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HOW I ORGANISE MY RESEARCH

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Talk given to ESRI Research Assistants
Wednesday, 16 October 1974

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Introduction

I give this talk with some trepidation. In the first place, I find it extremely egotistical to speak about the organisation of my own research, and I am nothing if not a modest man! More important, the general subject is one on which I never heard anybody lecture before. Thus, I do not have available many sources, the conventional prop of the professional research worker. Professor O'Connor, in organising these lectures, is providing for you a service which, in my experience, is not given anywhere else. Accordingly, I hope you will be patient with us if the first results are not too good.

The title given to me by Professor O'Connor is "How I Organise my Research", not "How I Do my Research". The latter would require me to deal with many exciting intellectual questions, such as objectivity, causality, creativity, inspiration, induction and deduction; whereas the former relates more to the nuts and bolts of organisation. Of course, I could always stretch my topic to embrace the exciting intellectual matters; but I have deliberately chosen to speak on the more mundane topics for three reasons. First, you can easily read about the intellectual issues, whereas you will not find much source material on the organisational matters. Second, far more people go astray because of ignorance or neglect of the more mundane matters. And, thirdly, by concentrating on the nuts and bolts, I hope I will succeed in taking some of the mystique out of research, which can be damaging to the confidence

of young people. Thus, much of what I say will seem very obvious, but in research you will find that nothing is obvious until it is asserted, and everything is obvious once it is confirmed.

I must enter one important caveat to forestall numerous representations to me at the next meeting with the Staff Representative Committee. I am speaking here in a purely personal and subjective capacity. I am talking about my own approach to the organisation of research. As Director, I must naturally be tolerant of the fact that not everybody will want to follow this approach. Just as "there are many mansions in my Father's house", there are many modes of arriving at the truth. A sensible Director must welcome, or at least tolerate, approaches that are far different from his own, provided, of course, the end product is satisfactory.

By research I understand a systematic search for new knowledge about a class of phenomena and the communication of the results. Thus, while research may employ many different techniques (e.g. formal deduction, surveys, econometrics, participant observation), and may provide many different types of knowledge (e.g. new data, new hypotheses, confirmation of existing hypotheses, evaluation of alternative policies, etc), nevertheless every research project will involve three main stages, under which I would like to organise my talk:

1. Project Selection and Planning;
2. Execution;
3. Writing-up and Publication.

1. Project Selection and Planning

Every piece of research should begin with an important question. As Lonergan states in his classic study:

"...insight depends on the accurate presentation of definite problems. Had Hiero not put his problem to Archimedes, had Archimedes not thought earnestly, perhaps desperately, upon it, the baths of Syracuse would have been no more famous than any others."

In my Oxford book I was asking the question: Why does productivity rise at different rates in different industries? It soon became obvious that differences in productivity growth are closely related to differences in the growth of output, and again the question is: Why? Essentially, that is the question the book sought to answer. In the book about post-war economic growth in Ireland, undertaken with Brendan Dowling and due to be published shortly, we are asking the question: Why was economic growth in Ireland more rapid in the 1960s than in the 1950s?

Now I should be very dishonest if I let you believe that I sat down to work and these questions emerged smoothly. In fact, the contrary was true. Obvious though they may be now, these questions were formulated only after a good deal of work. Indeed, for a person embarking on a research project for the first time, scarcely any part of a project is more difficult than selection. It may help to illustrate this if I outline my experience with my first major project, which later became the Oxford book.

Having completed "Generals" at Harvard, I went along to the Chairman of the Department, John Dunlop, hoping that he would suggest a thesis topic. Dunlop, in response to my question, looked at me benignly and said: "The process by which students find

their thesis topics is like marriage; nobody directs it but yet marriages take place". Though I left his room rather dissatisfied, and reflecting on the high divorce rate in the US, I now believe that there is a lot to be said for this approach, which is quite widespread in most universities. The selection of a project is an educative process in itself. At some stage of his life a research worker will have to select his own project, and the Ph.D. is probably the most appropriate learning stage.

