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A RETREAT FROM THE IDEAL: REFLECTIONS ON A FRUSTRATED ATTEMPT TO STUDY A SOCIAL PHENOMENON THE NATURAL SCIENCE WAY

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Almost 15 years ago, Filipino social scientists were provided a rare opportunity to design their own methodology for the analysis of the social and economic impacts of a wide range of governmental programs. A series of workshops assembled twenty or so researchers periodically, during which they agreed on the eleven types of impacts to be studied, the timetable for the work and the sharing of their results at special workshops. Called ESIA-WID (Economic and Social Impact Analysis/Women in Development), the research program had a set of American and Filipino consultants[1] who led the critiquing of the papers and presentations. The program assembled what up to that time was probably the largest single group of social scientists who ever worked together on a single research program — economists, sociologists, demographers, political scientists, psychologists, and public administrators.

The ESIA/WID program ran for three years. Because it was as interested in the methodology to be developed as in the project impacts themselves, it gave researchers the luxury of eight months just to write the design. Rebecca P. Albano and I submitted a proposal in 1979 with all the

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1. These included the top Filipino social scientists at that time — and even up to now — Dean Jose Encarnacion, Dr. Raul P. de Guzman, and, of course, Dr. Gelia T. Castillo.

elements of a proper statistical social evaluation: a comprehensive review of the literature, reasonably creative and appropriate operational definitions, and a battery of data collection techniques,[2] all of which were to be analyzed using a quasi-experimental design. I have been told that some colleagues used it as a model proposal for their students and that it still sits on library shelves for that purpose. I showed my gratitude by not implementing the design as it was laid out in that paper. Instead, I worked with a drastic modification of the original design.

The transformation of the design occurred because of practical difficulties I met in the course of implementation. These problems, in turn, made it necessary for me to look more closely at the implicit assumptions behind the various elements of a research design. Instead of taking them for granted as we usually do, I was forced to study each element and to retain, modify or delete it as seemed appropriate and proper.

Revisiting the project after a long passage of time, I think it might be useful to share with other social scientists some of my mistakes and some surprises I encountered as well as some reflections on the decisions I made. In the process, I shall expose what I see as the unexamined theories behind the usually accepted assumptions of the classic evaluation design and the casual model it implies. In so doing, I hope to contribute to an understanding not only of how social science differs from natural science but also of the reasons why social scientists need to take the linkage between methodology and theory more seriously. To my mind, this has been made necessary by the fact that the assumptions mask substantive theoretical questions that social researchers need to address directly to improve social science and social reality as well.

THE MODEL FOR THE STUDY

My assignment was to find out the impact of rural road projects on Philippine communities. As agreed upon in the larger study, a road was to

2. Among these are a household survey, transporters' survey, origin-destination survey, and the market basket technique.

be evaluated on its direct and indirect effects on eleven development concerns: production and productivity, income growth, income distribution, employment, health, nutrition, education and literacy, political participation, population, energy, and the environment. The extensive literature I reviewed identified roads, not surprisingly, as infrastructure for transportation. This suggested a clear line of analysis: the first direct effect of a road is its capacity to generate and divert traffic. All other effects flow from this — the increased access to production inputs, markets, employment opportunities and social, economic and political facilities. And only then would effects on the eleven development concerns be felt. The initial model is presented in Figure 1.

THE IDEAL DESIGN FOR IMPACT EVALUATION

To test the hypotheses implied by the model, I needed a design that would be able to show how a rural road changes a community. I then turned to the classic experimental design straight out of natural science. Ideally, that design has the following elements:

1. The project (or "treatment" or "independent variable")
2. Random assignment of units to different groups
3. Comparison with a group that did not receive the treatment
4. Without- and with-project comparisons.

Figure 2 depicts how these elements go together in the classic experimental design.

The "treatment" is the activity or item that constitutes the experiment. In a laboratory, it is easily identifiable and measurable and can be withheld or provided by the researcher at will.

The units under study are expected to be equivalent at Time One. Such equivalence is statistically vouchsafed by random assignment, under which each unit has an equal chance of winding up in either experimental or control group. When units are complex and have characteristics that may have a

