William Thomson

Youngsub Chun and Christopher P. Chambers

Questions asked by Youngsub Chun and Chris Chambers
Q: Most of us know the story, but would you explain how a Frenchman ended up with the name William Thomson?
A: Having a Scottish paternal grandfather has a lot to do with it.
Q: How did you get interested in fairness?
A: How can one not be interested in fairness? Open any newspaper, turn on your television! Most conflicts around the world are caused by unfair treatment of certain individuals, populations, or by perceptions of unfair treatment. It is the lack of interest in distributional issues among much of the economics profession that needs to be explained.

I conjecture that an important reason why economists shy away from the topic is the fear that value judgments would have to be made, and value judgments are unscientific. Yet, understanding the logical implications of value judgments is value-free scientific work. Value judgments will ultimately have to be made but the economist can leave them to the policy maker, the bankruptcy judge, the school administrator, …, the citizenry at large.
Q: In your view, how does the theory of fair allocation relate to welfare economics and public economics?
A: I am not sure what the canonical definitions of welfare economics and public economics are. Perhaps public economics has traditionally been focused on taxation, and it is usually studied in the context of market economies, where prices are used to allo-

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cate resources. Taxation and the financing of public goods are the essential concern. The theory of fair allocation, as I perceive the subject anyway, encompasses many problems of distribution of privately appropriable resources when these resources are described in a concrete way.

Consider the following array of problems. A collection of indivisible resources, often called "objects," has to be distributed to a group of people, say offices to coworkers in an office building. An interval of time has to be partitioned into subintervals during which a service is provided to a group of people; only one person can be served at a given time, and each person values subintervals differently, their lengths being one but not the only consideration. A facility has to be located on a road network; its users are distributed at different points of the network, and each user values its proximity to the facility differently. The cost of a service has to be shared among a group of people when their uses of it are nested; it could be a cab that coworkers hire to return home and they are dropped off at their respective homes; or an irrigation ditch that farmers use jointly and the costs of bringing water to each of them separately differ, due to their greater or lesser proximity to the main channel.

These are just a few examples of situations where fairness issues arise, but fairness actually come up in almost any type of allocation problem. These types of microeconomics problems, specified at this level of detail—I should perhaps speak of nanoeconomics—are not within the purview of standard welfare economics and public economics.

Much of the theory of fair allocation has been recently written from the design viewpoint, and the axiomatic method has been a principal mode of investigation. Again, this approach is not typical in traditional welfare economics and public choice. When engaged in this kind of work, we start from scratch. We try not to be too influenced by common practice, although of course we draw lessons from it.

Q: What was the environment like at Ecole Polytechnique? At Stanford? How did your education shape your way of thinking about economic problems?

A: At that time, economics education was very limited in French schools in general. At the high-school level, there was no such thing as economics.

At Ecole Polytechnique, the offerings amounted to very little. One course was compulsory, on national accounting. A few very specialized seminars were also offered. I attended one on general equilibrium, in which I discovered G. Debreu's *Theory of value*. I was surprised that economics could be treated in such a formal way. The rigor appealed to me.

In the early 70s, Stanford was one of the rare places where you could learn about game theory, but it wasn't part of the regular Ph.D. program yet. I am not sure it was part of any Ph.D. program anywhere in the world at that point actually. A main attraction was the IMSSS workshops, which lasted a significant part of each summer. They were devoted to general equilibrium and game theory. Some of the most prominent people in these fields attended them. K. Arrow, F. Hahn, and R. Aumann were regular participants. G. Debreu also made appearances.

Q: As one of the pioneers of the axiomatic approach to economic environments, we were hoping you would be able to explain your ideas behind the basics of the approach, how it should be implemented, and why it is useful for economic science.

A: I published an essay about the axiomatic method (Thomson 2001a), and I am currently at work on a larger, more ambitious and comprehensive, pedagogical exposition of it (Thomson 2021). I present my view of what the axiomatic program is about, and how to conduct axiomatic work. I also discuss the pitfalls of axiomatic analysis, how *not to do* axiomatic work.

