Evaluating the Effects of Stream Restorations

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Introduction

Rivers and streams are vital assets for human societies because of the range of goods and services they deliver, e.g., drinking water, food and energy production, but also recreational opportunities etc. In many countries, the natural course of rivers and streams has been modified to provide flood protection, to facilitate the production of electricity or agricultural products, and in many instances these modifications have impaired the functioning of ecosystems thus reduced the availability of goods and services.

Restorations have been undertaken in many countries such as Australia, Canada, China, Europe, the U.S., etc. (Jenkinson et al., 2006; Palmer et al., 2014). In the U.S., for example, thousands of restoration projects are undertaken annually costing over $1 billion per year to return rivers and streams to a more natural state (Bernhardt et al., 2005 and 2007). Common restoration goals are undertaken to improve in-stream habitat conditions, river morphology, lateral or longitudinal connectivity and water quality so that to enhance the ecological, recreational and aesthetic potential of streams. However, despite the considerable efforts devoted to restorations, there is still no substantial empirical evidence that they increase the availability of ecosystem services (Pretty et al., 2003; Lepori et al., 2005; Palmer and Filoso, 2009; Palmer et al., 2010). Furthermore, it is not clear which types of interventions are most likely to lead to a restoration of ecosystem services.

This lack of empirical evidence in the face of such consistent monetary investments raises the need to investigate quantitatively a) whether, and in what dimensions, restoration efforts are successful and b) which are the most important contributors for the success. Our main focus is on a) in this paper.

There are several metrics that can be used in ecology to evaluate restoration effectiveness (Woolsey et al., 2007; Palmer et al., 2014). However, many of them require information that is based on indicators that are not easy to measure or that do not have a direct link to human welfare. Another way to address the success of restoration efforts is by investigating empirically the effects on ecosystem services that are objectively measurable such as commercially or recreationally important fish species like salmons or brown trout (Palmer, 2008; Roni et al., 2008; Palmer and Filoso, 2009). Because fish can be considered as proxies of the environmental status of an aquatic ecosystem, observing the status of fish communities may provide an indication of the health of the ecosystem. Also, fish are related to several ecosystem services such as regulating services, cultural (recreational), and food provision (Holmlund and Hammer, 1999).

In this paper, we propose a rigorous assessment of the impact of rehabilitations and restorations of rivers and streams on recreational fishing, which is a well-known ecosystem service. This question is addressed by investigating whether there is any statistical evidence that river restorations led to significant effects on outcomes related to recreational fishing, specifically, whether restorations had any effect on catch and the number of fishing trips in streams and rivers.

Our analysis is based on an empirical application to recreational fishing on the streams in the canton of Graubünden, Switzerland. One of the most difficult aspects for evaluation of these projects is the data requirements are quite demanding including the need for historical stream characteristics and a measure of stream ecosystem change. For this region, we have historical data on river restorations, eco-morphology and connectivity, individual fishing trips including catch by species, and stocking all at the stream section level, along with demographics for the anglers and regulations of recreational fishing over time in the period 2002-2012. Detailed documentation on the restorations, e.g., length of the restoration and the exact details of what was done, allows us to observe restorations of river sections over time and across space and to perform a refined investigation of the temporal and spatial heterogeneity of rivers and recreational fishing in the region. We utilize the stream data to first establish the relationship between restoration events and trip-level catch totals by species and size. Through both traditional panel models and an alternative panel evaluation utilizing matching to preprocess the data controlling for potential selection effects based on pre-treatment stream attributes we find rather consistent evidence that the catch rates increase downstream from restoration events and, in fact, the caught fish are larger on average than their counterfactuals.

In a future step of the analysis we will able to utilize an actual shift in demand produced by the catch to measure the welfare effects of stream restorations giving a novel measure for a human induced environmental improvement. Further with recently compiled information at the restoration level we expect to be able to classify and assess which “types” of restoration are most productive in regards in catch rates. This paper provides initial empirical evidence of the effects of restorations of streams in the literature. It is also, arguably “one of the’ if not “the” only documented short run increases in catch from a human made attempt to restore ecosystem function outside of simply removing the human presence altogether.

Empirical application

Our aim is to determine the effects of rivers and streams restorations on a measurable ecosystem service, i.e., recreational fishing. To do so, we exploit a unique and rich compilation of data that include observable historical stream characteristics, detailed information on stream restorations, and anglers' characteristics that include repeated pre-and post-treatment outcomes. We estimate a series of panel estimators some of which combine panel data and matching methods to pre-process the data and make more similar the treated and untreated stream sections pre-treatment average outcome and the distributions of their baseline (time-invariant) covariates. The pre-processing also serves tomitigate concerns about potential heterogeneity of the treatment effect. In other words, because the treatment, the restoration, is not randomly assigned to the stream sections, we want to make sure that the treatment and the comparison groups are observationally similar in the period prior to treatment and thus increase the confidence in the underlying assumptions of the fixed effect panel estimators (Miranda and Ferraro, 2014).