In fact, it took me about four months of hard work to find a thesis topic. I had a list of about six subjects of interest to me and, on each in succession, I spent about two weeks reading and thinking. At the end of each of these two-week stints, I went along to see the most eminent name in that field in Harvard or MIT. A half-hour's discussion was sufficient in each case to show me that, for one reason or another, that was not the project for me. At the conclusion of each interview I always enquired if they could suggest a topic which some of them kindly did. Arthur Smithies, then engaged on a study of the Puerto Rican economy, suggested that I do a comparison of economic growth in Ireland and Puerto Rico, but the connection between the two economies eluded me. Edwin Kuh felt that I might attempt to develop a neo-classical growth model in which the growth rate would not be invariant to the savings ratio. Since this task was agitating some of the best minds in the profession, I felt it would be an unduly hazardous starting point. Karl Kaysen had funds for several industry studies in underdeveloped countries and was keen that I study the rubber industry in Malaysia, I think. These suggestions were all made with the best will in the world and with some knowledge of my capabilities and interests. Yet the fact that none of them was remotely of interest

to me indicates how hard it is to find a project for someone undertaking research for the first time. I may add that, with greater experience of research, I would not now mind tackling any of these projects. But for one's first independent attempt, one ought to have, in Dr Geary's phrase, "fire in the belly".

At any rate, as a result of all this, by the time I went along to see Kuznets I had at least reached the decision that I would work on some aspect of Irish industry. Kuznets is an extraordinarily experienced supervisor, who generally ended up supervising more than half the theses in Harvard. He was not particularly interested in the tentative ideas I had for research in the area, but he questioned me at length on the nature and coverage of the data. Though I thought I knew the data well, his probing questions soon revealed how casual I had been. He told me to go off and spend some time going through the data and then let him know exactly what was available. This I duly did. He then said: "Write up that". I looked at him in some astonishment, feeling that this was totally unnecessary. "All of it?", I asked. "All of it", replied Kuznets.

So I went away and spent another two weeks writing up the data, as Kuznets called it, painstakingly describing what data were available, over what period, what definitions and concepts were used, what breaks there were in the series, and what possibility there was of linkage where a break had occurred. At the time, I did not realise how extremely valuable this would prove. It put me completely in command of the data, and if my memory failed, I had a ready reference available. I may say that, in general, I find that more erroneous conclusions are drawn from failure to understand the concepts and coverage of data than for any other reason.

When working with a new set of data, I would strongly encourage following Kuznets's advice.

On subsequent research projects, after the first, it should not take quite so long to find a topic, and, indeed, you may be more fortunate than I in finding the first one quickly. As regards reading the literature in connection with project selection, a certain amount may be in order, but it is fatal to continue reading too long. Even for the largest project, I would not spend more than a month or so in reading.¹ I would then leave the reading aside, get involved in the project, and do further reading in conjunction with my own research. I find that I study the literature far more productively as a result of the questions posed by my own work. I must emphasise, however, that what I am saying here is based on the presumption of a thorough general foundation in the subject at the undergraduate and graduate level. There is no satisfactory substitute for this. That is why we consider that, in general, research assistants are not yet ready to undertake research on their own until they have undertaken graduate studies in their field.

I cannot over-emphasise the need for a research plan. The plan should cover the subject matter of the work, and the estimated time for each important stage of the project. In other words, if the total study is estimated to take twelve months to first draft stage, I would break this down into the important sections of the work, allocating so many weeks for each section. Apart from this overall plan I would also, at the beginning of each week, prepare a plan showing what I intend to do on each day during the week, and this plan, together with progress in regard to the overall project plan, is reviewed at the end of each week. The more things one has to do, the more interruptions one is likely to have, and the less time one has available for the project, the more need for such a plan.

1. In response to questions raised in the discussion of this paper, I would like to emphasise that these remarks do not deny the value of doing a fairly comprehensive review and assessment of the literature when one is entering a totally new area (e.g. the development of social indicators for Ireland). Since such work will be of general value, it should be treated as a small project in itself, and the results published.

I am aware that all of this sounds extremely rigid, but I hasten to add that the overall plan never remains fixed for the duration of the project. Indeed, I would even be prepared to go to the other extreme and say that a plan is something to be deviated from. However, unless one has a plan from which to deviate, one can become completely lost. By all means, revise the plan, but in doing so one must inquire from oneself honestly as to the reasons why the plan must be changed. Is it dictated by the needs of the project? Is it because one is working inefficiently? Is it because one is devoting too much time to reading? These are all questions that must be answered in the silence of the heart but, unless you regularly ask them, you will work very inefficiently. A plan is invaluable in attaining the discipline needed to ask them.

2. Execution

Thinking

If one has carefully selected the project and made out a sensible plan, execution becomes a comparatively straightforward task. How well the task is accomplished is, of course, another matter which depends to a great degree on the personal qualities of the research worker. Thinking is the most vital activity in the execution of good research. It is also the rarest. Prolonged abstract thinking is something that does not come easily, except to the very rare person. However, all of us can improve our ability to think by using certain aids, and I should like to mention some that I have found helpful.

