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Otis Dudley Duncan's Legacy: The Demographic Approach to Quantitative Reasoning in Social Science

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Otis Dudley Duncan's Legacy:

The Demographic Approach to Quantitative Reasoning in Social Science

"But sociology is not like physics. Nothing but physics is like physics, because any understanding of the world that is like the physicist's understanding becomes part of physics…" (Otis Dudley Duncan. 1984. *Notes on Social Measurement*, p.169.)

Population Thinking versus Typological Thinking

I would like to begin my discussion of Duncan's demographic approach to quantitative reasoning with a broad but crude overview of the history of science. The history of science is dominated by the history of physical science. Plato, perhaps the most important philosopher in ancient Greece, has had enormous influence on the way physical science has been conceived and practiced. Let me go a step further: Plato has had a lasting influence on western philosophy in general, including social science. To quote Whitehead (1861- 1947), a modern mathematician and philosopher, "The safest general characterization of the European philosophical tradition is that it consists in a series of footnotes to Plato" (cited in Mayr 1982, p38).

What made Plato so important in the history of science? Plato's main contribution to science, then called "natural philosophy" in Greek antiquity, was the way he defined "true knowledge", or "truths." One key element in his scheme of epistemology is the *separation* between the "world of being" (or the world of Forms) and the "world of becoming" (or the world of things). The "world of being" is where true knowledge resides. The "world of becoming" is what we actually observe in real life. Plato's definition of true knowledge—or science in today's language—requires universal and perpetual validity of true knowledge. It is *not* knowledge about concrete objects or phenomena in the world of becoming, because that knowledge would be unreliable. Truths must be at a higher level—knowledge about the world of being. The scientist's (philosopher's) task is to go beyond things that are observed, sensed, and experienced (i.e., world of becoming) to gain understanding of truths in the world of being. Laws are assumed to preexist and be immortal, as they were created rationally by the Creator. This definition of true knowledge underscores the word "discovery" — almost synonymous with scientific endeavor— meaning that great truths are hidden in nature, waiting to be discovered by scientists. This is the teleological aspect of science.[1]

Let me use a concrete example to illustrate Plato's point. To understand the true properties of circles, Plato would argue, it would be wrong to study any circles we observe in daily life or those we can make with our drawing devices, because none of the circles we observe in real life or can draw with the best instrument meets the criteria of a perfect, ideal circle.[2] The perfect circle exists nowhere except in the philosopher's mind. Understanding just one perfect, hypothetical circle, gives us true knowledge about all circles.

Thus, natural science takes on the "world of being" as the true reality, which we never actually observe but assume to exist independent of the world of becoming. Plato's world of being consists of discontinuous, abstract ideas or Forms. To Plato, the observed variation in real life has a simple explanation: objects in the world of becoming are poor replicas of the world of being. Mayr (1982, 2001)

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[1] By the way, because Duncan never accepted the teleological aspect of social science, he denied that he had any real "discovery" to speak of in his National Academy of Sciences autobiography (Duncan 1974, p.8).
[2] There is a reference to the example of the perfect circle in Plato's Letter VII (Plato 1997, pp.1659-1060). However, the authenticity of the letter's authorship is in doubt (Plato 1997, p.1634).

called this mindset "typological thinking."[3] According to typological thinking, natural science should focus on typical phenomena, such as the typical human body, the typical falling object, and the typical circle. Furthermore, scientists should try to isolate extraneous, confounding factors, such as temperature, size, and location, when studying these typical phenomena. A strong assumption, which has worked well in natural science, is that once we understand the typical phenomena, we can generalize the knowledge to individual, concrete cases.

Adhering to Plato's typological thinking in the physical sciences has resulted in a great success story. This kind of thinking also resolved the potential conflict between science and religion for a long time, as natural laws, from this perspective, provide sufficient, physical, or immediate causes governing natural objects, instead of relying on "final causes" directly from the God. One can trace highly successful examples in Copernicus, Galileo, and Newton to this line of reasoning. In typological thinking, real life deviations from the perfect world of being are considered nuisances, imperfections in making copies, and thus trivial and ignorable, unworthy of a true scientist's attention. The core of this philosophy is that a scientist can make a great scientific discovery only if he/she knows how to go beyond the nuisances posed by deviations in the world of being.

