may be that the magnitude of the difference in the proportion going north and south affects the degree to which new captains follow existing captains. It could also be the case that new entrants cannot distinguish between 50% and 51% of the fleet going to a particular area but undoubtedly can see when almost everyone is going north or south. To address this concern, we model the peer effect using some potentially more realistic mechanisms. First, instead of representing the behavior of the peer group as the arithmetic mean of the group's choices, we use a simple convex function to capture deviations from the mean: the peer effect variable is now: $(\bar{y}_{-i,p,t})^2 - E[\bar{y}_{-i,p,t}]$. Using this quadratic function as our peer effect variable results in a larger estimated coefficient on the peer effect in all three samples. For the first day sample, the coefficient increases from 0.7335 to 0.9473, for the first week sample the coefficient increases from 0.8871 to 1.1416, and for the first month sample it increases from 0.4960 to 0.5835 (see Table 12 for detailed results). In all cases, we obtain similar first stage results to Table 6 with F statistics all greater than 10.

As a further exploration of alternative functional forms, we created a new peer effect variable that attempts to capture the difficulty new captains may have in detecting small deviations in the behavior of the fleet. We label this new peer effect variable the "All or Almost All" peer effect and generate it as follows:

$$y^*_{-i,p,t} = 0 \ if \ \bar{y}_{-i,p,t} \leq 0.25; \ 0.5 \ if \ 0.25 < \bar{y}_{-i,p,t} < 0.75; \ 1 \ if \ \bar{y}_{-i,p,t} \geq 0.75.$$

This new variable attempts to capture the idea that captains can easily distinguish between a peer group that goes "all or almost all south", "mixed", and "all or almost all north", but perhaps have difficulty distinguishing between intermediate states. Results using this recoded variable as our peer effects variable are presented in Table 13. The estimated peer effect coefficients are similar in magnitude to those in Table 7 but the standard errors and the associated p-values are smaller, suggesting that this alternative specification might be more accurately capturing the type of peer group information that new captains are responding to. Reassuringly, all three functional forms for the peer effect produce similar results with a statistically significant correlation between peer location choice and individual location choice for all three time horizon samples.

6 Conclusion

Despite its importance, estimating the actual magnitude of peer effects in natural resource settings has remained elusive. Reduced-form approaches to estimation are becoming increasingly popular in resource economics (Abbott and Wilen, 2010) and we contribute to this literature with an identification strategy that can be implemented using readily available data.[23] Gold rushes provide quasi-experimental peer group variation that can be used to estimate and study peer interactions. We have implemented the strategy for a Californian fishery and found strong evidence of peer effects. Peer group location choices are an important influence on individual location choice.

Post-estimation diagnosis suggests that the assumptions underlying identification are robust when new entrants are very inexperienced but the classic hurdles to identification emerge as new entrants become more experienced, potentially influencing their peers. This highlights: (i) the importance of finding legitimate sources of identification for estimating peer effects and, (ii) the danger in assuming that observed correlations in behavior provide evidence for peer interactions. Despite these caveats, the results demonstrate that peer effects are important to understanding resource extraction patterns and can clearly affect policy outcomes (see, for example, recent work by Felthoven et al. (2014) on the impact of peer effects on the Bering Sea Crab Rationalization Program). Peer effects may be even more critical in fisheries where the target species is highly mobile or in other resource extraction settings. A fruitful area of future research would be to examine similar data in other settings.

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Tables

Table 1: Summary Statistics for First Day Sample

Variable	Obs	Mean	Std. Dev.	Min	Max
New captain's location choice	180	0.261	0.440	0	1
Peer group's location choice	180	0.260	0.212	0	1
Peer group's recent catch ($000s)	180	-0.217	0.188	-0.985	0.471
Peer group's historical experience	180	0.256	0.125	0	0.771
Wind speed (m/s)	180	4.732	2.138	1.342	10.979
Wave height (m)	180	1.794	0.529	0.756	3.225