The habit of noting casual ideas as they arise and subsequently transcribing these notes into appropriately organised

files is a very good way of getting your thought processes moving. In fact, I found it possible to write a whole paper on the basis of such notes, almost all of the ideas arising as I drove to and from work, (e.g. Ireland in the Year 2000). It is important to find a way of preserving such notes in an orderly fashion. In this regard, I would like to pass on a useful hint given to me by Abdul Khan who, in turn, got it from no less a man than Ragnar Frisch, the first joint winner of the Nobel Prize in Economics, namely, to put a date on every sheet of paper in your files, particularly where you have recorded data. You will find this a great time-saver, particularly when you have moved on to another project and wish to consult your notes on a previous one.

Poring over data is also a useful stimulus to thought, provided one tries to keep the mind totally active when engaged in what I sometimes hear described as routine calculations. Calculations become routine only if you shut your mind to the wider intellectual stimuli that will reach you from this task. In this regard, I may say emphatically that you have no business in applied research unless you develop a liking for working with data. If you find data collection and calculations boring, then it is the surest sign that applied research is not for you. I am totally unsympathetic to those ideas current in the Institute from time to time that data collection and calculation is a menial task. I know of no outstanding applied research worker who does not derive tremendous inspiration from working with data (example: Leontief and inversion of matrix).

I would also like to stress the importance of accuracy in the use of data. If calculations are done inaccurately, one can no longer hope to explain anything about the real world. The application

of the most sophisticated techniques on inaccurate data is dust and ashes. The fact that the basic data may contain some inaccuracies, does not justify adding to these inaccuracies through faulty calculations. In making calculations, I find it useful as an aid to accuracy to have a number of cross checks. You can build them up for yourself. For example, always try to have a prior idea of the order of magnitude to expect. Moreover, in calculating, for example, growth rates of value, price and volume, I would do the calculations for all three separately, and then check that the calculated value divided by the calculated price is equal to the calculated volume.

Model-building is another useful aid to thinking. I normally operate with representative numbers, rather than symbols or graphs. This means that I have to choose a great variety of examples to ensure that I have covered every conceivable possibility, and it is only at that stage that I try to generalise in mathematical form. Obviously, if you are a mathematician, you will prefer to begin with the general, and you are likely to think the problem through more quickly than I. However, I don't unduly regret my limitations in that regard, since I believe that playing at length with representative numbers may give me insights into economic behaviour that might be missed by the pure mathematician (Give a relevant example: e.g. entry-exit problem, Survey of Grant-Aided Industry problem).

The study of deviations is a particularly fruitful source of ideas. When fitting a regression line, for example, no matter how good the fit, examination of the residuals may suggest useful alternative hypotheses. One should never discard any piece of information that is awkward or recalcitrant. This is a very great temptation and frequently nobody but yourself will know. But,

by ignoring or suppressing awkward observations, one is escaping from reality and depriving oneself of the possibility of a thorough understanding of the area. In fact, the most profound insight might come from further thought about how to account for such observations. (Example: Freud and hysteria problem.) The same applies to the alleyways and byways encountered on the road to completing a project. As a research project progresses, several interesting, but apparently subsidiary, questions will arise which were not thought of at the beginning. It is largely a matter of judgment which of these should be pursued and which ignored. Here your planning schedule should be of great help to you. You cannot be expected to answer everything in one research project. However, you can always note them up for further analysis or a new research project.

Without spending too much time on the matter, I think it always worthwhile fitting the particular area under study into the general area. For instance, in studying industrial productivity, Kuznets rightly insisted that I place industry in the context of the economy as a whole. This gives a perspective to the area you are dealing with. Furthermore, it might lead to ideas about linkages with other sectors which may be vital to explanation of your sector. One should be highly interested in knowing as much as possible about the institutional side of the area, even though none of this may appear in the publication. In other words, no matter how technical your subject is, you should be able to listen to the practitioner's discussion of the area and see how his perspective fits into your deeper understanding. As in all things, a balance must also be struck here, of course; otherwise, one might spend far too long at this. Here, again, the planning schedule will be of great help in keeping one on track.

Interdisciplinary Work

I would strongly encourage attempts to answer whatever important questions arise even when this involves incursions into another discipline. An important question should not be ignored simply because it leads outside the traditional boundary of one's discipline. My own view on the subject was set out as follows in the ESRI Annual Report for the year ended 31 March 1972:

"The research worker, from whatever discipline, would be expected to consider the whole range of questions relating to his subject of research, even if some of these questions lay outside his own discipline. The resulting effort to become acquainted with other disciplines, and the response in terms of help and criticism from these other disciplines, would involve all disciplines in reaching out towards each other."