Plato's definition of science dominated the scientific community, and to a large extent, still does. In the mid nineteenth century, however, the English biologist Charles Darwin started a revolution. While Darwin is remembered today mostly for his evolutionary theory of natural selection, what concerns us here is his thinking about population. For Darwin, deviations are no longer considered as unreal, undesirable, and minor as they were to Plato. Rather, they are the very sources of evolution and have become the most interesting aspect of scientific inquiry.

For Darwin, variation *is* reality, not some undesirable error on the part of the observer. In his book *On the Origins of Species* (1859), the first two chapters are entitled "Variation under Nature" and "Variation under Domestication." What is important here is the individual, not just the type. Offspring of the same parents are different from each other. Variation is inheritable from generation to generation. Variation is fundamental to natural selection: in today's language, abundant genetic variation is produced in every generation, but only relatively few individuals survive and reproduce.

Darwin was a biologist. It was his cousin, Francis Galton, who introduced Darwin's population thinking to social science. Like Darwin, Galton was from a wealthy family. Disenchanted with university life, Galton traveled a lot and observed that people were very different in every possible way, from height to intelligence to beauty. To Galton, the value of averages is limited. "Individual differences… were almost the only thing of interest" (Hilts 1973, p.221). He then began to apply population thinking in Darwin's biology to the study of humans, using the tools of statistics.

Galton was not the first scientist to apply statistics to the study of humans. Adolphe Quételet, a Belgium mathematician, had already extended probability theories of measurement, those associated with the normal distribution, to social phenomena, under the label of "social physics" (Quételet 1842). Focusing his social physics on the "average man," Quételet was attracted to the idea that averages in a population or subpopulation seem to be stable and predictable, despite the apparent large variations and uncertainties in individuals' behaviors. That is, averages seem to satisfy Plato's high standards of invariance and absoluteness for universal truths.

Radically departing from Quételet, Galton was concerned with "how the quality is distributed" (Galton 1889, pp.35-36). Hence, Galton made an important advance over Quetelet in treating variability as a serious subject matter. For this reason, Galton changed the traditional term "probable error" to "probable deviation," as the term "error" seemed to imply undesirable, unreal, small quantities caused by measurement (1889). For Galton, deviations, as *one* property of a distribution, were just as important as averages in supplying information about reality. Galton's concern with individual differences and

[3] Duncan admired Mayr's work. In an email (Duncan to Yu Xie, May 10, 2004), Duncan referred to an article by Mayr (2001) and later commenting that "[t]he statement on population vs. typology is indeed magnificent. That old guy was pretty canny. I feel privileged to have heard the article to which I referred when it was delivered" (Duncan's email to Yu Xie, May 23, 2004).

variability rather than with averages eventually led him to the epoch-making discovery of "regression" and "correlation" (Hilts 1973).

We should notice that Galton had changed the meaning of variations in the study of humans. For him, they are a part of reality. With Galton, social science takes Plato's "world of becoming" as the true reality to be studied. That is, social science is interested in the complete distribution of individual, deviant cases. Before a scientist conducts a study, it is important first to define the population being studied. Otherwise, results are not interpretable. Because elements in a population all differ from one another, the results can differ widely depending on who is included in the study. This premise is the basis for scientific sampling.

Individuals can vary a lot in their behaviors and opinions. The social scientist's job is to describe the regularities in such variations. For the physical scientist, variations are undesirable measurement errors, i.e., extraneous noises to be eliminated. For the social scientist, variations are the very essence of social reality.

The core differences between typological thinking and population thinking have serious consequences for modern statistics. In typological thinking, deviations from the mean are simply "errors," with the mean approaching the true cause. That is, the true cause is constant, but what we actually observe is contaminated by measurement error. Suppose that we know in advance that the speed of sound in an ideal condition is a constant. Each time we measure it with an instrument, the result is slightly different. If we take repeated measures, we obtain a series of numbers. How should we treat the series of numbers that seem to be different? To satisfy typological thinking, probability statisticians found a solution – the law of large numbers. The law of large numbers states that as the number of observations increases, the average value based on these observations becomes more and more reliable and stable, approaching the true value. The central limit theorem then further developed this idea by showing that these estimates of the average are normally distributed. Of course, the assumption for these two statistical theories to hold is that the noise that causes the deviations from the true value is small, independent, and accidental. Technically, such deviations are called *measurement errors*.