FIGURE 1
The Impact of Road Projects on Households

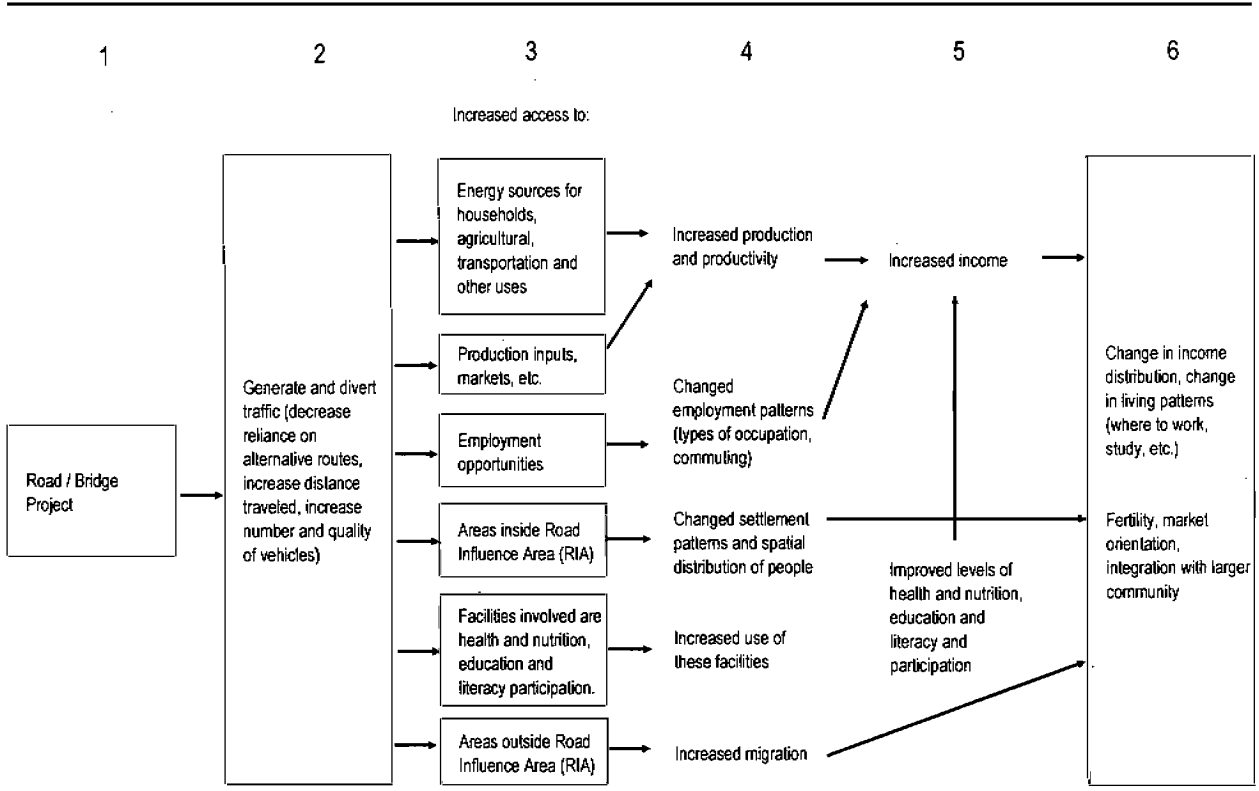


FIGURE 2
The Classic Experimental Design

R	TIME 1		2
Experimental group	e_1	X	e_2
Control group	c_1		c_2

where R stands for random assignment
e_1 for experimental group at Time One (without project)
e_2 for experimental group at Time Two (with project)
c_1 for control group at Time One
c_2 for control group at Time Two
X for the project or treatment variable.

systematic effect on their behavior, such bias is expected to be minimized by randomization which cancels out the effects of residuals. In any case, the amount of bias that may still creep in to the system can be calculated statistically.

In laboratory situations, the researcher randomly designates which group will be "experimental" (the recipient of the treatment), "control" (recipient of no treatment) or "placebo" (another treatment not expected to have a systematic effect on the group under study).

If these rules and procedures are followed, the observation at Time One simply documents that the groups were indeed equivalent prior to the treatment. With the introduction of *X* to the first group being its only distinguishing feature from the second, any differences between them at Time Two may be regarded as a result of *X*.

In social experiments with human beings as units, other factors have been identified as capable of changing each group between the two time periods. These are the passage of time or maturation, instrument decay, the

test-retest problem, history, and regression towards the mean. The influence of such extraneous variables is expected to be captured by the comparison of the differences between e_1 and e_2 and between c_1 and c_2 (Campbell and Stanley 1963).

The factors affecting the difference between the experimental and control groups are biased selection and the mortality of units in the samples. They are expected to be captured by comparing the differences between e_1 and c_1 and between e_2 and c_2 (Campbell and Stanley 1963).

The analysis of such a design would be straightforward and easy to do if all the elements were in place. I shall illustrate the problems using primarily my retreat from the use of most of these elements in undertaking the impact evaluation of rural roads under the ESIA-WID program.

THE ELEMENTS OF AN IDEAL DESIGN

X: The Rural Road Project

It seemed easy enough to define X — a "road," a transportation infrastructure and a "rural road" as one located in a primarily agricultural area. Since the ESIA-WID program was funded by the United States Agency for International Development, it was also clear that, among the many projects dealing with rural roads, I was expected to study the one funded by USAID, simply called "the Rural Roads Program" (RRP).

Once I started the research, however, the definition of X became slightly more complicated. For one thing, it turned out that RRP, and then the Ministry of Public Works and Highways, for that matter, classified as "road projects" infrastructure built solely on land (what lay people call "roads") or built over water ("bridges") as well as "road and bridge" combinations. Moreover, RRP constructed roads not only in really rural areas but also in census-defined urban areas of largely rural municipalities. In addition, rural roads differed in terms of their expected average daily traffic (major, minor or penetration), ownership (by the municipal, city, provincial, or national government), materials (soil, gravel or all-weather, asphalt, and concrete), length (from a few meters to several kilometers), the

terrain on which they are built (flat, rolling, mountainous, over water), distance from the *poblacion* or other population centers, etc. These characteristics described only the road per se, but already, one could think of variations in effects depending on the kind of road chosen. For instance, roads in really rural areas and in urban parts of rural areas could affect their communities differently, if only because of variations in ease of access. Besides, bridges might be expected to have stronger impacts than roads because they link areas previously unconnected or those linked only for people with light loads. The impact of a one-kilometer road may not be half as that of a two-kilometer road. In any case, it could very well depend on whether or not the road (1) is connected to another road or to a hinterland, (2) traverses a heavily populated area, or (3) links the farms of ordinary people and so on. Note that none of these characteristics was within my control as a researcher.

What all of these meant was that I needed to have a clear definition of the *X* whose impacts would be my major concern. More than that, I needed to have a theory of what characteristics of the road would be neutral or sensitive to the communities and impacts I would study. Ownership of the road by one level of government was one characteristic that seemed unlikely to influence the behavior of people relative to the road. On the other hand, distance from the *poblacion* or from farms/school/markets, the type of terrain, or the size of its traffic seemed likely to have differential effects on a community. These not only suggested that an "ideal-type road" would be difficult to get, but could also lull me into thinking that the effects I could get would be representative of the effects of roads with other categories of "sensitive" characteristics.