Table 2: Summary Statistics for First Week Sample

Variable	Obs	Mean	Std. Dev.	Min	Max
New captain's location choice	859	0.249	0.433	0	1
Peer group's location choice	859	0.257	0.196	0	1
New captain's recent catch ($000s)	859	-0.088	0.354	-1.957	1.492
New captain's historical experience	859	0.309	0.360	0	1
Peer group's recent catch ($000s)	859	-0.225	0.197	-2.173	0.757
Peer group's historical experience	859	0.252	0.116	0	0.771
Wind speed (m/s)	859	4.722	2.233	1.321	11.388
Wave height (m)	859	1.723	0.492	0.756	3.225

Table 3: Summary Statistics for First Month Sample

Variable	Obs	Mean	Std. Dev.	Min	Max
New captain's location choice	2,721	0.265	0.441	0	1
Peer group's location choice	2,721	0.268	0.204	0	1
New captain's recent catch ($000s)	2,721	-0.144	0.428	-3.109	2.282
New captain's historical experience	2,721	0.281	0.342	0	1
Peer group's recent catch ($000s)	2,721	-0.216	0.201	-1.823	1.014
Peer group's historical experience	2,721	0.255	0.120	0	1
Wind speed (m/s)	2,721	4.628	2.168	1.192	12.939
Wave height (m)	2,721	1.703	0.509	0.738	3.950

Table 4: OLS Regression Results

	(1)	(2)	(3)
	First Day	First Week	First Month
Peer group's location choice	0.2502	0.2061***	0.2379***
	(0.1879)	(0.0790)	(0.0493)
New captain's catch history	-	0.0119	0.0136
		(0.0382)	(0.0483)
New captain's relative experience	-	-0.1890***	0.1553**
		(0.0374)	(0.0629)
Wind speed weighted by direction	-0.0361**	0.0003	-0.0006
	(0.0164)	(0.0084)	(0.0049)
Wave height	-0.0331	-0.0728**	-0.0763***
	(0.0694)	(0.0334)	(0.0197)
Wind direction dummies	Yes	Yes	Yes
Captain Fixed Effects	No	Yes	Yes
Port Dummy	Yes	Yes	Yes
R-squared	.15	0.13	0.26
Observations	180	859	2721

Notes: Dependent variable is the location choice of a new captain. Robust standard errors clustered by captain are in parentheses. Each column represents a different sample. Column (1) restricts the sample to each new captain's first day in the fishery. Column (2) restricts the sample to each new captain's first week in the fishery. Column (3) restricts the sample to each new captain's first month in the fishery. * indicates statistically significant at the 10% level; ** indicates statistically significant at the 5% level; *** indicates statistically significant at the 1% level.

Table 5: Probit Regression Results

	(1) First Day	(2) First Week	(3) First Month
Peer group's location choice	0.2333	0.1974**	0.2851***
	(0.1739)	(0.0788)	(0.0438)
New captain's catch history	-	0.0044	0.0206
		(0.0503)	(0.0264)
New captain's relative experience	-	0.5435***	0.6333***
		(0.0491)	(0.0341)
Wind speed weighted by direction	-0.0469**	-0.0035	-0.0015
	(0.0211)	(0.0095)	(0.0056)
Wave height	-0.0373	-0.0651*	-0.0694***
	(0.0721)	(0.0340)	(0.0195)
Wind direction dummies	Yes	Yes	Yes
Captain Fixed Effects	No	No	No
Port Dummy	Yes	Yes	Yes
R-squared	0.14	0.28	0.31
Observations	180	859	2721

Notes: Dependent variable is the location choice of a new captain. Marginal effects reported. Standard errors are in parentheses. Each column represents a different sample. Column (1) restricts the sample to each new captain's first day in the fishery. Column (2) restricts the sample to each new captain's first week in the fishery. Column (3) restricts the sample to each new captain's first month in the fishery. * indicates statistically significant at the 10% level; ** indicates statistically significant at the 5% level; *** indicates statistically significant at the 1% level.