In population thinking, deviations are the reality of substantive importance; the mean is just one property of a population. Variance is another, equally important, property. In Duncan's 1984 book *Notes on Social Measurement*, he commented on the distinction made by Jevons between a "mean" and an "average," with a mean representing the average value of observations that deviate from a true constant cause and an average representing the average value of numbers that are intrinsically different from each other. Similarly, Edgeworth made the distinction between the "mean of observations" and the "mean of statistics" (Duncan 1984, p. 108). Later in the book, Duncan contrasted the two views of statistics with a clear preference: "Whereas statistics was once known as the 'science of averages,' it is better (though incompletely) described as the 'science of variation'" (p.224).

Duncan as a Population Thinker

On numerous occasions, Duncan told me in personal communications that he was a population thinker and had preached population thinking in sociology all his life (December 7, 2002; February 16, 2004; May 10, 2004; May 23, 2004). However, it is important that we locate supporting evidence more directly in his published written work. For this purpose, I cite three examples.

<u>Example 1</u>

Duncan published many books during his career. Among them, he considered his 1984 book, *Notes on Social Measurement, Historical and Critical,* his "best book …of enduring and not merely historical value" (personal communication, September 27, 2004). Reflecting his broad and critical views on the practice of social research, the book contains his philosophy of social science. In the book (1984), Duncan makes an explicit reference to Darwin and population thinking:

> Darwin's emphasis on the variation among individuals in any natural population and the heritability of such variation actually provides the general conceptual framework for psychometrics and makes clear its affiliation with the population sciences. (Psychophysics, by contrast, has usually taken a typologically oriented interest in the species norm…and has only grudgingly conceded the existence of interindividual variation, regarding it as a nuisance rather than a primary object of inquiry.) (p. 200)

Duncan's exposure to and interest in psychometrics began relatively early in his career. In his autobiography (1974), he proudly pointed out that he introduced the psychometrics literature to Art Goldberger, a renowned econometrician, during the developmental days of path analysis and structural equations models (pp.19-20).

<u>Example 2</u>

Hauser and Duncan (1959) gave demography a classic definition: demography is "the study of the size, territorial distribution, and *composition* of population, *changes* therein, and the components of such changes"(p. 2). This definition is so precise and so profound that it is still widely used today (Xie 2000). What is most remarkable in this statement is that Hauser and Duncan explicitly included "composition of population" and "changes therein" in their definition. The inclusion reflected their belief in population thinking – that there are individual variations within a population.

Duncan's definition has allowed demography to flourish as a basic, interdisciplinary, social science that provides the empirical foundation upon which other social sciences are built. The central tenet in Duncan's approach is the primacy of *empirical* reality. Much of what we know as "statistical facts" about American society, for instance, have been provided or studied by quantitative sociologists following the demographic approach. Examples include socioeconomic inequalities by race and gender, residential segregation by race, intergenerational social mobility, trends in divorce and cohabitation, consequences of single parenthood for children, rising income inequality, and increasing economic returns to college education (Xie 2000).

<u>Example 3</u>

In an email to me (December 7, 2002), Duncan wrote:

> Here again: I'm starting to remember where I said things. See pp. 96-98 of Notes on Social Measurement for a Neyman quote and the reference to the Copernicus symposium in which his article appeared. Seems I said some pretty wise things before I got old enough to claim wisdom on the basis of age alone.

After receiving the message, I went back to the book (1984) and found the following passage by Neyman that was quoted by Duncan:

> Beginning with the nineteenth century, and increasing in the twentieth, science brought about "pluralistic" subjects of study, categories of entities satisfying certain definitions but varying in their individual properties. Technically such categories are called "populations."(p.96)

Clearly, Duncan was influenced by Neyman's observation and believed that social science is indeed a population science. Duncan was disdainful of the search for supposedly universal laws of society that would mimic those of the physical sciences, because he believed that such laws did not exist and would be meaningless.