Our data come from mainly two sources: 1) geocoded data on eco-morphological characteristics of rivers and streams and 2) a virtual census of recreational fishers, anglers, in one of the largest cantons in Switzerland.[1]

Recreational fishing is an important activity in Switzerland. About 240,000 persons practice angling at least once a year, and in total spend around 150 million EURO per year (Burkhardt-Holm et al., 2002). Despite stocking efforts, trout catches from Swiss rivers and streams have declined by as much as 50% since the 1980s possibly due to a combination of causes such as water temperature, inadequate management of fisheries, altered hydrological regime, and poor morphological quality of rivers and streams (Burkhardt-Holm et al., 2002, 2005). In Switzerland, about 50% of streams and rivers below an altitude of less than 600 m above sea level are considered to have poor habitat quality and 22% of the Swiss rivers are in a bad eco-morphological state (Zeh Weissmann et al., 2009) and it is suggested that about 80,000

[1] A canton in Switzerland can be interpreted as a state in the U.S..

artificial structures should be removed to improve fish migration (Notter et al., 2007; Peter et al., 2008).

This analysis focuses on the Swiss canton of Graubünden (about twice the size of Rhode Island) that is primarily mountainous and rural in character and is a destination for recreational fishing. It contains 11,000 km of streams and over 600 lakes and ponds.[2] Within this geographical region we have a large amount of data for both the water infrastructure (streams and lakes) and anglers. For the streams we have a unique and broad set of ecosystem data including eco-morphology that is classified by an index for the degree of naturalness at the stream section level covering both the in-stream and stream banks. We also have detailed data on the degree of developed infrastructure near and in the stream that may limit the connectivity of stream segments; we have weather data, restoration data, and fish stocking data.[3] We also have a vast amount of angler data such as fishing locations, angler origination, species caught, size, and restoration data in time and space for the region. These data comprise ~1.2 million trips from ~25,000 fisherman fishing ~1.45 million specific locations. These anglers catch ~100,000 fish annually and trout is overwhelmingly the target fish accounting for over 70% of the catch. Trout is the overwhelming target species in the area especially in streams. The data covers the period 2002-2012.

Econometric strategy

We are primarily interested in determining the impact of stream restorations on fish populations as measured from the catch of the census of anglers from canton Graubünden. Restorations affect catch through their effect on fish stock. In the long run, returning the habitat to a more natural

[2] http://www.gr.ch/IT/cantone/panoramica/Seiten/JagdundFischerei.aspx last accessed 1/14/15.
[3] Weather and restoration specific data are not included in this paper as the German translation is in process.

state should restore the ecological processes that favor the growth and survival of fish and thus increase fish stock. All else equal, higher fish stock should yield higher catch. In the short run, restorations may affect fish movement between stream sections, thus changing the stock density, or biomass, in the section, and so increasing catch rates.

We are also concerned with possible site selection issues related to these restorations. Restorations may alter the choice of fishing site, the section, by anglers through their effect on the appearance of the fishing site or the effect on water quality. To break this direct link between restorations and the site choice we define as treated sections the fishing sections that are immediately downstream from a restored fishing section. In this way, restorations affect the anglers' choice of fishing site, and thus the number of fishing trips to each site, *only* via catch rate for the given section.

Another issue related to site selection regards the choice about which section gets to be restored. From 2011, by law, Swiss cantons are required to rank planned restorations based on the eco-morphological status, ecological and landscaping relevance, risk of flooding, with precedence given to restorations yielding benefits from ecological and landscape improvements potentially higher than costs.. Before this law was passed, the selection of the restoration sites was not based on any fixed criteria, but rather on the bases of a more political and economic feasibility. At any rate, the ranking on the planned restorations is not directly related to recreational fishing. Nevertheless, it is plausible to think that in the time before (and after) the law was implemented, some of the determinants of the choice of the restoration site may have been correlated with the fish stock in the section itself. Hence, by simply comparing catch rates in restored and unrestored sections would not avoid the bias due to the factors affecting both the likelihood to restore the section and the fish stock in the section. If notoriously poor habitat is selected for restoration then the proper comparison groups are those with equal compromised habitat but void of restoration.