Why it is useful to economic analysis should be clear. I cannot think of a better way of addressing questions of design. Economic institutions are not God-given, they are man-made. The axiomatic method provides complete answers to design questions. It starts with the axioms, which are the reasons, written in mathematical form, why we are interested in allocations and allocation rules. An axiomatic study addresses the compatibility of social objectives embodied in a list of axioms, when imposed in various combinations. When axioms in a list are compatible, it usually offers a description of all the rules satisfying them. The goal of the axiomatic program as a whole is to trace out, for each class of problems that one may encounter, the boundary that separates those lists of properties that can be met together from those that cannot, and when compatible, to obtain as explicit as possible a description of the rules that satisfy them all.

Q: You have demonstrated a particular interest in "variable population axioms", that is, in axioms that relate the behavior of economic systems across populations. Can you explain us a bit of the ideas of these ideas. For example, where does the idea of population monotonicity come from?

A: It is the idea of solidarity that underlies most of the monotonicity axioms. Put abstractly, it says that when a parameter of an economy changes and if no one is responsible for the change—no one deserves any credit when it is socially beneficial (that is, permits a Pareto improvement) or any blame when it is not (that is, when no such improvement is possible)—everyone's welfare should be affected in the same direction: if someone is made better off, no one should be made worse off. The parameter that changes can be resources, technologies, preferences, population…In each of these applications, solidarity takes the form of a separate axiom, often a monotonicity axiom if the space to which the parameter belongs is equipped with an order structure. Population monotonicity is such an axiom.

Solidarity is enshrined in most religions and in the moral codes of most societies. It is part of our ambient ideology. Passing from the general idea of solidarity to our specific solidarity axioms requires only a small step. I don't see a significant conceptual difference between its application to variations in resources, technologies, or preferences, and its application to variations in populations. It is true that the fact that population is a discrete variable makes it somewhat special because it is a little easier to imagine applying different rules for different populations than to doing so for different endowments of unproduced resources or to different technologies. Mathematically, this has significant implications, however, because one has to work in a model in which the dimension of the space of allocations is not fixed. It depends on the population size. This certainly makes it more challenging.

Q: You are known for having spent long hours advising your students. Did you apply any of your ideas on allocating time to this particular problem? How would you suggest scientists balance their time between students and research?

A: I wish I could say that I had invoked the axiomatic method to solve this kind of problems, or any of the problems I have faced in my daily life, but there probably would have been too many variables to take into account. The axiomatic method does have limitations, and one of them is that the problems to be solved should not be too complex; their description should not involve too many parameters.

As for how a young professor should balance teaching and research, I would say focus on your research. Time goes very fast, the publication process is very slow and subject to a lot of randomness. Soon, requests for letters evaluating your work will be sent out by your department to the best scholars in your field. So, make yourself visible in the profession: circulate your research, attend conferences, do a good job with your refereeing assignments, although here I would say, do not feel obliged to always say yes. Getting too deeply involved in teaching is dangerous, and advising graduate students should not be your priority. If you can engage in some joint research with the better ones, you may do so however.

Q: Do you have any advice for young researchers interested in the axiomatic approach?

A: Earlier, I mentioned two pedagogical pieces on the subject, one journal article and the other a book size manuscript. In both, I discuss how I think axiomatic work should be carried out. I also address my pet peeves about the way it is sometimes done. So here, I will only emphasize one point, which is that the focus should be on the economic content of axioms. Will the man on the street understand the idea that underly them? Will he endorse it? The answers should be yes. At least, there should circumstances in which he would. The relevance of each axiom does depend on the context.

However, one should also be ready to accept the technical help that some axioms provide. Occasionally, in order to make progress on some otherwise intractable model, one has to invoke properties that may not be as compelling as one would like. I find that to be a reasonable research strategy. With time, as one' s understanding of the subject progresses, one may be able to do without these crutches.

Q: If there is a student interested in what you are doing, which papers do you recommend to read? Why?

A: I was fascinated by Nash (1950)'s paper on the bargaining problem when I was a graduate student and I suspect that this paper remains an excellent introduction. Arrow (1951), Sen (1970) were important to me, and I recommend the more recent books by Moulin (1988) and Young (1994). Several areas of economic theory have developed in the axiomatic mode and showcase the power of the method: two-sided matching, with applications to the assignment of medical students to residency programs; "school choice," which has to do with the assignment of students to the various schools in a school district when each student—rather his or her parents—has in mind a ranking of the schools and each school has its own ranking over students; allocating scarce organs to patients waiting for a transplant. In none of these applications money changes hands, but in the next two it does: assigning workers to jobs and specifying their salaries, and allocating frequency bands to telecommunication companies. All of these areas have been the object of important design literatures, and axiomatic reasoning has been critical in their development.