Two Approaches to Regression and Path Analysis

Typological thinking and population thinking, two philosophical views of science, gave rise to two approaches to statistical analysis, particularly in the form of regression analysis that is the most widely used methodological tool in quantitative social science today. Lacking better labels, I call the two approaches the "Gaussian Approach" and the "Galtonian Approach."[4] Stylistically, we can draw the following contrast:

> Gaussian Approach (Typological Thinking):
> Observed Data = Constant Model + Measurement Error
>
> Galtonian Approach (Population Thinking):
> Observed Data = Systematic (between-group) Variability + Remaining (within-group) Variability

This difference between the two is very subtle, as it pertains to the interpretation, but *not* the parameter estimation, of regression.[5] After all, researchers use the same mathematical formulas and statistical packages and look up the same statistical tables for statistical inferences, regardless of their philosophical view of regression or, more likely, despite their lack of a clear view of this distinction.

An easy way to understand the distinction is to simplify the model so that we have observations around a fixed quantity:

$$y_i = \mu + \varepsilon_i \qquad (1)$$

This is the well-known measurement model. In the physical science, the scientist may know that a fixed quantity exists yet be unable to measure it due to measurement errors. Measurement theory in statistics was developed to solve the problem: under usual conditions (such as no systematic bias of the measurement instrument), the average of the repeated observations approaches the true quantity with desirable precision (Duncan 1984; Stigler 1986). In this case, the mean is the least-squares solution of the regression equation.

In a population science (such as social science), observations of y could differ from one another not only because of measurement error but also because they are inherently different members of the same population. If we force our attention onto a single quantity to estimate, we can apply the same estimation technique to estimate the population mean. In this case, $\mu = E(y)$, with individual y_i's varying from each other in a population. Even in the absence of measurement error, we would still find different values of observed y_i. In this case, ε_i represents the deviation of the *i*th observation from the population average. Because members of the same population have different values of y, it is important that a random (scientific) sample be drawn so that the sample mean can be used to estimate the population mean, which is one of many potential quantities of interest.

The first instance, in which there is a fixed, universal mechanism that generates observed data, is called the "Gaussian Approach" of regression. The second instance, in which the interest is to summarize population variability with a parsimonious description, is called the "Galtonian Approach" of regression. Duncan was keenly aware of this distinction. In *Notes on Social Measurement* (1984), Duncan borrowed Edgeworth's distinction between observations and statistics, with observations being quantities around a true cause, and statistics being different quantities in a population. Duncan further cited with approval Jevons's suggestion that we distinguish an "average" from a "mean," with the former associated with observations, and the latter with statistics (p.108). Although the two approaches use the same estimation procedure (say least-squares estimation), they differ radically in terms of interpretation, underlying assumptions, and research objectives.

If we look back at the early days of path analysis and structural equations, we can see that Duncan was clearly thinking in population terms, which was not always understood and appreciated by scholars who followed him. Let me illustrate this point by contrasting Duncan with Blalock, another

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[4] I do not know who first coined the labels. I saw their use in a letter from David Freedman to Dudley Duncan (April 25, 1986).

[5] Indeed, David Freedman kindly told me that it is "too subtle, because the positions seem to be statistically indistinguishable" (personal communication, October 28, 2005).

pioneer in sociological methodology who, under the influence of Herbert Simon, actually started to work on causal models before Duncan. This contrast was first made by Duncan himself. In one of his early letters to me (April 26, 1988) accompanying his correspondences with David Freedman, a statistician, Duncan wrote, referring to my own work on Franz Boas (Xie 1988), "The stress on the populational as opposed to the typological approach is valuable. I was totally unable to get it across to H. Blalock."

Both Duncan and Blalock were pioneers in path analysis and structural modeling, but their views concerning the use of causal models were radically different, as Duncan himself acknowledged. To Blalock, such models, at least ideally, capture universal laws that can be understood in terms of Plato's world of being. For example, Blalock asked, in his well-known book, *Causal Inferences in Nonexperimental Research* (1961), "Why not formulate our causal laws and other theories in terms of these ideal models and completely isolated systems, then noting how the real world deviates from such a model?"(p. 17). Later in his book, Blalock also remarked that "It is the regression coefficients which give us the laws of science" (p. 51). This view, which I call "Gaussian model" regression, interprets the regression as representing a single, true, law-like relationship, with the deviations of individual observations resulting from undesirable noises. Lieberson and Lynn (2001) characterize this desire to model social science after physics as "barking up the wrong branch."