Instead, I got a sample of different types of roads in order to get a "feel" of the range of variation of impacts which a rural road project could provide. Given limitations of time and resources, I decided to study four projects that were planned to be road-and-bridge combinations ranging in length from five to 11 kilometers. I then faced an administrative problem: like my research design, the plans were changed in the process of implementation (Table 1).

TABLE 1
Length of Rural Road Projects Under Study

Project	Length of road (km)		Length of bridge (km)	
	Expected	Completed at time of study	Expected	Completed at time of study
Laurel-Tamayo Road Laurel, Batangas	11	0	35	35
Palsara-Alangilan Road Balete, Batangas	5	0	15	15
Mabilao-Binday Road San Fabian, Pangasinan	5.6	3	28	0
Dapdap-Nipaco-Rangayan Road, Paniqui, Tarlac	6	4	0	0

All the implemented projects were shorter: two built bridges only (the first was constructed in 1979 and the second partly in 1979 and partly in 1980) while the other two built roads only (both of which were constructed in 1980). Serendipitously, they provided variations in the type of road, in length, in location and in time elapsed after construction. If their effects were very different from one another, I would be constrained to talk of the effects of a particular case, with the recommendation that similar cases be studied to make us understand more how, say, a bridge, changes the life of a community. If the effects of the four cases were similar to one another, it would strengthen the hypotheses about the impacts of rural road projects as a whole since the results show that the effects of the different factors in the package of these cases seem to have cancelled each other out. Nevertheless, I would still be constrained about generalizing their results because of both the smallness of my sample, and the fact that I selected them in a purposive manner.

Randomization

The research under discussion may be classified as a "natural social experiment." As the name suggests, a natural social experiment is a situation occuring "in nature," or in the actual field condition, which lends itself to treatment as an experiment (i. e., observations at time periods without and with the project, and comparison with a real-world condition that escaped or did not receive the project that is the focus of study). At the outset, a natural social experiment already relaxes the first requirement of an experiment, in that the project is not randomly accorded to any local area. Moreover, the people already living and working in that community cannot be randomly assigned to transfer to another community which will serve as the control group. In most cases, it would be impossible to move people to allow a scientific study to get underway; one can only imagine the costs of moving whole communities around, with the concomitant disruption of work, residence, relationships, etc. Even when costs are attainable, the ethical undesirability of such disruption for the sake of scientific purity is obvious. The pragmatic reason is thus joined by the ethical in the decision to drop randomization.

The original design omitted randomization from the start and thus earned the title "quasi-experimental."

Even in social experiments in the laboratory where randomization is empirically possible, questions have been raised as to whether or not the advantages of randomization are attainable. Such laboratory experiments often use people who volunteer. By that act alone, they may set themselves apart from the ordinary run of human beings and thus produce results which cannot be generalized beyond the small group from which they sprung. Critics have dubbed the results emanating from such studies as "the social science of the sophomore class" if conducted in laboratories using student volunteers since these are usually the volunteers or conscripts in social experiments, freshmen being too new in the university and juniors and seniors likely to be either too smart for the experimenter or already helping the researcher to conduct the study. When volunteers come from outside academe, the findings from their involvement are called "the social science

of nice people," implying that their amiable qualities could confound the results of the experiments. It is a hypothesis that should be tested.

The Control

A control is a sample deemed to be equivalent to the experimental group before the introduction of *X*. The issue at hand, then, is: What would constitute "equivalence"?

In the RRP, a control road was designated for every road project that was identified. This was a proposed project for a transport infrastructure that was not approved by the program but was comparable in length and type to one that had been constructed. Thus, contrary to the expectation that the communities would be similar, the presumed equivalence in the RRP control resided on the *X*, not on the communities to be affected by *X*.

It should be noted that RRP also had a regulation that barred any construction from being made in the control road's community for five years from the period of its designation. This was made for purposes of ensuring scientific purity. However, the ethics of withholding a needed good to a community for the sake of establishing a reasonable scientific finding could be questioned.

The next step was to study the communities of the putative control roads of the projects we had chosen for intensive study. Generally, these were adjacent to the "experimental community" and would have the same provincial and ethnic background, but in all cases, their similarity ended there. The pair of control and experimental communities differed in their size, economy (dependence on rice, level of diversification of crops, size of the trucking industry), distance from important landmarks (the town center, the provincial capital, etc.), political characteristics (type of mayor, partisanship and activeness of the community), presence of government supplementary investments (other special projects located in the town, e.g., irrigation, electrification), and private firms (rice mills, factory, etc.). All these could be hypothesized to affect the direction and pace of development of the town, that is, the operation of the eleven development concerns. As such, they constituted rival hypotheses which could then confound the

problem of attribution of effects to the rural road project. Because these factors could not be randomly distributed or controlled, I decided to drop the idea of using control communities.

It might be noted, parenthetically, that before I made this important decision on theoretical grounds, I had already contended with the practical difficulty of finding communities, not roads, which were equivalent prior to the construction of the road.

Suppose I had been lucky enough to find communities equivalent at Time One, should I then have kept up the use of controls? Based on data on the change undergone by the communities I actually studied, my answer would still be negative. The issue being raised here is: What is the theory of the pattern of change in the communities?

The use of controls assumes not just the equivalence of communities before the project, but also the similarity of their rate of change over time. However, this assumption has not been tested. If two children, at age 13, were both five feet tall, their height would be equivalent at this time, but it would not be correct to assume that five years hence they would still be of the same height. A better measure of prediction may be to note their growth over time and thus to establish how they change from year to year.

Social methodologists have thus suggested that, instead of independent control groups, a researcher may be better off studying the subject over a much longer time period in order to establish its pattern of change. The spurt of growth of the project under study could then be noted as an aberration in its regular pattern, and thus as an indicator that the road had such an effect.