Table 6: First Stage Regression Results

	(1) First Day	(2) First Week	(3) First Month
Peer group's catch history	0.4017***	0.0732	0.1656***
	(0.1277)	(0.0612)	(0.0395)
Peer group's relative experience	0.4656*	0.7337***	0.5517***
	(0.2386)	(0.1373)	(0.0772)
New captain's catch history	-	-0.0064	0.0381*
		(0.0250)	(0.0213)
New captain's relative experience	-	0.0040	0.0197
		(0.0261)	(0.0234)
Wind speed	-0.0093	-0.0082*	-0.0049**
	(0.0066)	(0.0043)	(0.0022)
Wave height	-0.0677***	-0.0511***	-0.0511***
	(0.0253)	(0.0150)	(0.0085)
Wind direction dummies	Yes	Yes	Yes
Captain Fixed Effects	No	Yes	Yes
Port Dummy	Yes	Yes	Yes
R-squared	0.45	0.17	0.20
Observations	180	845	2708
F-stat on Instruments	23.9	21.4	106.31
Partial R-squared of excluded instruments	0.3	0.13	0.14

Notes: Dependent variable is the average location choice of a new captain's peer group. Robust standard errors clustered by captain are in parentheses. Each column represents a different sample. Column (1) restricts the sample to the behavior of the peer group of new captains on each new captain's first day in the fishery. Column (2) restricts the sample to the behavior of the peer group of new captains during each new captain's first week in the fishery. Column (3) restricts the sample to the behavior of the peer group of new captains during each new captain's first month in the fishery. * indicates statistically significant at the 10% level; ** indicates statistically significant at the 5% level; *** indicates statistically significant at the 1% level.

Table 7: Second Stage Regression Results

	(1) First Day	(2) First Week	(3) First Month
Peer group's location choice	0.7335**	0.8871***	0.4960***
	(0.3479)	(0.3059)	(0.1365)
New captain's catch history	-	-0.0037	0.0013
		(0.0441)	(0.0441)
New captain's relative experience	-	-0.1784***	0.1480**
		(0.0426)	(0.0616)
Wind speed	-0.0293*	0.0042	0.0009
	(0.0172)	(0.0092)	(0.0050)
Wave height	0.0184	-0.0310	-0.0620***
	(0.0778)	(0.0366)	(0.0205)
Wind direction dummies	Yes	Yes	Yes
Captain Fixed Effects	No	Yes	Yes
Port Dummy	Yes	Yes	Yes
Observations	180	845	2708

Notes: Dependent variable is the location choice of a new captain. Robust standard errors clustered by captain are in parentheses. Each column represents a different sample. Column (1) restricts the sample to each new captain's first day in the fishery. Column (2) restricts the sample to each new captain's first week in the fishery. Column (3) restricts the sample to each new captain's first month in the fishery. * indicates statistically significant at the 10% level; ** indicates statistically significant at the 5% level; *** indicates statistically significant at the 1% level.

Table 8: IV Probit Results

	(1) First Day	(2) First Week	(3) First Month
Peer group's location choice	0.6892**	0.3887**	0.4114***
	(0.3007)	(0.1645)	(0.0868)
New captain's catch history	-	0.0065	0.0218
		(0.0505)	(0.0264)
New captain's relative experience	-	0.5298***	0.6210***
		(0.0503)	(0.0348)
Wind speed	-0.0414*	-0.0016	-0.0009
	(0.0232)	(0.0096)	(0.0056)
Wave height	0.0140	-0.0531	-0.0634***
	(0.0742)	(0.0352)	(0.0198)
Wind direction dummies	Yes	Yes	Yes
Captain Fixed Effects	No	No	No
Port Dummy	Yes	Yes	Yes
Observations	180	859	2721

Notes: Dependent variable is the location choice of a new captain. Marginal effects reported. Standard errors are in parentheses. Each column represents a different sample. Column (1) restricts the sample to each new captain's first day in the fishery. Column (2) restricts the sample to each new captain's first week in the fishery. Column (3) restricts the sample to each new captain's first month in the fishery. * indicates statistically significant at the 10% level; ** indicates statistically significant at the 5% level; *** indicates statistically significant at the 1% level.

Table 9: Over-Identification Tests

	p-value for Hansen's J test
First Day Sample	0.40
First Two Days Sample	0.29
First Week Sample	0.10
First Month Sample	0.09

Table 10: Summary Statistics for Ordered Sample for First Month

Variable	Obs	Mean	Std. Dev.	Min	Max
New captain's choice	2,329	1.085	0.766	0	3
Peer group's choice	2,329	1.078	0.397	0	3
New captain's catch history	2,329	-0.090	0.234	-1.622	0.770
New captain's relative experience	2,329	0.374	0.194	0	1
Peer group's catch history	2,329	-0.145	0.092	-1.251	0.506
Peer group's relative experience	2,329	0.345	0.074	0	0.667
Wind speed	2,329	4.646	2.171	1.192	12.939
Wave height	2,329	1.714	0.509	0.738	3.950