Duncan's understanding of path analysis and regression models was at odds with that of Blalock. Duncan did not want to impute a "causal" interpretation into the results. In his best known work on intergenerational mobility with Peter Blau (1967), for example, the authors stated that "We are a long way from being able to make causal inferences with confidence, and schemes of this kind presented here had best be regarded as crude first approximations to adequate causal models" (p.172). In his seminal paper "Path Analysis" (Duncan 1966), Duncan emphasizes in the abstract that "Path analysis focuses on the problem of interpretation and does not purport to be a method for discovering causes" (p.1). Thus, Duncan described the Galtonian view of the regression model.

The two divergent views, Gaussian versus Galtonian, also characterize the differences between Duncan and David Freedman in a long series of communications between the two. Their correspondence began when Freedman sent Duncan a critique of path analysis in social science. Freedman originally criticized the Blau and Duncan (1967) book but later changed the target when the critique was published (Freedman 1987).

Freedman began the correspondence with a letter dated May 31, 1983 that accompanied the original version of his critique of path analysis. The crux of Freedman's critique is that structural equation models are misused in social science because they presume true causal models (in the sense of Plato's Forms) that cannot be justified. Duncan's reply (June 2, 1983) was quite gracious, stating that "Over the years I have become more aware of its various deficiencies. Some of the small increment in my wisdom is in the 1975 text [Duncan, 1975] which you are good enough to quote. There I omitted all empirical examples, not having any at hand which seemed to justify the use of the approach." Duncan also sent to Freedman the last chapter of his forthcoming 1984 book, *Notes on Social Measurement,* and pointed out an excerpt that was "written with you [i.e., Freedman] in mind."

Duncan's non-defensive reply truly impressed Freedman, who wrote back to Duncan (June 13, 1983) that "You are far more generous than I would be, in your place. I would hate to be seen as taking a pot shot at you." The two scholars kept active, often collegial correspondence for awhile and met in person a few times. One of the examples discussed in their correspondence is Hooke's law. In Freedman's view, "Regression is fine for making inferences when there is a 'Hooke's law' sort of mechanism generating the data" (Freedman to Duncan, March 3, 1986). In contrast, Duncan expressed a very different view: "If linear regression can only be justified in the 'Hooke's law' situation, then I see little point of computing regressions of income on education" (Duncan to Freedman, February 25, 1986).

Duncan laid out his views on the use of regression analysis in social science in a 5-page (single-spaced) letter to Freedman on December 6, 1985. In a remarkable passage that set himself sharply apart from Freedman, Duncan states,

> Our work is in what Neyman called the "population sciences," and statistical methods have a different meaning and function there than they do in the "exact" sciences. I wish you would write down an exemplary success story of how some meteorologist, geologist, or ecologist grappled effectively with the kind of messy observational data we have where no study can be strictly replicated, there is no single quantity to estimate, and interventions are impossible or likely to be trivial.

By the end of their correspondence, Duncan and Freedman saw each other's differences and began to accept the differences. In a letter (Freedman to Duncan, April 25, 1986), Freedman acknowledged that "Your distinction between the Gaussian and Galtonian regression traditions seems right." Freedman represents the Gaussian tradition, which in turn follows Plato's typological thinking.[6] Duncan represents the Galtonian tradition, which in turn follows Darwin's population thinking. This difference in orientation explains why they were so far apart in interpreting the role of regression in social science research.

Duncan's Influence on Quantitative Reasoning in Social Science

More than anyone else, Duncan was responsible for today's quantitative sociology and social demography. Besides his exemplary research in social stratification, social demography, and statistical methodology, Duncan's influence has been most important in establishing a new intellectual tradition. While some early sociologists tried to model sociology after physical science, Duncan was openly disdainful of the search for supposedly universal laws of society that would mimic those of the physical sciences. This does not mean that Duncan did not wish that we could have universal laws as in physical science. Rather, he was keenly aware of the differences between typological thinking and population thinking, and the huge amount of variability in human societies that makes it unfruitful to search for universal laws.