Upstream restoration activity is linked with downstream fish population invalidating any strategy using characteristics of upstream streams as instruments. On the other hand, the propensity score matching method relies on the existence of the relationship between upstream stream health (and subsequent restorations) and downstream fish populations but non-identifiably of an instrument that directly is related to restoration but not catch. Our rich data on streams combined with the ability to difference our outcome variable of interest make the propensity score matching method a useful standalone for examining outcomes aggregated to the stream section. However, one drawback of the propensity score method is the cross-sectional nature of the estimator when one has panel data. In our case we have extremely detailed angler data for the decade from 2002-2012.

The traditional approach with this type of panel data is to assume that the fixed portion of the panel addresses the selection issues inherent with restoration site selection. That is, after controlling for the time variant characteristics (e.g., lagged aggregate catch, stocking data, weather, etc.), and having comparison streams from the same local environment (economic and socio-demographic) any systematic differences between treated and untreated units are thus assumed to be captured by time-invariant characteristics whether these characteristics be observed or not. The fixed effect model assumes the expected trajectories of the treatment and control groups are the same in the absence of the treatment, an assumption of homogeneous treatment effects. However as suggested by Ferraro and Miranda (2014a,b) if treated and untreated stream sections are exposed to cotemporaneous, post-treatment shocks, the assumption of homogenous average responses among treated and untreated units is questionable. In our data we have a precipitous decline in angler activity during the study period unattributed to any particular observable data. If this or any other “shock” impacts the treated streams disproportionately then the homogeneity assumption of the treatment effect is in question. By combining the advantages of the propensity score estimator to construct observationally equivalent comparison groups and then the sample isexposed to a post-treatment shock, the assumption of homogenous average responses among treated and untreated units is more plausible (e.g., if stream attributes are correlated with exposure and responses to stimuli that are causing declining angler interest). Moreover, if the important sources of treatment heterogeneity are functions of observable characteristics, matching and reweighting the sample can render the homogenous treatment effect assumption less problematic.

To estimate a model with a stricter interpretation of the homogeneous treatment we use matching algorithms to pre-process the data and make the treatment and comparison groups observationally similar prior to treatment assignment (Miranda and Ferraro, 2014a, 2014b). We refer to the occurrence of an upstream restoration as the treatment and we present two comparable panel data econometric approaches in this paper. The first, using a fixed effect panel approach, has obvious tractability advantages in this context and the second approach estimates the same models having reweighted the sample based on the propensity score analysis.

In all time periods of the analysis, anglers choose among fishing sites and complete a report documenting where they fish and the number of catch in multiple size categories by species (figure 1).[4] The panel models are based on the combination of angler / fishing location. So one angler’s inherent skill is effectively allowed to vary by location. There is no clear guidance on the modeling if time variation in these types of models so we choose a highly

[4] Importantly, they also report zero catch.

flexible year by quarter time dummy.. These models incorporate non-parametrically the impact of unobserved time-varying covariates that account for the dynamics of angler activity.

To formalize the model we estimate the following equation:

(1) $$C_{isd} = \alpha_q + \mathbf{W}_{isd}\beta + D_{isd} \cdot \gamma + \varepsilon_{isd} \, ,$$

where C is the catch by angler *i* in stream section *s* on day *d*, α_q denotes a sequential quarterfixed effect $\mathbf{W}_{isd}$ is a vector of time variant controls (weather, stocking data, aggregate past catch, and D_{isd} is the treatment dummy equal to 1 if a stream restoration occurred in the most direct upstream section at any time in the past. We are most interested in estimating γ, the average treatment effect on the treated (ATT).

The quarter fixed effects are one approach to capturing the shocks represented by changing attitudes toward recreational fishing and the spatial location or locale desirability is absorbed by the angler/spatial fixed effect. The effect of the treatment is identified by variation within the stream-quarter (year) section cell. We estimate 3 panel models – a logit model for catching any fish, a count model for catch number, and an interval regression to discern the impact of *D* on the size (length) of the average caught fish.

As anticipated, there exist that plausible circumstances that may induce heterogeneity in the treatment effect. To account for this, we also preprocess the data using propensity score matching in an attempt to restore balance on observables and near or plausible homogeneity of the treatment effect. In this framework, we wish address selection issues, measure the macro effect where the observation of interest is the stream section and to further utilize the resulting estimation weights to preprocess a second set of panel models. The *treatment, D,* is again the presence of a stream restoration upstream (in the nearest upstream section) to the observation, $D \in \{0,1\}$. The *outcome* of interest, *Y*, is the aggregate yearly catch in the stream section.