Table 11: Second Stage Ordered Regression Results

	(1) First Day	(2) First Week	(3) First Month
Peer group's location choice	1.0865**	1.1917**	0.3991**
	(0.4563)	(0.5650)	(0.1798)
New captain's catch history	-	0.0218	-0.0250
		(0.2329)	(0.1426)
New captain's relative experience	-	-0.6606***	0.0375
		(0.2360)	(0.1775)
Wind speed	-0.0095	0.0081	-0.0045
	(0.0393)	(0.0195)	(0.0101)
Wave height	0.2893	0.0992	-0.0888**
	(0.1826)	(0.1234)	(0.0420)
Wind direction dummies	Yes	Yes	Yes
Captain Fixed Effects	No	Yes	Yes
Port Dummy	Yes	Yes	Yes
Observations	156	721	2329
Hansen's J p-value	0.3422	0.8227	0.6423

Notes: Robust standard errors clustered by captain are in parentheses. Dependent variable is the location choice of a new captain. Each column represents a different sample. Column (1) restricts the sample to each new captain's first day in the fishery. Column (2) restricts the sample to each new captain's first week in the fishery. Column (3) restricts the sample to each new captain's first month in the fishery. * indicates statistically significant at the 10% level; ** indicates statistically significant at the 5% level; *** indicates statistically significant at the 1% level.

Table 12: Second Stage Regression Results Assuming Quadratic Peer Effect

	(1) First Day	(2) First Week	(3) First Month
Peer group's location choice	0.9473*	1.1416**	0.5835***
	(0.4955)	(0.4517)	(0.1710)
New captain's catch history	-	0.0025	0.0066
		(0.0438)	(0.0447)
New captain's relative experience	-	-0.1719***	0.1524**
		(0.0444)	(0.0623)
Wind speed	-0.0308*	0.0035	0.0000
	(0.0170)	(0.0094)	(0.0050)
Wave height	-0.0008	-0.0583*	-0.0788***
	(0.0783)	(0.0344)	(0.0201)
Wind direction dummies	Yes	Yes	Yes
Captain Fixed Effects	No	Yes	Yes
Port Dummy	Yes	Yes	Yes
Observations	180	845	2708

Notes: Dependent variable is the location choice of a new captain. Robust standard errors clustered by captain are in parentheses. Each column represents a different sample. Column (1) restricts the sample to each new captain's first day in the fishery. Column (2) restricts the sample to each new captain's first week in the fishery. Column (3) restricts the sample to each new captain's first month in the fishery. * indicates statistically significant at the 10% level; ** indicates statistically significant at the 5% level; *** indicates statistically significant at the 1% level.

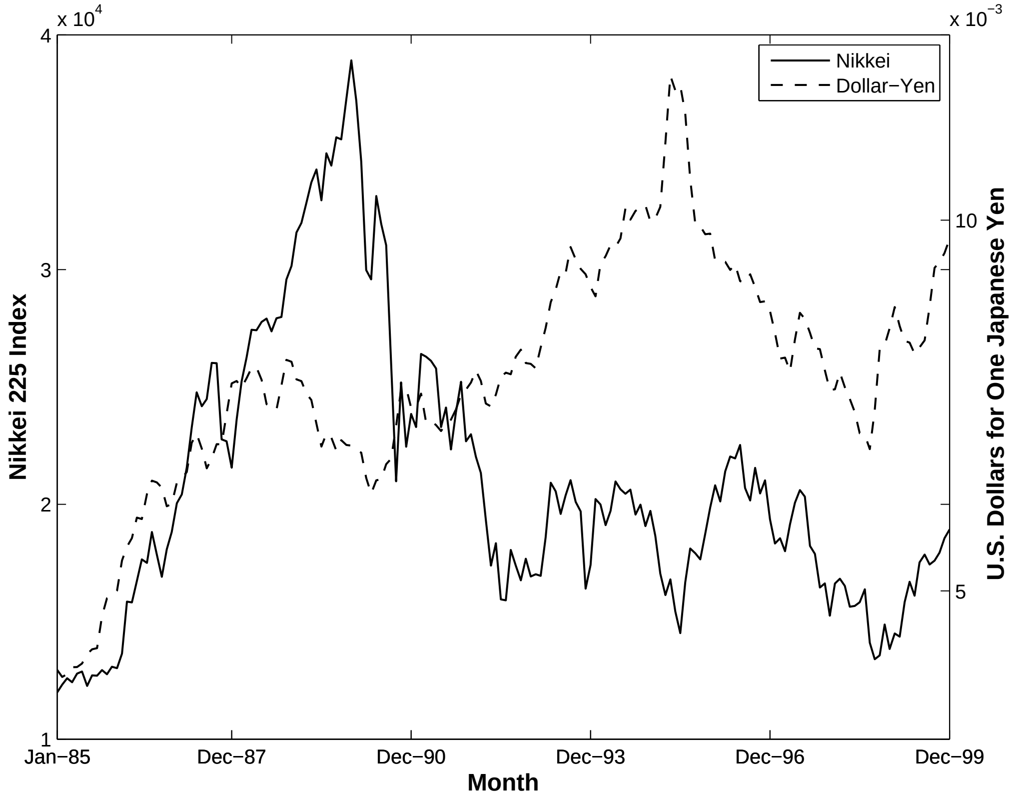
Table 13: Second Stage Regression Results Using "All or Almost All" Peer Effect Variable