Duncan's new approach was built on a long-standing tradition in demography: it is of foremost importance to document and understand empirical patterns in real populations. To Duncan, this meant focusing on the variability of population characteristics. This view set Duncan apart from his peers, and was instrumental in transforming demography. Before Duncan, the central focus was on changes in population *size*. It was Phil Hauser and Duncan who emphasized the *composition* of a population as the subject matter of demography. This shift in emphasis in subject matter is exemplified by a large amount of exemplary work produced by Duncan and other scholars who followed him on such matters as social mobility and social inequality, education, income, family, race and ethnicity, residential segregation, gender roles, and social measurement in general. This new field even acquired a name, "social demography." Thus, the development of quantitative sociology after Duncan is closely linked to the development of social demography. The convergence between the two fields was powerful-- making demography more social, and making scientific sociology more empirical. The end result has been a new demographic approach with the following characteristics:

1. It is empirically based.
2. It is quantitative, making use of either survey or census data, collected at the lowest possible level of units of analysis (often individuals).
3. It involves statistical analysis with regression-type techniques that involve attention to empirical regularity, i.e., between-group variation. However, results from such analysis do not necessarily have causal interpretations.
4. It requires the researcher to pay attention to the population under study. Ideally, it calls for the use of nationally representative (population-based) surveys for social science research. This new standard was set by the Blau and Duncan (1967) study.

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[6] I do not mean that David Freedman is a Platonist. He would claim to be "an empiricist, or a positivist, or a realistic, or a Baconian" (personal communication, October 28, 2005). Freedman's argument is that the Gaussian view of regression is needed for regression to be used for causal inference in the way typically seen in social science. Freedman presents his summary and updated views on the use of regression analysis in social science in his 2005 book (Freedman 2005).

Dissatisfaction with Statistical Sociology

Any quantitative sociologist who interacted with Duncan professionally knew that he was very critical of the general way in which most sociological research was being conducted, and above all, his own work. In light of this, Duncan's dissatisfaction with statistical sociology is understandable. Duncan had sometimes been incorrectly referred to as the inventor of path analysis and structural equations. He was very uncomfortable with this attribution, stating that he was "made intolerably nervous by being called one of the 'architects of the method'" (Duncan to Freedman, January 14, 1985). His reluctance to take credit for introducing path analysis to sociology reflected his general dissatisfaction with the use of the method in sociology, as some researchers began to use it carelessly and in ways that he never intended.

This also explains Duncan's non-defensiveness in his initial response to Freedman's critique (Duncan to Freedman, June 2, 1983). In his follow-up letter (Duncan to Freedman June 21, 1983), however, Duncan launched a defense (or, more accurately, an explanation) that the Blau-Duncan path model (Blau and Duncan, 1967) was only a crude but simplifying approximation for extremely complicated social processes:

> I have continued to ruminate about your critique since I wrote you, and I guess I would like to make a couple of further observations. First, the caveat on our p.172 [in Blau and Duncan, quoted above] which you quote on your p.20 strikes me as a pretty strong one. It was meant sincerely. For you to follow it with a sneer is a little hard to take, particularly when, on p.8 you fault us for omitting quality of education, when education was obtained, when respondents entered the labor force, history, the economy, world wars, and the great depression. Is it really possible to include all this in a model and have it *less* complicated [emphasis original]?

This passage illustrates an important point that is often under-appreciated or even misunderstood by many of Duncan's contemporary quantitative sociologists: statistical models in social science are ultimately reduced-form and thus not structural. Imputing structural interpretation to statistical models could be dangerous and was not something Duncan was comfortable doing.

Thus, in Duncan's view, quantitative tools should not be used to discover universal laws that would describe or explain the behaviors of all individuals. He totally rejected such endeavors as meaningless. He believed that all quantitative analysis can do is summarize empirical patterns of between-group differences while temporarily ignoring within-group individual differences. Over time, however, social scientists can improve their understanding of the world by incrementally adding greater complexity to their analyses.