Professor Phelps Brown put the same thought in the following words:

"Let the scope of our inquiries be determined not by the customary blinkering of our field of view but by what the subject matter presents. Where an economic problem arises, let us observe whatever seems significant, and follow clues to causes wherever they may lead."

I must confess, however, that the view I am advancing here is an ideal to be aimed at rather than one that I can claim to have always followed in my research to date. I am, however, increasingly worried about the continued specialisation in intellectual disciplines and the widespread reticence of experts in treating a subject other than their speciality. If one looks through the economics books, for instance, of even fifty years ago, one will find a great deal of discussion of general issues interspersed with technical problems. For example, in a chapter on the division of labour in his Principles of Economics, Marshall devotes a few pages of great wisdom to the need for relaxation among persons engaged

in intellectual work and how that relaxation might best be taken. No economist would dare include such a discussion in his work nowadays, and, if he did, he would be asked how he had measured relaxation. Are we sacrificing wisdom in our quest for knowledge? However, I would not wish my remarks here to be construed as lending support to an undisciplined approach to social studies. A justifiable retreat from continued specialisation may, unfortunately, provoke an over-reaction, where social scientists feel called on to pronounce on any subject "off the top of their head". Such a propagandist approach is equally undesirable. In preserving a balance, we would all be greatly helped by the development of a distinctive philosophy of the social sciences - a challenge that I hope will be taken up by your generation.

Techniques

On the subject of techniques, my own approach is to use the simplest technique that will solve the problem. I am not enamoured of technique for technique's sake, and I never believe in using a sledgehammer to crack a nut. At a more fundamental level, I would encourage the use of direct observation rather than technique wherever this is possible. As I pointed out in my Academy paper, I believe that the present malaise in economics arises from excessive emphasis on formal theorising to the neglect of the empirical foundation. Far too much effort goes into the derivation of the formal properties of models, based on assumptions that have not been validated, rather than first establishing the empirical validity of the assumptions. (Example: factor substitution).

Confidence

Self-doubt is the corrosive enemy of good research. It is important, therefore, that you try to sustain your confidence in the course of a project. Confidence can be maintained by working within

your limitations. If you do, you will eventually transcend them. On the other hand, if you aim far beyond your capabilities, the likelihood is that you will become discouraged and achieve nothing - "the best is the enemy of the good".

In general, young people attach too much importance to the complex, and not enough to the simple aspects. At Oxford, seminars were sometimes given by dons and sometimes by students. The essential difference between the papers was that the don did not hesitate to say the simple and obvious thing, if it were important and true, so that his paper was relatively easy to follow and, moreover, dealt with the really significant questions in the area. On the other hand, students normally lacked the confidence to say the simple thing, and felt that they would look foolish if they did not deal with the abstruse aspects. Hence, they very often lost sight of matters of great consequence. (Example: demand curve.) Moreover, whenever you find yourself stalled in explaining why something happens, turn your attention to describing how it happens. The work involved will bolster your morale and may also give you a fresh perspective on the causality question.

3. Writing-up and Publication

Although I am dealing with it at this stage, I must emphasise that writing-up should begin at the very early stages of a project. From the beginning I try to carry in my mind a vision of the finished manuscript, a vision that is, of course, ever-changing. Moreover, in the course of a project I would never go for longer than a month without writing up a chapter or a section based on the work during that month. I may subsequently re-write these drafts to such an extent that they would have no similarity whatever with the final version, but, even so, they provide invaluable buildingblocks. Scaffolding is essential to the construction of a building, even though it is not part of the finished structure.

I find writing the most valuable of all aids to thinking about, and organising, a project. Frequently, it is only by writing up something that I realise how badly I have thought the subject through. In addition, writing is the way in which you communicate your findings. No matter how good your research may be, it is useless if you cannot communicate it, at least, with clarity and, hopefully, with elegance. I defy you to mention one of the great authorities of the social sciences who was not a forceful writer (Keynes, Freud, Marshall, Marx, etc).

A research worker should, therefore, take a craftsman's pride in his writing. I would never show my material to anyone as a first draft until I had done at least three drafts myself. Most of the poor writers I know seem to write one draft and leave it at that, whereas, in fact, they, if anything, would need to do four or five drafts. One should always go back on the first or second draft in a highly critical fashion, and examine every paragraph, sentence and word with a view to brevity, clarity and elegance. Much of the material I read could be reduced by at least one-third or even a half, and it would be a great saving of all our time if research workers took a bit more care in that regard. (Example: structure of DNA).