Using this reasoning, in place of control communities I turned to the idea of approximating a "reflexive control group," "reflexive" suggesting that the community was being compared with itself over time. The best data-set for this purpose is the measurement of characteristics over a long period of time, so that a trend could be discerned. RRP was supposed to have made annual surveys which could have allowed for a partial application of this idea.

Unfortunately, even this reflexive control group design could not be used for two reasons. First, RRP annual surveys were either not completed for all the years they were supposed to have been done, or did not have the data I needed to measure the level of the community on the eleven development concerns. Second, the projects were not constructed as scheduled, in effect changing the X in midstudy.

Because of these, I could not test a theory of change for any community. For two cases, I had access only to one "before" and one "after" data which showed the effect of the project but could not establish the pattern of change of the community. For the third, there was one "without" and two "with-project" surveys. For the fourth, where comparable dara were available for three years, the pattern could not be established because construction occured between all surveys (Table 2).

Without and With-Project Observations

The comparison of without and with-project observations was the element of the classic design that I kept intact. However, having dropped the idea of controls, I decided to use a comparison of in-depth case studies in lieu of a statistical analysis of characteristics of communities at two or more time periods. This required supplementing the survey of structured interviews with other data-gathering techniques. Thus, aside from the RRP surveys and the ESIA/WID household surveys, origin-destination surveys, the market basket technique and a special survey of transporters, unstructured interviews with key informants, and planned observations in the areas were undertaken. All of these activities were done for at least two time periods, and included at least one pre-project gathering of data.

The comparison of observations at several time periods allows the researcher to see the effects of the introduction of the project into the community, which should be guided, as I have already mentioned, by a theory of how communities change. I also had to tackle two other questions: (1) When does a project begin to show effects on the 11 development concerns? (2) What mediating mechanisms are needed for the effects to occur? As in previous problems, there were implicit assumptions on timing

TABLE 2
Date of Construction, Periods of Data Collection and Organizational Sources of Data

Project	1978	1979	1979-80	1980	1980-81	1981
Laurel-Tamayo Road, Laurel, Batangas	RRP*	RRP*		ESIA-WID	Construction	ESIA-WID
Palsara-Alangilan Road, Balete, Batangas	RRP*	RRP*		ESIA-WID	Construction	ESIA-WID
Mabilao-Binday Road, San Fabian, Pangasinan	RRP*	RRP*	Construction	ESIA-WID	Construction	ESIA-WID
Dapdap-Nipaco-Rangayan Road, Paniqui, Tarlac	RRP*	RRP*	Construction	ESIA-WID		ESIA-WID

* No data available: RRP surveys were either not undertaken or had no relevant data.

and mediation of effects. For me to make sense of what I was doing, I felt it necessary not only to make those propositions explicit, but also to convert them from assumptions to hypotheses.

The Timing of Effects

My model posited certain factors as direct or indirect effects, suggesting a certain order in the generation of effects. I also had some notions of which effects would be immediate, medium term or long term. The schedule of the ESIA-WID program implied that most, it not all, effects could be noted after at most a year of the project. I had doubts, however, as to how much health and other social effects could occur at that period although I had prepared indicators of how to measure them. If only for practical purposes,

then, I needed to have a sense of how soon each effect might be capable of being observed, in other words, a theory of the timing of effects.

Given the ESIA-WID and RRP schedules, this was no armchair theorizing. I was faced with two communities whose road project was built only six months before my schedule for data collection at Time Two: Would it be fair to measure how it had generated and diverted traffic, how it had contributed to production and productivity, income change, income distribution, etc? Fortunately, I was studying two other projects which had different dates of construction. This allowed me somehow to test hypotheses of the timing of effects, even though this was not in my long list of working hypotheses to start with.

The findings which Wilfredo Carada and I wrote up in the 1982 ESIA-WID report included the following:

> 1. As suggested by the model, the effect of road projects on the generation and diversion of traffic was direct and immediate. This was shown not only in the increased usage of the road but also in the growth in the number of transport groups — trucks, jeepneys, tricycles and, in one case, horses — found in the markets of those communities within six months of construction. In addition, effects on energy consumption were immediately apparent. They were indexed by the annual savings in vehicle operating costs enjoyed by most transporters and vehicle owners which were estimated to be P4,528.74 in Laurel and P3,760.96 in Balete.
>
> 2. Road projects also had immediate beneficial effects on income and production. My model posited that they would be mediated by two factors — the traffic and the increased access to farms and other places of production which could eat up time. These mediations did take place but effects could be noted within six months of construction anyway. Table 3 illustrates this in the case of the income effect.

TABLE 3
Mean Household Monthly Incomes Before and After Construction

	Before (1980)	After (1981)	Percent Increase	
Road users	P254.17	P472.22	85.8	t = 3.57 significant at p < 0.001
Nonroad users	P273.08	P450.00	64.8	n.s.
All households	P259.18	P460.82	77.6	t = 4.42 significant at p < 0.001

Sources: Household survey at Road Influence Area of Alas-as Bridge, Laurel, Batangas, 1980 and 1981 (at current values).

3. My conclusions would have stopped there if I did not have a project where effects could be observed over a longer time period. It showed that income change not only occurred but accelerated with the passage of time and with the second phase of construction of the road. I was not able to determine how long the fast pace would proceed because the ESIA-WID project ended then. It was also not possible to test whether to attribute the change to time and/or the additional construction of a larger part of the road (Table 4).

4. It might be said that, in general, social effects needed a longer gestation period. An exception was the increased access to health facilities which was immediate in one case, although health status conditions did not improve even after the lapse of two years.

An incipient theory of the timing of effects of a road project might then include immediate results in the generation of traffic as well as in the environment, energy use, and income, but a longer period would be needed to study the occurrence of change in all other development concerns.