These are many situations where making allocation decisions through prices is not a good option, or is not an option at all. Sometimes markets are considered unethical; sometimes they are even illegal. Alternatives to markets have to be invented. Researchers in these areas refer to their program as "market design" but routinely open their seminar presentations or their research papers by pointing out those multiple situations where markets do not have the good properties that they enjoy in our textbooks or are simply unavailable, and that something had to be done about it. An expression that would better describe what they contribute to is "alternatives-to-markets design."

It is not only resource allocation that is the object of interesting axiomatic work. Take the study of ranking methods for example. Rankings are more and more pervasive in our daily life. An ever widening range of important decisions are being made on the basis of rankings, of and by individuals, organizations, societies. That certainly includes academia: journals are ranked, universities are ranked, economics programs are ranked, researchers are ranked. Whether we like it or not, rankings play important roles, implicitly or explicitly, in promotion and tenure decisions (think of the profession's obsession with the "top-five"[1]), and in the allocation of research funds. The axiomatic literatures on ranking and voting are undergoing very interesting developments.

Q: In your book, you give guidance to young economists on how to write papers. Can you go one step further and tell young economists how to choose a topic and find interesting papers? Can you also make suggestions as to which field young economists should work on?

A: I don't have a recipe to find a good topic, but I would recommend to develop early on the habit of reading papers not to find out what's in them, but rather what's not there, what's missing. Because what's missing can be the subject of one's own paper.

Also, we should not ignore the discomfort we sometimes experience about some of the assumptions underlying a model when we are first introduced to it. We may feel that an author or a speaker's approach to a problem suffered from some important limitations; yet, we soon forget about our hesitation and go along with the approach. We should challenge it instead.

But the best topics, certainly the ones that are the most exciting to work on, emerge spontaneously and randomly, in the course of a conversation, when attending a seminar, when raking one's brain for homework or exam questions; also, when engaging in activities that are unrelated to research, for example, when dealing with bureaucracies to enroll our kids in school or standing in line to buy a concert ticket or a service.

Q: What do you think of the future of social choice? How about economic theory in general?

A: It is often said that economic theory is in decline. The power of new computational techniques and data availability has greatly increased the attractiveness of empirical work. I do not see a fundamental conflict between these approaches. Design work can be complemented with empirical and experimental work. The allocation rules that

[1] I should say "unhealthy" obsession.

we design have to be implementable on computers and opportunities for computer scientists and economists to interact have multiplied considerably in recent years. These rules should also be put to the test in actual practice. There may well be things that we worry about in our theoretical work but are not perceived as much of a problem in actuality, and conversely, problems that we dismissed as not being that important and that people care a lot about even though they occur rarely.

"Abstract" Arrovian social choice theory has often been criticized for mostly delivering negative results. But investigators should not be held responsible for the answers to the questions they ask if the questions are legitimate. It turns out that whether one ends up with positive or negative results depends in important ways on the structure of the set of alternatives, and on the properties that one can assume preferences satisfy. For the unstructured sets of abstract social choice, one is much more likely to end up with incompatible properties of social choice mappings than for the highly structured models that arise in the study of concretely specified resource allocation problems. These structures are reflected in restrictions on preferences that would not be meaningful otherwise; they allow us to formulate requirements of good behavior of allocation rules that one could not even discuss without these structures, and to define rules that could not be defined. Some rules often enjoy various combinations of these properties that would have no counterpart in unstructured models or whose counterparts would be incompatible. Of course, the introduction of "economic" structures into the model of traditional social choice theory do not necessarily lead to positive results. But the opposite would have been surprising. So theory allows us to understand that it is not good enough to speak about the desirability of social objectives in some abstract way.

Q: You have produced more than 50 Ph.D. students. What is the best way of teaching Ph.D. students? Can you give your advice to young professors?

A: I do not want to presume that I know enough about this to give advice to young professors. I have had the privilege of working in a top-notch department with a reputation for high-level theory and I will comment instead on the role that this has played in recruiting good students.