Duncan's deep belief in the reduced-form nature of statistical models can be seen in his early correspondence with Goldberger, who later helped unify, along with Duncan, similar models in the forms of structural equations, factor analysis, and path analysis in econometrics, psychometrics, and sociological methodology. In his first letter to Goldberger, who initiated the correspondence in a long letter dated June 19, 1968, Duncan made the following keen, unsolicited observation on the difference between sociology and economics on the seventh and final page (Duncan to Goldberger, June 26, 1968):

> (g) Sociologists appear to be most interested in an "inductive" strategy with respect to models, holding to the somewhat forlorn hope that it will be possible to "discover" the right model through data analysis…. Economists, I take it, have somewhat more confidence in their theories which have a status of a priori information with respect to their models, and therefore are more concerned with efficient "estimation."

Duncan's distinction between sociology and economics won the approval of Goldberger, who replied (July 9, 1968):

> In reviewing the path analysis literature, it struck me that economists do operate the other way round. To stretch a point slightly, path analysts are interested in decomposing a reduced form coefficient into the structural coefficients which are its components. Most economic work proceeds in the opposite direction – and here I'm thinking of theoretical economics as well as econometrics. We start with a structural model and are interested in deriving the reduced form from it.

While the empirical approach of decomposing reduced form coefficients in the form of path analysis was initially a good step forward for Duncan, it quickly grew into an unsolvable problem. Once the genie of decomposition gets out of the bottle, where should it stop? Since reduced form coefficients can be decomposed in an infinite number of ways, what would be a sound way to choose among so many alternatives? Clearly, Duncan was very unhappy to leave the decision to statistical tools and data analysis, a tendency in quantitative sociology that did not please him. This led him to a harsh criticism of "statisticism." Duncan wrote the following passage in *Notes on Social Measurement* (1984) with Freedman in mind and referred to it in his first letter to Freedman:

> [W]e often find the syndrome that I have come to call *statisticism*: the notion that computing is synonymous with doing research, the naïve faith that statistics is a complete or sufficient basis for scientific methodology, the superstition that statistical formulas exist for evaluating such things as the relative merits of different substantive theories or the "importance" of the causes of a "dependent variable"; and the delusion that decomposing the covariations of some arbitrary and haphazardly assembled collection of variables can somehow justify not only a "causal model" but also, praise a mark, a "measurement model." (p. 226)

What can be done to escape the trap of statisticism? Duncan suggested two possible paths: improvement of social measurement and an emphasis on the conceptualization of social processes and research designs that reveal such processes. Duncan (1984) spent a whole book on the first remedy. He also placed much emphasis on the second remedy in a series of papers in his late career that were concerned with the Rasch model (e.g., Duncan and Stenbeck, 1988; Duncan, Stenbeck, and Brody, 1988). The following published passage in Duncan and Stenbeck (1988) on a study of voter turnouts summarizes well Duncan's call for the second remedy:

> [T]he thesis we wish most to emphasize is that the application of statistical models and methods should be strictly subordinate to the central scientific task. That task we take to be the formulation of cogent theories explaining the processes under study and the invention of research designs suited to the testing of such theories. Our rhetorical strategy is to engender skepticism about statistical models by showing that the best we can do by way of a statistical model for turnout data falls far short as a scientific model of the process by which persons acquire and exercise their tendencies to vote. Others, we hope, will do better. (p. 2)

The Duncan and Stenbeck (1988) paper is important not only because it was Duncan's last published statement on sociological methodology[7] but also because it revealed his dissatisfaction with his earlier data-reduction and data-summary approach. The authors conclude the paper as follows:

> In our view, the time has come to rectify an imbalance between the application of statistical methods in data analysis and the exploitation of statistical models for data reduction, on the one hand, and, on the other, the development of genuinely explanatory or "structural" models. The central task of methodology should be the critique of research designs – not the exposition of techniques of statistical inference – just as the central task of the research scientist is to contrive the designs that will force "nature" to reveal something about how processes actually work. (pp. 31-32)

While Duncan expressed his dissatisfaction with the data analysis and data reduction approach in general and called for research designs that would yield truly structural models, he never actually ventured into such territories. What particularly troubled Duncan is the combination of the two intrinsic features of social science data. First, there is an uncertainty to an individual's response (either in attitude or behavior) even if we know the true underlying model. Second, the underlying model actually varies across different members in a population – a feature sometimes called "population heterogeneity." It was on population heterogeneity that Duncan spent most of his late career working, in connection with the Rasch model.