The main advantages of the matching procedure includes removing sensitivity to functional form in constructing the counterfactual,[i] exposing violations of the common support (cases where treated observations are substantially different from untreated observations), and, as pointed out by Rubin (1997), in cases with many confounding variables, making clear that "small differences in many covariates can accumulate into a substantial overall difference" such that groups of stream sections may differ in a multivariate direction to an extent that cannot be discerned from comparisons of means or histograms between groups. The use of the matching weights to preprocess our panel data will most obviously benefit from the identification and exclusion of stream sections without comparable counterfactuals (violation of the common support).

Here we draw on a class of estimators called propensity score matching (PSM) estimators, first suggested by Rosenbaum and Rubin (1985). Applications of propensity score matching are now quite prevalent in the literature, especially in labor economics where the evaluation of job-training programs represents a significant econometric challenge (e.g., Smith and Todd, 2005; Dehejia and Wahba, 2003). Before explaining the specifics of our own application, we lay out the general form of the matching estimation procedure following such standard references as Heckman, Ichimura, and Todd (1997); Heckman, Ichimura, Smith, and Todd (1998); and Smith and Todd (2005).

In general, let N be the number of stream sections at the beginning of the time period, the year 2002/2003 in our data. Over the next 7 years we observe restoration activity in the landscape. Those stream sections downstream from the restoration activity are the treated sections. In any period, T, any number of these stream sections, N_D are treated. Outcomes emerge in some period subsequent to T discussed in detail below. Stream sections can experience treatments prior to the sample period and we are careful not to contaminate in the analysis by removing any sections treated in the past, from both the treated and control sections (Holland, 1986).

Following common notation, we define Y_1^c as the fish catch outcome under treatment and Y_0^c as the catch outcome with no treatment. For any stream section, only one of these outcomes can be observed, D = 1 indicating that a section has been treated and D = 0 indicating the untreated state. $\boldsymbol{Z}$ is a vector of K conditioning variables comprised of the ecomorphology of the upstream streambed, stream banks, instream infrastructure, and stream bank infrastructure, as listed in table 1. These are variables that we expect will affect the probability of treatment and variables that can be expected to affect the outcome (the catch totals). To further address the actual stream characteristics of the section we difference the outcome variable such that Y is the difference in catch in 2012 and the average of the 2002 and 2003 years. Since the precise location of fishing within a section is indeterminable the actual own stream section data is not useful, however, given there are no changes in own section exogenous characteristics the differencing effectively mitigates this source of bias.

The usual task set out by propensity score matching procedures is to estimate the mean "treatment effect on the treated." For our problem, this is the effect on the catch rate averaged over all sections that were treated. Specifically, we want an estimate of

$$ATT = E(Y_1^c - Y_0^c \mid \boldsymbol{Z}, D = 1) = E(Y_1^c \mid \boldsymbol{Z}, D = 1) - E(Y_0^c \mid \boldsymbol{Z}, D = 1) \qquad (2)$$

where ATT is the average treatment effect on the treated. This equals the expected value of the difference between the treated outcome and the non-treated outcome, conditional on exogenous explanatory factors, $\boldsymbol{Z}$, for the group of sections that are actually treated. The first term in the last expression in (2) is easily obtained, as it is the average actual outcome for the treated observations—in our case, the catch totals in the treated sections. However, the second term, representing the counterfactual or potential outcome, is never observed. It is the expected outcome for the treated observations *had they not been treated*. The task is to define an estimator for $E(Y_0^c \mid \boldsymbol{Z}, D = 1)$.

Matching estimators' pair each treated observation with one or more observationally similar non-treated observation(s), using the conditioning variables, $\boldsymbol{Z}$, to identify the similarity. This procedure is justified if it can be argued that conditional on these $\boldsymbol{Z}$'s, outcomes are independent of the selection process. That is, if those observations found in the set $D = 0$ were actually treated, the expected value of their outcomes, once conditioned on the $\boldsymbol{Z}$'s, would not differ from the expected value of outcomes in the treated group.