	(1) First Day	(2) First Week	(3) First Month
Peer group's location choice	0.6991**	0.6924***	0.4505***
	(0.3149)	(0.2297)	(0.1294)
New captain's catch history	-	0.0073	0.0001
		(0.0409)	(0.0435)
New captain's relative experience	-	-0.1505***	0.1548**
		(0.0456)	(0.0631)
Wind speed	-0.0240	0.0059	0.0001
	(0.0177)	(0.0092)	(0.0051)
Wave height	0.0298	-0.0435	-0.0535**
	(0.0785)	(0.0347)	(0.0219)
Wind direction dummies	Yes	Yes	Yes
Captain Fixed Effects	No	Yes	Yes
Port Dummy	Yes	Yes	Yes
Observations	180	845	2708

Notes: Dependent variable is the location choice of a new captain. Robust standard errors clustered by captain are in parentheses. Each column represents a different sample. Column (1) restricts the sample to each new captain's first day in the fishery. Column (2) restricts the sample to each new captain's first week in the fishery. Column (3) restricts the sample to each new captain's first month in the fishery. * indicates statistically significant at the 10% level; ** indicates statistically significant at the 5% level; *** indicates statistically significant at the 1% level.

Figures

Figure 1: Nikkei Index and Dollar-Yen Exchange Rate

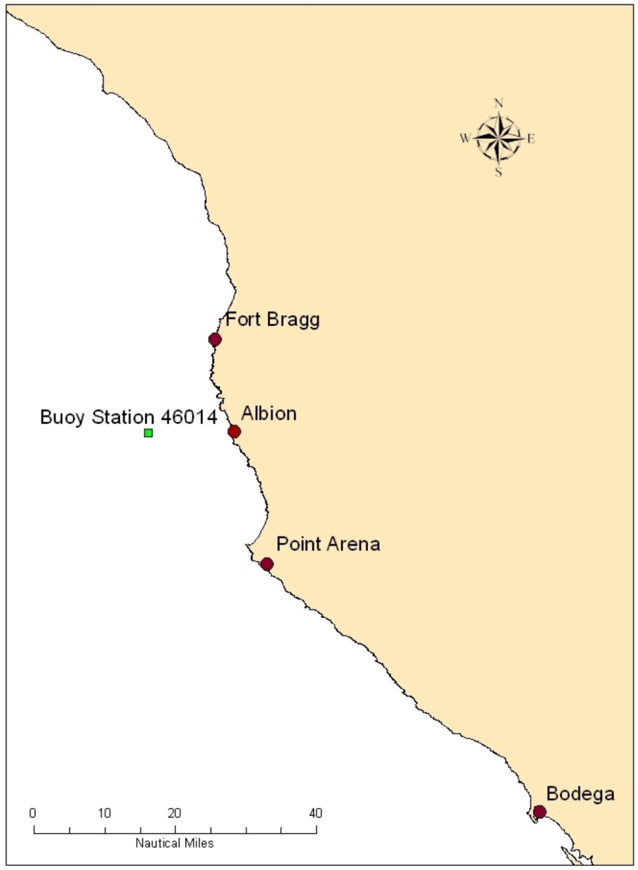


Notes: Solid line indicates the monthly average closing price for the Nikkei 225 stock index, dashed line is the monthly average US Dollars for one Japanese Yen exchange rate Yen (Sources: Yahoo! Finance and Federal Reserve Bank of St. Louis).