Lionel McKenzie started it all. He set the tone, established the standards of rigor that are the hallmark of our department. Thanks to him, generations of well-trained students have come to us to pursue Ph.D.'s. I have been the beneficiary of his pioneering work.

My book of pedagogical essays (Thomson 2001b) has a chapter entitled "Being a grad student in economics," and many of the things I collected there were lessons I drew from the mistakes I made when I was a graduate student. I should simply refer to that chapter, but keywords are "initiative" and "follow-up".

You don't have to be invited to show up at your advisor's door to tell them about the ideas you just got from reading an exciting paper; however, to make the best use of your advisor as a sounding board, be ready to present these ideas in as clear a way as possible: write down formally the definitions you will need; work out an example to illustrate a conjecture your formulated; refresh your memory of papers that seem relevant; prepare what you will write on the board. Also, don't forget that very soon you will be the expert on your subject, and you will have to guide your

advisor as much as your advisor will guide you. These conversations do not have to be very long by the way. It is more important that they be frequent. And even more important, follow-up on these exchanges: read the suggested papers; try to prove the result that you discussed, to construct a counterexample to a conjecture, or look for the data to support some hypothesis you formulated.

Here are a few things that I feel students do not do enough of: reading papers that are not in their area of research; auditing classes when they are done with their course work; meeting with outside speakers and asking questions during their seminars; interacting with students in other fields. Also, faculty will certainly be very grateful for any help hosting visitors, organizing workshops and special events.

I said it before, but it bears repeating that as a young professor, you shouldn't get swallowed up by teaching, departmental service, the various committees on which you may be asked to serve, and student advising. It is more important for you to make yourself visible in the profession at large. Advertise your findings in your webpage and by posting them on the internet. Participate in conferences and perform your refereeing duties with diligence. However, learn to say no. Don't accept too many assignments; in particular, don't accept refereeing assignments that are too far afield from what you know.

Don't expect that every decision made about your submitted papers will be fair. There is much randomness to the publication process. Your papers will be rejected when they deserve to be accepted more often that the opposite will be true: as a young and unproved author, you won't be given the benefit of the doubt. Don't be discouraged. Your paper will eventually be accepted but it will take much longer than you expect.

Q: In recent years, you have spent a lot of time on the bankruptcy problem. Is there any special reason why you like this problem so much?

A: There are many special reasons.

First, let me use a slightly more general term than bankruptcy because bankruptcy is just an application of the formal model. I will talk about a group of people having claims on a resource when there isn't enough of the resource to fully honor all of their claims, and use the expression "claims problem."

It concerns an issue that all societies have to face, always have had to face. It is discussed in antiquity and medieval literature. How can one not be intrigued by Aristotle's view of it, the "contested garment problem," the "three-wives problem," and the recommendations the Talmud makes for them, by the "four-son inheritance problem" of Ibz Ezra, what he proposed as a solution for it, or by Rabad's generalization of this proposal?

Moreover, the data of a claims problem can be interpreted in the context of taxation, and taxation is also an issue that all societies have to deal with, that affects every citizen, from the man on the street to the investment banker.

Mathematically, claims problems constitute an extremely simple class: for an n-claimant problem, $n + 1$ numbers, a list of claims and an amount to divide, plus an inequality: the sum of the claims should exceed the endowment. Less than a line suffices to define it. You do not need any background in economics to understand

what it's about. Everyone immediately gets it, economists and non-economists alike. And everyone has an opinion about it.

Next, and in part because of the simplicity of the model, many interesting rules can be defined to solve claims problems that enjoy a great variety of appealing properties. The richness of the inventory of available rules makes it in fact particularly critical, more critical than for other types of problems, that one should find ways of differentiating among them. The axiomatic method is the ideal way of evaluating them. And as always, it allows us to go further than what is done in practice, to find out if something better can be done, to identify those maximal lists of desirable properties that can be satisfied together; and when the properties are parameterized, to understand the tradeoffs between these parameters.