It is rather extraordinary to me that people assume so readily that they are able to write without any real training on this aspect, even though they spend years in preparation for the research itself. The nature of education at the undergraduate level in most British universities (where there is a weekly essay), or in America (where there are many term papers) is a great help to the prospective research worker in learning to organise and

write-up material. Unfortunately, in Ireland, not nearly enough attention is given to this in undergraduate teaching, or at least it was so in my time.

As regards the organisation of a paper, you might find Darwin's approach useful, as I do. His practice was to begin by listing all the chapters of the book or paper. He would then set out the major sub-headings of each chapter. Under each sub-heading he would then write a few sentences, each of which would subsequently be developed into a paragraph. Proceeding in this way, he was able to maintain a coherent view of the whole while fitting together the parts in a systematic and logical fashion. If one has already written, as I suggested, draft chapters during the progress of the research, Darwin's method is an invaluable and time-saving way of doing the first full draft.

Apart from the organisation of the material, the actual style of writing can greatly enhance, or alternatively greatly detract from, the impact of the study. A well-chosen simile or metaphor can often clinch a point more effectively than pages of mathematics. Who can forget Marshall's comparison of the joint determination of price by supply and demand with the blades of a scissors? I am sure that, for many of his readers, Keynes destroyed the classical theory of interest as much by this quotation from Ibsen as by his arguments:

"The wild duck has dived down to the bottom - as deep as she can get - and bitten fast hold of the weed and tangle and all the rubbish that is down there, and it would need an extraordinarily clever dog to dive after and fish her up again."

When one has forgotten Keynes's intricate arguments on the rate of interest, these lines remain in the memory. Nonetheless, rhetoric should never be used as a substitute for good arguments. Moreover, there is nothing worse than a badly-chosen simile or

metaphor. Again, this is a case where the thought processes must be closely allied to the ability to write well.

It is always worth paying heed to the criticism of colleagues that something is not clear. If you have failed to make it clear to someone in your own field, then you can be certain that it is not clear to people in general. No matter how well you feel you have expressed it, you should reconsider whether the idea itself is correct and, if so, whether it can be better expressed.

Before I became Director I would not have thought it necessary to say anything about the need for publication, since I felt that publication would be an obvious and immediate goal for every research worker. However, experience as Director has taught me that this is not necessarily so. Some people would like to work in a field for many years before publishing anything. I have absolutely no objection to a research worker taking a five- or even a ten-year view of his area. However, it is vital that along the way he should bring out every year or two an important publication on an aspect of the area. "Great problems are solved by being broken down into little ones."

Publication is obviously important for career reasons. From an intellectual point of view, publication has a profound effect on one's thinking. Nothing concentrates the mind like death, and publication is a form of death. The knowledge that one's work is going to appear before the public at large, and be subject to criticism from any quarter - and often without quarter - concentrates the mind enormously. Publication also helps to bolster the morale. If a research worker can produce a publishable paper

every year or two, this is a great boost to his confidence. On the other hand, if he goes for five years or more without publishing, his confidence may go to pieces.

Concluding Remarks

Looking beyond the individual project to research as a career, I would say that its major challenge is discouragement. In most ways, research is an easier life than business or the public service where one is under day-to-day, or even hour-to-hour, pressure. However, in such jobs, and also in university teaching, one can go home at the end of the day with a sense of accomplishment. No such feeling of satisfaction comes to the research worker. He may go on for weeks and months without feeling that he has achieved anything. In fact, invariably in the middle of a research project, I wake up some morning, having already done several months' work, with the firm conclusion that what I have been doing amounts to nothing. All research workers experience this, and the feeling may last for several days. There is only one answer to it and that is to go on. Eventually, confidence will be restored. But it is a lonely life. As Joseph O'Malley said recently, in an article in the Sunday Independent, research requires a temperament as well as a talent. Not everyone with the intelligence and technical skills for research has the necessary temperament.

But I will not conclude on that negative note. Research, for those of us who like it, is a most exciting life. Few have expressed this better than Lonergan in the following paragraph:

"Deep within us all, emergent when the noise of other appetites is stilled, there is a drive to know, to understand, to see why, to discover the reason, to find the cause, to explain. Just what is wanted,

has many names. In what precisely it consists, is a matter of dispute. But the fact of inquiry is beyond all doubt. It can absorb a man. It can keep him for hours, day after day, year after year, in a narrow prison of his study or his laboratory. It can send him on dangerous voyages of exploration. It can withdraw him from other interests, other pursuits, other pleasures, other achievements. It can fill his waking thoughts, hide from him the world of ordinary affairs, invade the very fabric of his dreams. It can demand endless sacrifices that are made without regret though there is only the hope, never a certain promise, of success. What better symbol could one find for this obscure, exigent, imperious drive, than a man, naked, running, excitedly crying, 'I've got it!'