TABLE 4
Household and Per Capita Income
Mabilao-Binday Road Influence Area
1979, 1980, 1981

	1979	1980	Percent difference from 1979	1981	Percent difference from 1980	Percent difference from 1979
Average Annual Income	4,827	5,412	11.7	7,765	43.8	60.9
Per Capita Income	804	899	11.8	1,294	43.9	60.9

Sources: Household Economic Surveys, 1979, 1980, and 1981, prepared by the Pangasinan Provincial Development Staff for Rural Roads Project.

The Mediation of Effects

Associated with timing was the issue of what factors other than the road triggered the generation of effects. My already complex model was not able to anticipate the operation of other factors that would facilitate the generation of effects or falsely attribute to the road effects which they were more directly influencing. My decision to have intensive case studies of the communities of four road projects allowed me to recognize two other basic sets of theories missed in my model: (1) the question of automaticity versus assistance, and (2) the presence of rival "causes."

Automaticity versus assistance. My model posited that the effects of the road had to be mediated by access to facilities. My understanding at the start of the research projects was that the presence of the road would in itself trigger access. In the case of truckers and farmers, the hypothesis of automaticity was borne out. Indeed, the road immediately diverted traffic to that area and provided access to the markets they needed.

In case of the health effect, however, the presence of the road was not enough. For it to have an effect on health, the municipal health officer needed to take it upon himself to use the road to regularly bring his team of medical workers to the barangay which has been made accessible. The road's presence and the rural health unit's location along the new road were not sufficient in themselves to generate a small health effect, such as it was.

Income distribution effects underscored even more the need to recognize the role of mediating factors. In all four cases, incomes in the four communities increased with the road project. However, although the incomes of the poor also increased, the gap between them and the richer members of the community did not significantly decrease. For instance, in Laurel, although farmers and drivers realized incomes greater that those of other occupational groups, they remained at the bottom of the totem pole (Table 5).

Among the farmers, those who could transport their own goods had the highest income increases followed by road users, while those who did not use the road had only minimal changes in their incomes. These farmers were arrayed according to their status. Those with their own means of transport were the most well-off. Those who transported goods by foot or animals, and who sold them without using the road had lower incomes. Those who used the road were probably bringing heavier cargoes which required the use of motor vehicles (Table 6).

Why these differences? The elite had the facilities or the wherewithal to buy facilities that could immediately maximize the use of the road. They had vehicles, money to buy inputs, access to extension workers or new research, or to the capital, and maybe even connections with political leaders. The rest of the community in effect only had the road. They did increase the quantity of crops they could bring to the market, but that was all. In light of this, one hypothesis presented itself: "If government introduces only a rural road, it could exacerbate the income distribution problem. This is because the more wealthy could readily make full use of it since they already have other resources that facilitate this. However, the poor will only receive minimal benefits unless they get some assistance or undertake other

TABLE 5
Mean Monthly Income of Various Occupational Groups
Laurel Tamayo Road Influence Area
1980 and 1981

	Pre-Construction (1980)			Post-Construction (1981)			
	N	Mean Income	Rank	N	Mean Income	Rank	Increase (%)
Farmers	75	P161.47	6	72	P352.78	6	118.35
Nonfarmers	23	398.53	—	25	414.22	—	39.3
Drivers	4	300.00	5	3	550.00	5	83.3
Sales persons	6	466.67	3	6	733.00	2	57.1
Janitors	2	450.00	4	3	683.33	3	51.8
Office workers	7	750.00	1	8	975.00	1	30.0
Other manual workers	4	625.00	2	5	650.00	4	4.0

Sources: Household Survey, 1980 and 1981.

activities that could increase their ability to use the road. For instance, they may need a resourceful municipal health officer or a similar official to increase their access to facilities. Or they could form cooperatives to improve their access to vehicles or markets."

On a more general hypothesis, "the introduction of any new technology or facility, unaided, without any supplementary assistance or activities on how ordinary people may use it, will lead to more benefits for the rich than for the poor."

TABLE 6
Mean Income of Farmers
Laurel-Tamayo Road Influence Area
1980 and 1981

	Pre-Construction (1980)		Post-Construction (1981)			
	N	Mean Income	N	Mean Income	Difference	Percent Increase
All farmers	75	P161.47	72	P323.68	151.28[a]	118.3
Farmers who						
bring produce to wholesalers	39	198.72	51	438.23	239,51[b]	120.5
use the road	26	196.15	32	506.25	310.10[b]	158.1
did not use the road	13	203.85	19	323.68	119.83[a]	62.9

[a]Not significant
[b]Significant at $p<0.001$
Sources: Household Survey, 1980 and 1981.

Alternative or rival cause factors. The study was done in an ongoing social situation rather than in a vacuum such as in a laboratory. Thus, despite the RRP taboo on the construction of another USAID road project in the sample communities, other projects might have been introduced in the area which could have theoretically led to effects similar to the ones generated by the RRP road project. These developments, which in general are called the factor of "history" by Campbell and Stanley (1963), confound the attribution problem in this natural social experiment. Such varied factors as the opening of a cotton ginnery in one town, or the election of a new mayor

in another coincided with the road construction and could have either accounted for the changes that occurred, or reinforced the effects of a new road. Relying on a structured set of questions for residents alone would have made me unaware of the confounding factors abounding in the area. Gladly, the need to identify and analyze the complex social situation demanded by the intensive case technique alerted me to the problem.

THE ALTERNATIVE EVALUATION DESIGN

Based on these considerations, the design I actually implemented in the field was very different from the classic design I started out with. It became a series of intensive and comparative case studies where the community of each road project was observed at several time periods to approximate a reflexive control group (approximate because the RRP timetable for survey and construction did not materialize). This design is shown in Figure 3.

As already stated and as can be seen in Table 2, only the 1980 measures were available for Alas-As Bridge and Balete Bridge RIAs.