One could of course argue that the base model that I described is too simple, but it does cover a situation that is common enough. Again, think about bankruptcy and taxation. Besides, it has been enriched in countless interesting ways. I cannot think of a class of problems that is so important and so amenable to axiomatic analysis. I started working on claims problems 15 years ago, and if someone had told me that I would end up writing a book on the subject, I would have laughed: a book about $n + 1$ numbers related by an inequality?

Many positive results have been obtained but of course there are incompatibilities too. For some models, it turns out that very elementary axiom systems either lead to impossibilities or to very unpalatable conclusions. In other contexts, we often find that all the decision power should be given to a single person, leading to what we call "dictatorships." Monotonicity properties are particularly demanding in most contexts—strong domain restrictions often have to be imposed for positive results—but such properties are easily satisfied in the context of claims problems. Of course, it has to do with the model being described in terms of a small number of real-valued parameters. But the modeling is natural, and it does allow a large number of compatibility results. It is rewarding to work with a model that is blessed with so many good news.

So, claims problems are important; there is mystery, interesting mathematics, and the entry cost for researchers is extremely low. In fact, claims problems are an extremely powerful vehicle to introduce the axiomatic method to neophytes: they can only be described as a little pedagogical paradise [2].

Q: In the early days of your career, you focused on the axiomatic theory of bargaining. What motivated you to the study this class of problems. You do not seem to have done much research on the bargaining problem recently. What is the reason?

A: What is appealing about Nash's (1950) formulation of what we call Nash's bargaining problem is that it is very general, so in principle, any solution to the bargaining problem can be used to solve any type of allocation problem that one is interested in. It suffices to map the allocation problem into a bargaining problem, apply the chosen bargaining solution to get an outcome in utility space, and then return to the physical space of the allocation problem by taking its inverse image under the profile of utility

[2] For a survey see Thomson (2019)

functions. The study of the bargaining problem has been the object of considerable axiomatic analysis, so we have a very good understanding of which social objectives can be met jointly in this context.

There are several difficulties with that approach however. One is that the mapping from allocation problems to bargaining requires that choices be made and there are often more than one, especially as pertains to the disagreement point. And the eventual decisions to which we are led will often depend on these choices.

Another is that even if a solution to bargaining has been characterized in terms of a list of axioms expressing ideas that one finds particular compelling in one's particular application, the class of problems obtained as the image of the class of allocation problems under investigation may not be large enough to guarantee that this characterization holds.

A third is that the informational basis of allocation theory is quite different from what it is in Nash bargaining. First, in the passage from allocation problems to bargaining problems, the concrete "economic" structure of the space of alternatives is lost. As a result, two instances of the allocation problems under consideration may be mapped into the same bargaining problem. Yet, we may feel that the specific way in which they differ is quite relevant; these differences have to be ignored when working in the space of bargaining problems.

On the other hand, because Nash's bargaining problem is formulated in utility space whereas for most allocation problems, only the rankings of alternatives by the participants are available or deemed relevant or practical, some "cardinalization" of preferences need to be introduced to convert an allocation problem into a bargaining problem, that is, cardinal numerical representations of preferences have to be chosen. Obviously, the allocation that is eventually chosen will depend on that choice, injecting some amount of arbitrariness in the final choice.

Q: Has any graduate student not been able to complete the degree with you? What was the reason? Any recommendation to a beginning student?

A: I recall only one student who started working with me and finished with someone else. After a while, it became apparent that the match wasn't good.

Q: What are the good conditions for a good economics department?

A: I already mentioned the role played by Lionel McKenzie and the intellectual climate that he fostered, so let me mention more mundane things. How a department is physically configured is not to be neglected. The way ours is organized, on a single floor, with all offices opening on a circular corridor, and with two large rooms in the middle that we use for seminars and classes, is most conducive to people bumping into each other all the time. It would be even better if all of the graduate students' offices were also on that floor, but that would probably be impractical. Unfortunately, there are not enough office for that.

Having an easily accessible lounge with decent furniture, a coffee machine, and a blackboard is also a must. I have often been surprised to discover, when I visit other schools, that they only have a small coffee room, with no or little furniture, and that people have to stand in front of the coffee machine waiting for their turn; sometimes only a few people fit in the room.

Congeniality is of course the most important thing. People have to get along. The culture of the department has to be one of mutual respect. Exchanges between faculty and students have to be encouraged and facilitated. All of these things are true at Rochester.

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