Figure 2: The "Blue Water Gold Rush" in Northern California

Notes: The dashed line shows the total number of boats fishing in the Northern California sea urchin fishery each year from 1988 to 1999. The solid line shows the number of new entrants to the fishery each year.

Figure 3: Map of the Study Area



Notes: Map showing the major urchin ports in Northern California and the location of Buoy Station 46014. Bodega is located approximately 70 miles north of the city of San Francisco.

Figure 4: Histogram of Peer Group's Location Choice

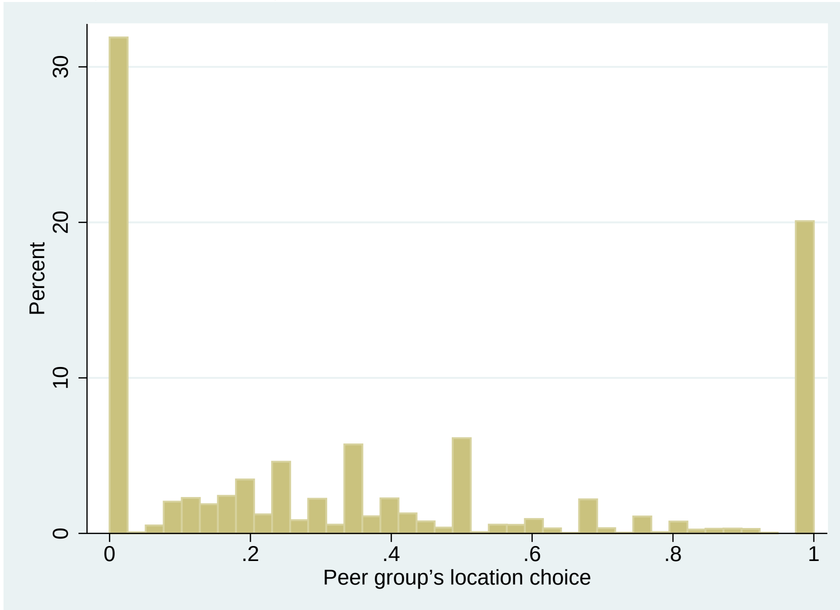
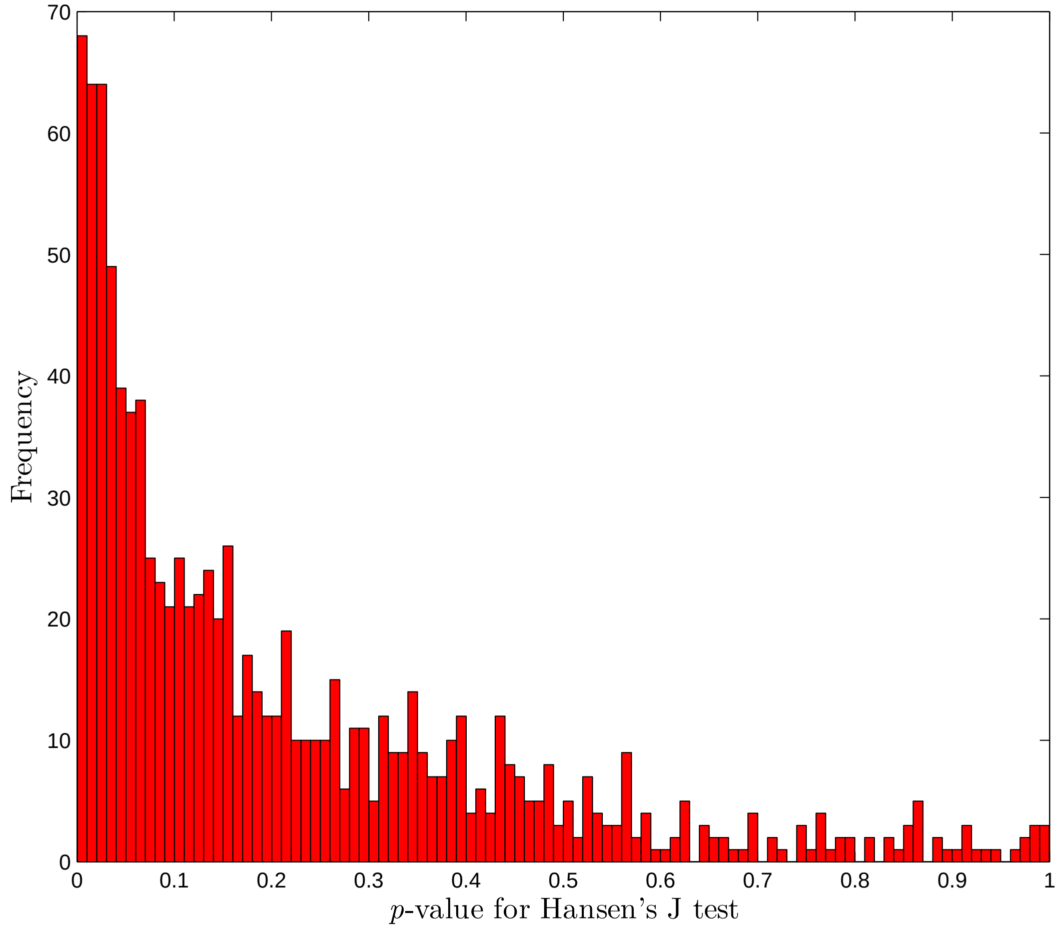