More precisely, conditional mean independence is required, such that

$$E(Y_0^c \mid \boldsymbol{Z}, D = 1) = E(Y_0^c \mid \boldsymbol{Z}, D = 0) \qquad (3)$$

Direct implementation of the above equation would be difficult for a large number of conditioning variables, yet ensuring that (3) holds would typically require a rich set of these variables. Rosenbaum and Rubin defined the propensity score matching estimator by showing that instead of conditioning on all K elements of the $\boldsymbol{Z}$ vector, one can equivalently condition on a one-dimensional function of that vector. They show that if outcome Y_0^c is independent of selection when conditioned on the $\boldsymbol{Z}$'s, then it is also independent of selection when conditioned on the "propensity score," which is defined as the probability of selection conditioned on the $\boldsymbol{Z}$'s. Defining

$$P(\boldsymbol{Z}) = Pr(p = 1 \mid \boldsymbol{Z}) \qquad (4)$$

the treatment effect in equation (2) combined with equation (3) can now be rewritten as:

$$ATT = E(Y_1^c - Y_0^c \mid P(\mathbf{Z}), D = 1) = E(Y_1^c \mid P(\mathbf{Z}), D = 1) - E(Y_0^c \mid P(\mathbf{Z}), D = 0)$$

(5)

Equation (4) is estimated as a binary probit, with treatment, *strRest*, as the dependent variable.

Having established the grounds for matching, we need to define the form of the matching estimator for the treatment effect in equation (5). We use nearest neighbor matching, which is a pairwise matching scheme that selects the counterfactual from the untreated set on the basis of the most similar propensity score. For simplicity we implement 1 nearest neighbor which facilitates the reweighting of the panel estimators.

Once having obtained the propensity scores shown in table 2, the common support condition can be examined. As shown in Table 3 in the row labeled "On Support/Off Support", we find several instances where the common support is violated, and all treated effects are reported excluding these violators. Additionally, before calculating any of the average treatment effects on the treated, *ATT*, the outcome must be shown to be mean independent of the treatment, conditional on the propensity score. Given the conditional independence assumption set out in equation (3) above, this requires ensuring that the covariates in ***Z*** meet this condition, which is equivalent to achieving "balance" between treatments and their controls. In more general terms, balancing ensures that covariates in ***Z*** cannot be used to predict membership in the treatment or control group—that is, the ideal situation of a random assignment has been recreated. We implement the balancing tests using mean independence, and all specifications balance on all covariates and interaction terms (when necessary) at a significance of 5% where interactions are included following Dehejia and Wahba (2003) table 4 displays these results and show that 9 of the conditioning variables go from mean statistical difference to zero statistical difference.

Results

Utilizing the full panel results we see an increase in the probability of catching *any* fish, an increase in the number of fish caught, and a slight decrease in the size of the caught fish. Table 5 shows these results and marginal effects. Further a comparison between the total catch in Figure 2 panels 1 and 3 (reading from left to right) suggest the potential baseline "unobserved" differences in catch across the treated and untreated sections. If the assumption of homogeneity in treatment holds then the area by angler fixed effect is adequately capturing this variation and one can be satisfied that restorations seem to have small but measureable impacts. However, if there is any reason to believe that heterogeneity exists in the treatment effect due in part to unobserved time variant shocks that impact fishing areas heterogeneously then one should be concerned with preexisting observable differences in these areas that when controlled for the homogeneity assumptions become more econometrically palatable.

The propensity score results are useful for two aspects – first they provide an overarching aggregate impact measure of interest in general and secondly the resulting weights from the matching algorithm allow the panel to be preprocessed such that homogeneity assumptions more plausibly hold. Table 2 shows the estimation of the treatment based on upstream stream attributes. While interpretation is minimally important it is positive that the pre-period stream data are suggestive of location of the restoration activity. We utilize a differenced dependent variable equal to the 2012 total catch minus the average of the 2002-03 total catch.

$$ATT = \left[Y^c_{1,2012} - (Y^c_{1,2002} + Y^c_{1,2003})/2\right] - \left[Y^c_{0,2012} - (Y^c_{0,2002} + Y^c_{0,2003})/2\right].$$

Table 3 out shows the single nearest neighbor results for balanced specifications with bootstrapped standard errors. In the aggregate the restorations seem to increase the total catch, by 277 (significant at 95%), and the total trout catch by 242 with a large portion of this average impact coming from larger fish (> 34cm), 191 and 158 for total and trout catch respectively both significant at 95%. While the design of the treatment is intended to minimize the “attraction” to restorations by anglers we do see slight evidence of an increase in trips of 140 per year significant at 90%, but this increase in trips is more than offset by an increase in catch per trip by almost 0.30 significant at 95%. These aggregate numbers are suggestive of positive treatment impacts but assume perhaps implausible restrictions on angler behavior that we address by using the full panel preprocessed with the propensity score weights.

The panel is defined on angler location so any angler “skill” is differentiated by site. The results presented in Table 6 from the fixed effects logit suggest an increase in the odds of a catch post treatment of 4 to 5%, the count models suggest an insignificant increase in the expected number caught and a statistically significant slight decrease in trout caught, and the interval regression suggests an increase of .22 cm for all catch and .26 cm for trout both significant. These panel results contrast rather dramatically with the preliminary panel analysis in both number and size, reversing, in several cases, these results.