Implementing the new design also produced some changes in the model. They may be grouped into three questions: (1) What other causes were there? (2) What other effects were there? (3) Who were the actual beneficiaries?

Other Causes

The path of each factor was traced historically rather than statistically, making it possible to recognize other characteristics that mediated or reinforced the influence of the road. Also, greater familiarity with the area required by an in-depth case analysis facilitated the identification of variables not known in the design stage that could have acted as cause factors rivaling the identified independent variable, the road project. Much of this issue, including some examples, have already been discussed in the section on "Without and With-Project Comparisons." Figures 4A and 4B show examples of the more complex chains discovered in the actual study for two development concerns – health and income.

FIGURE 3
The Alternative Design

	1978	1979	1980		1981
Alas-as Bridge	a_1	a_2	a_3	X_1	a_4
Balete Bridge	b_1	b_2	b_3	X_2	b_4
Mabilao-Binday Road	m_1	X_{31}	m_2	X_{32}	m_3
Dapdap-Nipaco-Rangayan Road	d_1	X_4	d_2		d_3

where:

a_1, a_2, a_3, and a_4 are observations of Alas-as Bridge RIA in different time periods, and X_1 is the construction of Alas-as Bridge;

b_1, b_2, b_3, and b_4 are observations of Balete Bridge RIA in different time periods, and X_2 is the construction of Balete Bridge;

m_1, m_2, and m_3 are observations of Mabilao-Binday in different time periods, and X_{31} and X_{32} cover the construction of Mabilao-Binday Bridge;

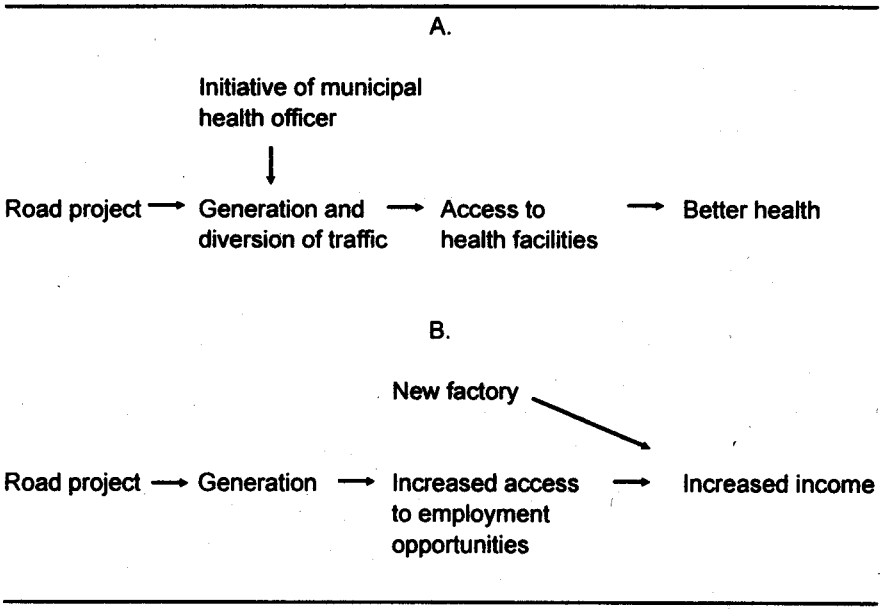
d_1, d_2, and d_3 are observations of Dapdap-Nipaco RIA in different time periods, and X_4 is the construction of Dapdap-Nipaco-Rangayan Road.

In the first, the initiative of a municipal health officer supplemented the road in bringing about new traffic and access to health facilities and health care. Together they brought about a social impact of the road. In the second, it was found that the opening of a cotton ginnery in another part of town, instead of the road, accounted for changes in income for some residents of the community.

Other Effects

The original model hypothesized that the influence of a road would be felt in terms of its nature as a transport infrastructure alone. The key informant interviews and observations in the area suggested that the model made a grave omission and needed to be revised to recognize a road's other facets. Under the ESIA-WID listing, all fell under the rubric "environment" (Figure 5). A road, whether used for transportation or not, changes the

FIGURE 4
Examples of New Causal Chain
Showing Effect of Factors Other Than Road Project

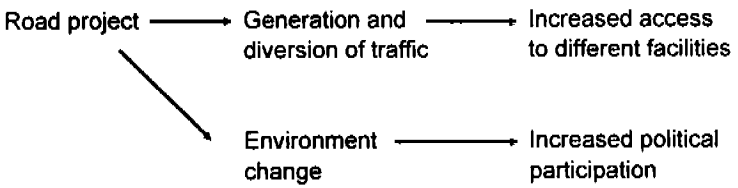


landscape and affects development through that fact. Indeed, that would have been obvious to anyone who has seen a rural road used for rice drying (affecting productivity and income). In this study, a road affected political participation as it became the focus of municipal beautification efforts and, in the absence of a town plaza, became the site of political meetings (not to mention its role in kite flying, as a lovers' lane, etc.).

Who Benefits?

Throughout this paper, I referred to the community around the road as the focus of the study, implying that it was the sole or at least the main beneficiary of the road construction. RRP coined the term "road influence

FIGURE 5
Rubric Environment of a Road



area" or RIA to give flesh to that assumption. It was operationally defined as the community within a five- to ten-kilometer radius of the road project. Under the classic design, residents in the RIA would have been the only beneficiaries to be taken into account.

An anomalous finding brought the error of this assumption to my attention. As Table 7 shows, significantly more people in one RIA reported walking to the market *before* the road was constructed than *after*. This meant that they used the road less, using instead a shortcut that nonetheless required fording a river on foot. Why did they shun the road after it was built and chose instead to use a more uncomfortable alternative? Was it because the road was now full of vehicular traffic that made it dustier and noisier? The dust and noise would have been ignored had the vehicles been able to accommodate the residents bringing their produce to market. As it was, by the time the vehicles reached the RIA, they were already full of passengers from outside the so-called road influence area. Thus, community residents with easy access to the road found themselves the ones benefiting less from it as compared to residents from distant places.