Figure 5: Histogram of p-values for Bootstrapped Samples



Notes

[1]In the words of Moffitt (2001, p. 49) : "Interventions that forcibly reassign group membership or that offer inducements to do so voluntarily alter the composition of groups without changing the fundamentals for any individuals and hence can identify the existence of social interactions by whether the population outcomes within groups are affected."

[2]Many resource extraction decisions are akin to an investment under uncertainty decision. No one extractor knows with certainty the best location to go to but makes a decision based on their information set. Thus, location choices indirectly reveal individual information sets. Extractors can learn by observing the location choices of others and this, in turn, influences their own location choices.

[3]There is a considerable literature in fisheries economics on search behavior and location choice but this literature has not focused on peer interactions (Eales and Wilen, 1986; Mistiaen and Strand, 2000; Smith, 2000; Marcoul and Weninger, 2008). van Putten et al. (2012) provide an excellent overview of the literature on fishing fleet dynamics and location choice. Some recent papers have studied how fishers respond to the behavior of others, but not in the sense of a true peer learning effect. Haynie et al. (2009) study coercion and cooperative behavior among peers to avoid by-catch hotspots. Abbott and Wilen (2011) find evidence that captains are aware of the byatch externality they impose on their peers and take steps to reduce this. A recent paper by Hicks et al. (2012) examines responses to peer behavior but within the context of "safety in numbers" or congestion effects, not in terms of learning.

[4]To date, the only published study using a reduced from approach that we are aware of is Lin (2009). Lin analyzes whether the exploration decisions made by the owner of an oil tract depend on the decisions of firms owning neighboring oil tracts. Her approach exploits an unusual feature of federal oil lease sales (5 year deadlines) to overcome endogeneity concerns but she does not find any peer effects.

[5] There were no binding limits on entering the fishery during this period.

[6]Mattison et al. (1976) estimate that red sea urchins within a kelp forest move on average 7.5cm per day. Dean et al. (1984) find that aggregations of red sea urchins, if they are mobile, move about 2 meters per month. Harrold and Reed (1985) find similarly slow rates of movement within kelp forests but observed movement rates as high as 80cm per day at barren sites. Urchins in barren sites tend to be of little interest to commercial divers because of their poor roe content. Congestion of boats at a particular location is not a major concern. Boats are small (22--29 feet) and can easily set up next to each other and fish the same reef or kelp bed. However, in theory, any location can become too congested and it is important to note that we do not specifically address this potential concern in our analysis. See work by Timmins and Murdock (2007); Bayer and Timmins (2005, 2007); Hicks et al. (2012) on how to incorporate congestion effects into

travel cost and locational sorting models.