Concluding remarks

Little evidence of direct links between human ecosystem restoration and measurable species recovery exist yet human reversing poor past decisions regarding ecosystem in general and streams specifically has recently been a focus of scientists and policy makers. This paper utilizes a unique set of data essentially a fishing census in an area of active stream restoration activity to demonstrate small but important impacts on fish catch and size that are at worst suggestive that these interventions are experiencing meaningful short term success.

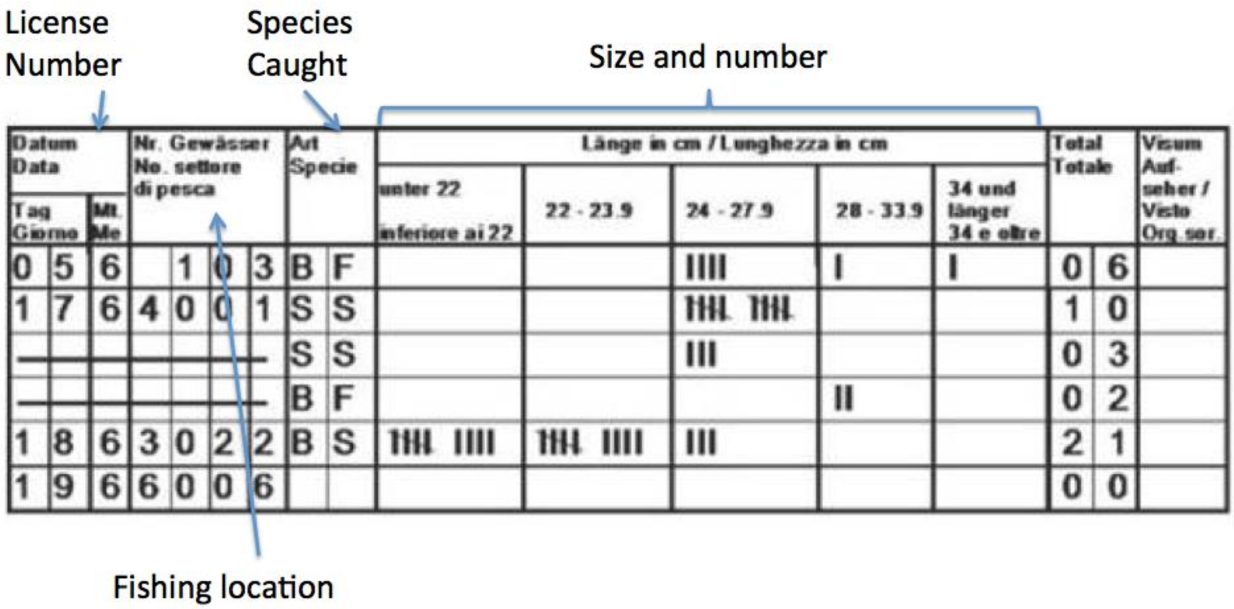

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Figure 1. Fishing report card that each angler has to fill out and submit to the local authority.