The message of this table was reinforced by the increased income of truckers and public utility vehicle owners and operators, most of whom did not live in the area. Indeed, to have limited the study to the residents would have given a false picture of the benefits of the road project. While all the feasibility studies extolled its virtues as a farm-to-market road, what seemed

TABLE 7
Use of Alas-as Bridge in Laurel-Tamayo Road Project for Purchasing Goods Before and After Construction 1980 and 1981

ALL AREAS

Road usage	Period		
	Before	After	
Using	72	47	119
Not using	26	52	78
	98	99	197

$X^2 = 12.85$, df = 1, significant at $p<0.001$

NEAR AREAS (within one kilometer of Alas-as Bridge)

Road usage	Period		
	Before	After	
Using	43	19	62
Not using	13	37	50
	56	56	112

$X^2 = 19.12$, df = 1, significant at $p<0.001$

FAR AREAS (beyond one-kilometer radius)

Road usage	Period		
	Before	After	
Using	29	28	57
Not using	13	15	28
	42	43	85

$X^2 = 0$

Sources: Household Survey, 1980 and 1981.

TABLE 8
Average Monthly Income of Different Types of Transporters, Before and After the Project Construction (At current prices)

	Before		After	
	Income	Rank	Income	Rank
Truck Cargo/operators *			17,240.00	1
Jeepney owners-drivers	1,236.38	1	1,967.31	2
Jeepney operators	1,236.00	2	1,867.40	3
Bus drivers *			1,069.50	4
Jeepney drivers	683.25	3	1,004.82	5
Tricycle operators *			901.65	6
Carabao cart owner-driver *			750.00	7
Tricycle owner-driver *			567.00	8
Horse owner-riders	371.12	4	417.19	9
Carromata owner-drivers *			135.00	10

* No transporter of this type was available before construction.
Sources: Transporters' Survey at Alas-as Bridge, RIA, 1980 and 1981.

to emerge was that it was a market-to-farm road, increasing the access primarily of those who had more to start with, instead of making it possible for the poorer among them to compete on even terms.

I also found that market vendors were coming from more distant areas than before and that the proportion of outsiders selling in the market increased. Table 5 already showed that the gap between the highest and lowest income earners in the community had not decreased after the road project. But if one included nonresidents operating in the area, the gap between the highest and lowest income earners would have clearly been greater. For instance, the average monthly income of truck operators who did not live or operate in the area before the bridge construction was P17,000, several times higher than that of jeepney owner-drivers, the second highest income earner, and the highest among residents.

The assumed concentration of benefits in the RIA might be a case of what Hoover (1975) called mistaking "place prosperity" for "people prosperity." Indeed, all the communities seemed to be more active and to earn more income after the road was constructed. But the prosperity redounded to the place rather than to the majority of the people residing therein.

This is, however, not a brief for stopping the construction of road projects. Rather, it suggests the need to supplement the road with support and assistance directed to the less wealthy residents in the area. The initiative of the rural physician could be accompanied by similar initiatives on the part of agricultural extension workers, or better still, the municipal and barangay leaders to enable the poorer residents to reap some of the benefits to be derived from the construction of a road in their locality.

LESSONS AND CONCLUSIONS

This change of a research design from conception to implementation taught me several lessons that have since guided my conduct of any research project. Let me discuss a few of them here.

First, the subject and situation surrounding the conduct of social research are different from natural science. Thus, a direct transfer of the

methodology of the latter is probably neither feasible nor even desirable in many instances. In this essay, I have tried to raise the ethical, methodological, practical and theoretical problems related to the use of randomization, the designation of control groups and the complexity of the X to be introduced that transformed what in the beginning looked like a simple natural social experiment.

Second, any plan is probably not capable of implementation as is. However, it is important to be conscious of what you are giving up and why, and to enrich your research as you trade one idea or technique for another. I gave up the classic evaluation design for the more humble case study method literally because of forces beyond my control. But being aware of its limitations, I also tried to make full use of the many opportunities it offered. A study of cases made it necessary to look more deeply into the history and contemporary events happening in an area and to consider several ways of getting at that information. Thus, the case technique opened up to me a whole arsenal of data collection techniques and actual new information which permitted me to recognize causes, effects and beneficiaries that had been ignored in my original model.

At the same time, the resort to case studies did not mean that my research had to give up rigor. If anything, using only cases intensified the desire and the need to analyze all the data I had as deeply and as comprehensively as possible. My review of the literature remained comprehensive, and my operational definitions, methods and analyses were subjected by my colleagues and seniors in the ESIA-WID to their usual penetrating criticism. Nor was this entirely a shift to mere qualitative analysis since computations of vehicle operating cost savings, Chi square tests, and t-tests were used extensively throughout the study.

Third, while diagrams and figures make presentations easier, they are never really simple. Rather they assume a number of important factors that are often left implicit. As shown in this paper, the assumptions should not be taken for granted because they have complex theories behind them. At this stage of our social science, issues like the pattern of change, the timing of effects and who are the beneficiaries of the road project or of any other

intervention are theoretical issues that should be analyzed and tested rather than assumed.

All in all, I hope that laying out the mistakes and surprises I encountered is helpful in showing why methodology and factors related to it should be more consciously placed in the service of theory.

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COMMENTS

There was a time when we used to say that *academic excellence* is relevant to the University of the Philippines but gave more importance to administrators than to scholars and researchers. Thus, in the 1960s, I wrote a critique in the *Philippine Journal of Public Administration* on the university system, indicating that we ought to prove that academic excellence is indeed the university's aim.