[7]As an illustrative example: in 2006, one urchin diver in California landed a total of 92,993 lb. in the year for a cumulative value of \$75,506 while in the same year, another diver landed over twice as many urchins (201,762 lbs) but earned less money (\$63,097).

[8]In particular, new captains have strong incentives to pay attention to experienced captains since seasoned captains are more productive than new entrants. A regression of catch value on experience, including appropriate controls, produces a positive and statistically significant coefficient for the experience variable. As an illustrative example the mean catch value for captains with less than 100 days of experience is \$608.82 whereas the mean catch value for captains with more than 100 days of experience is \$659.79 ($p<0.001$).

[9]To illustrate this point, the average catch for experienced captains in our sample is \$598.48 if they go south and \$611.44 if they go north ($p=0.11$ on a t-test comparing means).

[10]Precision is the inverse of variance.

[11]The Gaussian family of distributions is conjugate to itself: a Gaussian distributed prior updated on a Gaussian distributed signal with a known variance gives a Gaussian distributed posterior.

[12]It is important to note that we have deliberately chosen the precision of the catch value signal so that each captain's posterior belief is essentially a 1 month backward looking average. This is done to match the work of Smith and Wilen (2003, p. 192): "Expected catch is a patch-specific rolling 1-month backward looking average. These proved to be best fit in our preliminary specification tests." The precise value of $r_{i,t}$ doesn't actually matter since it drops out of the formula for the captain's posterior belief; we could have chosen any value greater than zero and the updated beliefs would be exactly the same. For a discussion of the various approaches to expectations formations in fisheries models and the presentation of a more nuanced model that allows for information sharing see Abbott and Wilen (2011).

[13]We used Station 46014 which is located at 39.22° N, 123.97° W, close to the main urchin harvest areas (see Figure 3).

[14] Note that we are assuming that each new entrant treats all the members of the group of existing captains the same. It could be the case that new entrants pay more attention to some peers than others (such as highliners) and this would make for an interesting extension of our analysis.

[15]We introduce fixed effects to allow for the possibility of correlation between any time-invariant unobserved effects and the observed explanatory variables. For example, unobserved preferences over the two locations are likely to be correlated with a captain's relative experience at the two locations.

[16]A week is defined as four days since the fishery is generally closed for three days a week in the summer. A month is defined as twenty days.

[17]It's also important to note that maximum likelihood estimates from a nonlinear fixed effects model

with small T and large n (a few days of observations and a large number of fishermen as is the case in this paper) are biased and inconsistent (Greene, 2004). Solutions to this problem have been proposed (see, for example, the Chamberlain (1980) proposal to use random effects) but we follow Greene's advice that "ignoring heterogeneity (in a probit model) is not necessarily worse than using the fixed effects estimator to account for it. But using the random effects estimator is worse" Greene (2004, p. 100).

[18]We define the "right choice" as going to the location with higher mean catch value for experienced captains on that day. This is obviously a very crude measure of which location is the better choice.

[19]The estimated standard errors have been adjusted to allow for potential heteroskedasticity and within-captain correlation. For example, error terms could be larger on new captains' first days compared to their third days and certain captains could have larger error terms on average than others. The variance-covariance matrix is adjusted using the weights proposed by Rogers (1993), which is based on Huber (1967).

[20]We also report the results from estimating our model for a new captain's first two days.

[21]As a further robustness check, we tested our assumption that existing captains ignore new captains by running a regression of the peer group's choice on the choice of the new captains. Identification is only possible for the one day sample and we find that the estimated peer effect is extremely small (0.007) and not statistically different from zero. This supports our identification strategy but it should be noted that the failure to reject the null may be due to a lack of statistical power.

[22]As an illustrative example, a catch of $1,000 in the far south gets a value of -1,000 whereas the same catch in the near south gets a value of -500. The intuition is that the more negative the captain's belief, the more likely he is to go far south (0) instead of near south (1) and the the more positive his belief, the more likely he is to go far north (4) instead of near north (3).

[23]Despite the rising popularity of reduced-form approaches, it is important to emphasize that Bayesian techniques have been less frequently utilized in the economic analysis of fisher behavior than might be appropriate.