Figure 2 Catch totals by sample – treated, PSM weighted controls, controls

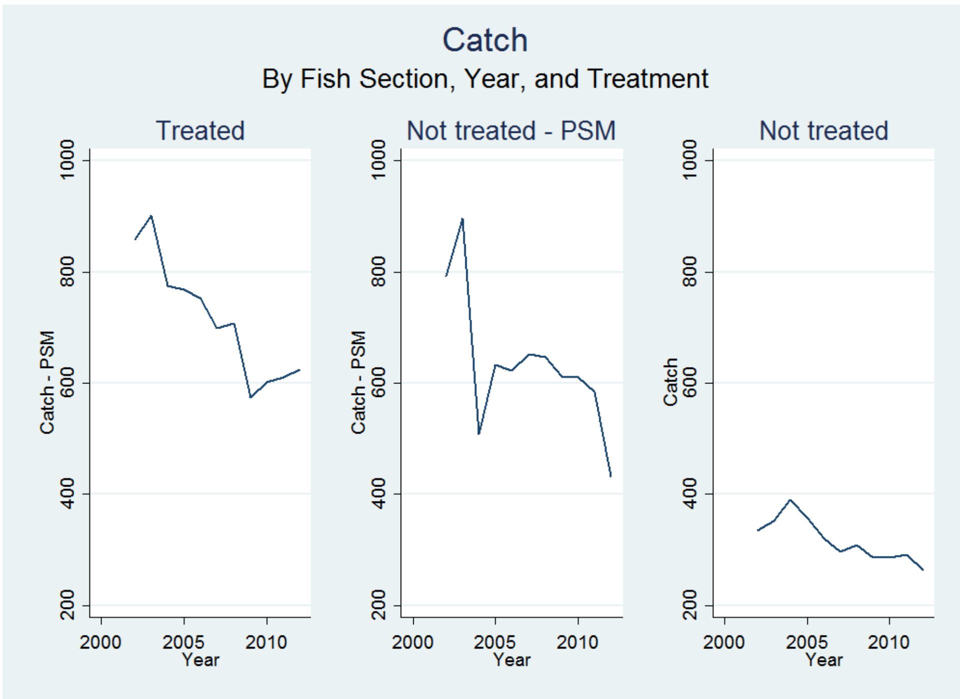

Table 1 Variables used in the construction of the propensity score

Variable	Definition	Mean	SD	Min	Max
pctUp_oeko_4	Percentage of upstream geomorphology non-natural	0.0621	0.2208	0	1
pctUp_oeko_2	Percentage of upstream geomorphology slightly impaired	0.1395	0.3019	0	1
pctUp_oeko_3	Percentage of upstream geomorphology severely impaired	0.1448	0.3087	0	1
pctUp_FUSS_DCH	Percentage of streambank permeable (connected)	0.3995	0.4235	0	1
pctUp_FUSS_BAU	Percentage of streambank impervious	20.1475	30.9835	0	100
pctUp_UFER_BR	Percentage of streambank sufficient width	0.3645	0.4231	0	1
pctUp_UFER_BE_1	Percentage of shore area impervious	0.0603	0.1990	0	1
pctUp_UFER_BE_2	Percentage of shore area connecting streams	0.4223	0.4518	0	1
pctUp_UFER_BE_3	Percentage with no shore area	0.0196	0.1054	0	0.931
pctUp_UFER_BE_4	Percentage of shore area artificial	0.0792	0.2293	0	1
pctUp_SOHLE_PROZ	Percentage of streambed containing construction (weirs, dams, bridges)	1.3353	7.5326	0	85.88
pctUp_WSPIEGLB_V_1	Percentage of stream width with no restrictions	0.2850	0.4095	0	1
pctUp_WSPIEGLB_V_2	Percentage of stream width with partial restriction	0.1911	0.3406	0	1
pctUp_WSPIEGLB_V_3	Percentage of stream width with full restriction	0.1053	0.2622	0	1
anyUpSohle	Dummy for upstream construction	0.4760	0.8279	0	5

Table 2.

Variable	Coefficient	Robust SE	T-Stat	pval
pctUp_oeko_4	-4.278	1.945	-2.200	0.028
pctUp_oeko_2	-0.992	0.927	-1.070	0.284
pctUp_oeko_3	-0.793	1.443	-0.550	0.583
pctUp_FUSS_DCH	-0.361	0.515	-0.700	0.483
pctUp_FUSS_BAU	-0.001	0.008	-0.130	0.893
pctUp_UFER_BR	-0.691	0.639	-1.080	0.280
pctUp_UFER_BE_1	5.394	1.955	2.760	0.006
pctUp_UFER_BE_2	5.539	1.605	3.450	0.001
pctUp_UFER_BE_3	4.730	2.024	2.340	0.019
pctUp_UFER_BE_4	4.964	1.735	2.860	0.004
pctUp_SOHLE_PROZ	-0.015	0.030	-0.490	0.624
pctUp_WSPIEGLB_V_1	-3.671	1.257	-2.920	0.004
pctUp_WSPIEGLB_V_2	-2.730	1.019	-2.680	0.007
anyUpSohle	0.260	0.159	1.630	0.103
_cons	-1.894	0.279	-6.800	0.000
Observations	208			
Psuedo-R2	0.270			
LogL	-70.35			

Table 3 – Nearest neighbor matching results

		95% Confidence Int^			Support Treated	
Outcome	**ATT**	**Lower CI**	**Upper CI**		**Off**	**On**
Total Catch	277.8**	75.74	778.71		5	34
Total Large Catch	191.1**	66.02	456.77			
Total Trout Catch	242.1**	75.37	569.16		**Support Controls**	
Total Large Trout Catch	158.3**	85.34	321.04		**Off**	**On**
Total Trips	140.6 *	-20.79	413.77		0	169
Total Catch Per Trip	0.296**	0.16	0.61			

^ Confidence intervals constructed using bootstrapped bias corrected standard errors from 1000 replications.

** - significant at 5%, * - significant at 10%

Table 4: PSM covariate balancing results.