In my sixth month as UP president, I reinvented the meaning of the word "University Professor." Before, the rank of University Professor had only been given to two of the university's presidents: Gen. Carlos P. Romulo and Ambassador Salvador Lopez; to two of the highest university administrators: Enrique Virata and Emmanuel Soriano; and years later, almost as an afterthought, to four known scholars: Teodoro Agoncillo, Leopoldo Llaves, Cesar Majur and O.D. Corpuz. When I became president, I resolved to make the University Professor the highest permanent position in the university whose basic salary is next to the University President's. In the administrative hierarchy, the rank would be the second highest but in the academic hierarchy, the rank would be the highest. Reinventing the title of University Professor, thus, meant upgrading the individual concerned to the position he/she most richly deserves: the highest in the academic hierarchy.

At that time, I had three individuals in mind who were worthy of the position: Gelia Castillo, Jose Encarnación, and Mercedes Concepcion. They are individuals who had achieved excellence in their respective fields and are recognized nationally and internationally. They were therefore the first three who were elevated to this rank. Today, we have 16.

I am sharing this information with you because I think if we really want to promote serious creativity, scholarship and research, our value system, ranking system and remuneration system should give importance to scholarship and creative work.

Let me now comment on Leddy's (Dr. Ledivina Cariño) excellent presentation. I would like to underscore her comment on the difficulty to apply the methodology in the social sciences. As president of the University then, I became a member of the Board of Trustees of the International Rice Research Institute (IRRI). For six years, I was exposed to natural scientists. I thought how elegant their conceptualization process was — at that time, they had this concept of the various ecosystems of rice.

In our meetings, they would present to us the ideal rice plant they wanted to produce for each ecosystem. They would describe all the components and then really aspire to produce the plants with all such accompanying characteristics. I thought then, *how can we apply all these to political science? How can we have the ideal political leader, the ideal citizen and community?*

I also felt that the natural scientist was missing a very important social factor. During my six years in the IRRI Board, there were two of us who insisted that natural scientists take into consideration the people when they think of raising and distributing new plant varieties. Often, their distribution of research results and interventions leave out the poor rice farmer and poor consumer in the equation. I might just be exaggerating but the point is that the human factor should be a major consideration in any research.

Another point I would like to make is this: Every public policy or public intervention is really a hypothesis ... We must therefore be on guard for unintended effects which may actually be dysfunctional despite the noble objectives of the policy or legislation.

I like Leddy's review of her research experience and her learning from it. I have a political scientist-friend from Hawaii who wrote on the US intervention in Korea and then 20 years later, looked back on his career. When he re-read his book, he saw how "violent" he was in his language and how much of a pacifist and non-violent person he had become.

As a young graduate student at the University of Michigan, I once observed a debate on behavioralism. The anti-behavioralist's point of view seemed to be: "What counts cannot be counted." I do not entirely believe in that. The point is too simplified. The question should thus be rephrased: "What counts can be counted in different ways." This means that we need rigor in inventing the ways of measuring, and to reach the sophistication demonstrated by Leddy in discriminating among different methods and approaches. It is not that we do not need rigorous, quantitative methodologies. Only, we need to supplement these with other methodologies and know which ones to apply. There are qualitative values that defy measurement but must be considered.

This seems very elementary but more often than not, we have to remind our students that events are caused not only by one intervening factor but rather by many causes and intervening factors that sometimes produce multiple effects.

Implicit in Leddy's analysis of the effects of the roads is that social scientists should have a clear concept of the *good society*. The 1987 Constitution is rich, for it describes what constitutes the *good society* and the kind of society we want. After all, when we come up with policies and analyze them, we ought to trace our decisions to our concept of a *good society*. For instance, the concept is summed up in our Constitution as peaceful, free, just, democratic, prosperous and contextually, pro-God and pro-country. This value framework is very useful in our social scientific analyses.

Highlights of Discussion

LOOKING AT A SOCIAL PHENOMENON THE NATURAL SCIENCE WAY

Few people write about their experiences in developing and implementing methodologies in their research work, much less the pitfalls that they encounter. To begin with, researchers shun methodological types of research in examining social phenomena because of the false notion that this kind of research involves mainly numbers and statistics, and not much of insights. In a sense, researchers view methodological research from a narrow perspective and in the process, feel that they have little or sometimes nothing more to contribute to its enrichment.

A research project developed in the late 1970s, called ESIA-WID (Economic and Social Impact Analysis/Women in Development), was an attempt to convene a broad range of social scientists to analyze the social and economic impacts of government projects. When it started, there were beautiful models put forth to examine the issues. Unfortunately, these models were never operationalized, not so much because models have little meaning to social scientists nor that different types of social scientists have different views on economic and social issues but because reality is more complex than theory. Field research often elicits "surprises" and unexpected findings. And the ESIA/WID research project was no exception.

operationalize a multidisciplinary research like the ESIA-WID. Experts from different disciplines and fields can work together based on an integrative framework.

Developments on methodological convergence prompted social scientists to work more closely with natural scientists, thereby earning more respect and appreciation for their own work and role in research. In this regard, more researchers must take up the challenges of social science research. Practicing social scientists should also be encouraged to share their experiences (write papers and essays, teach basic principles and methodological approaches in research, etc.) with young people so that they would not go through the same mistakes as those of their mentors.

Finally, the social context of analysis of policies and programs has somewhat changed through the years. Society finds it more difficult nowadays, ideologically speaking, to tolerate the fact that in the process of studying the impact of a policy decision on the majority of the people, certain groups, especially the marginalized sectors, are negatively affected. Which is why non-government organizations (NGOs) and people's organizations (POs) have become very important.

Social scientists now have to "skirt" between the policymakers, on one hand, and the NGOs/POs, on the other, to draw from both sets of perspectives a workable solution and, at the same time, make inputs to actual program implementation and policy development.