		Mean		T tests	
Variable	**Sample**	**Treated**	**Control**	**t**	**pr>|t|**
pctUp_oeko_4	Unmatched	0.0490	0.0651	-0.41	0.682
	Matched	0.0504	0.0452	0.13	0.897
pctUp_oeko_2	Unmatched	0.1469	0.1378	0.17	0.865
	Matched	0.1686	0.2317	-0.79	0.435
pctUp_oeko_3	Unmatched	0.4232	0.0805	6.92	0
	Matched	0.3442	0.2599	1.03	0.305
pctUp_FUSS_DCH	Unmatched	0.5422	0.3666	2.36	0.019
	Matched	0.5900	0.7228	-1.75	0.084
pctUp_FUSS_BAU	Unmatched	37.6330	16.1120	4.05	0
	Matched	31.7870	25.4910	0.98	0.33
pctUp_UFER_BR	Unmatched	0.4535	0.3440	1.46	0.146
	Matched	0.5067	0.6566	-1.88	0.065
pctUp_UFER_BE_1	Unmatched	0.1252	0.0453	2.28	0.023
	Matched	0.1266	0.0486	1.56	0.124
pctUp_UFER_BE_2	Unmatched	0.5967	0.3821	2.72	0.007
	Matched	0.6342	0.7773	-1.85	0.069
pctUp_UFER_BE_3	Unmatched	0.0297	0.0172	0.67	0.507
	Matched	0.0274	0.0430	-0.5	0.622
pctUp_UFER_BE_4	Unmatched	0.1498	0.0629	2.15	0.032
	Matched	0.0987	0.0966	0.04	0.965
pctUp_SOHLE_PROZ	Unmatched	1.2737	1.3495	-0.06	0.955
	Matched	1.4168	0.8932	0.75	0.459
pctUp_WSPIEGLB_V_1	Unmatched	0.3211	0.2767	0.61	0.543
	Matched	0.3683	0.4748	-1.14	0.26
pctUp_WSPIEGLB_V_2	Unmatched	0.3436	0.1559	3.17	0.002
	Matched	0.3762	0.3783	-0.02	0.982
pctUp_WSPIEGLB_V_3	Unmatched	0.2368	0.0749	3.57	0
	Matched	0.1424	0.1124	0.66	0.514
anyUpSohle	Unmatched	0.9487	0.3669	4.1	0
	Matched	0.9118	1.2353	-1.21	0.232

Table 5.

	FE Logit								
	Outcome	**Catch**							
Model		**Coeff.**		**Std Error**		**Odds Ratio**		**Obs**	**Groups**
(1)	Catch	0.0393	***	0.0111		1.0407		1430254	188889
(1')	Trout Catch	0.1551	***	0.0119		1.1677		1175956	172158
	Negative Binomial Model on fish catch counts								
	Outcome	**Catch**						**Number of**	
		Coeff.		**Std Error**				**Obs**	**Groups**
(2)	Catch	0.0567	***	0.0058				1256084	73722
(2')	Trout Catch	0.0312	***	0.0059				1016882	66202
	Interval Regression on fish catch size								
	Outcome	**Catch**						**Number of**	
		Coeff.		**Std Error**				**Obs**	**Groups**
(3)	Catch	-0.0974	*	0.0570				883468	121215
(3')	Trout Catch	-1.8436	***	0.0598				954181	138876
	*** - signif at 1%, ** - significant at 5%, * - significant at 10%								

Table 6.

	FE Logit	**Posttreat**						
	Outcome	**Catch**						
Model		**Coeff.**		**Std Error**	**Odds Ratio**		**Obs**	**Groups**
(1)	Catch	0.0470	***	0.0147	1.0481		444953	40427
(1')	Trout Catch	0.0556	***	0.0159	1.0572		386826	37554
	Negative Binomial Model on fish catch counts							
	Outcome	**Catch**						
		Coeff.		**Std Error**			**Obs**	**Groups**
(2)	Catch	0.0065		0.0067			399245	17488
(2')	Trout Catch	-0.0354	***	0.0072			346840	16594
	Interval Regression on fish catch size							
	Outcome	**Catch**						
		Coeff.		**Std Error**			**Obs**	**Groups**
(3)	Catch	0.2156	***	0.0702			444962	40428
(3')	Trout Catch	0.2587	***	0.0725			386836	37556

*** - signif at 1%, ** - significant at 5%, * - significant at 10%

Note: all models include angler by fishing site fixed effects and time by quarter fixed effects

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[i] PSM is technically equivalent to fully interacted OLS, but PSM still exposes common support issues.