[7] Until the very end of his life, Duncan clung to his view as previously expressed in the Duncan and Stenbeck (1988) article. He referred to this article in a number of communications (to Yu Xie, December 7, 2002; September 2, 2003; to Lee Wolfle, April 18, 2004; to Leo Goodman & Yu Xie, May 10, 2004).

The Key Problem: Population Heterogeneity

Duncan spent more than ten years in his late career working on the Rasch model (Goodman, 2004). Even toward the very end of his life, Duncan still cared a great deal about this model. Once he wrote me asking why Dan Powers and I did not cover the Rasch model in our book on categorical data analysis (Duncan to Yu Xie, March 8, 2001). When the ASA Methodology Section decided to name its keynote speech series the "Duncan Lecture," Duncan expressed the "hope [that] the first lecturer gives a discourse on Rasch models" (personal communication, September 2, 2003). When Goodman gave his inaugural Duncan Lecture in Ann Arbor in April 2004 on the relationships among the loglinear model, the latent class model, and the Rasch model, Duncan was so happy that he made references to this fact in his emails to friends announcing the "penultimate" phase of his life (Duncan to friends, September 13, 2004; September 27, 2004). To understand Duncan's contributions to sociological methodology, thus, it is important to understand why he was so fascinated by the Rasch model.

Georg Rasch (1901-1980) was a Danish mathematician and statistician who devised a measurement model with the following features: (1) a subject's response to an item is always probabilistic; (2) an item's true values are invariant with subjects; and (3) a subject's tendency to score is the same across all items. (Rasch 1966, 1980) These properties essentially mean that the probability of a subject scoring on an item can be decomposed, after a logit transformation, into an additive component due to the item's difficulty and another component due to the subject's score. The model can be written as follows.

Let p_{ij} be the probability that the ith subject will give a positive response to the jth item. The Rasch model specifies the following logit model:

$$\log[p_{ij}/(1 - p_{ij})] = \theta_i + \beta_j , \qquad (2)$$

where θ_i is the person-specific parameter, and β_j is the item-specific parameter. As shown in equation (2), an important property of the Rasch model is that it allows for individual-variability (i.e., additive population heterogeneity) and item-variability, but it achieves separability between them through the invariance properties: θ_i does not vary with item (j), and β_j does not vary with subject (i). Potentially at least, the Rasch model thus allows the researcher to treat all different subjects as constituting unique "classes" themselves, thus avoiding the seemingly arbitrary procedure of classifying subjects, based on response patterns, into latent classes in the traditional latent class models.

Although Duncan published many papers on the Rasch model (see Goodman 2004), I find his thinking on the subject best represented by an unpublished manuscript for a presentation made at Yale University on April 20, 1982, entitled "Rasch Measurement and Sociological Theory."[8] It is of special interest to note that he used "sociological theory" as part of the title. I am not aware of any other occasion in which he used "sociological theory" as part of a title. So, what did he have in mind by "sociological theory"? He explained, "The style of theorizing I want to see imitated is beautifully exemplified in the classic papers by P. E. Converse that introduced the concept of 'non-attitudes' and pioneered in the mathematical modelling of responses to survey questions" (p.1). Duncan then went on explaining Converse's approach (Converse 1964) and extended it in the paper.

To appreciate Duncan's insight, let us first take a look at four of his figures, labeled as Figures 1 through 4.[9] Figures 1 and 2 both give an overall response probability of 7/12, but the two situations are very different. In Figure 1, "we assume each respondent carries just one pellet, and on any trial will tell you whether it is the shaded or clear pellet"(p. 4); in Figure 2, "we assume each respondent carries a spinner (demonstrate) and on each trial will spin it with probability p of getting the shaded pattern" (p.4). On other words, Duncan set up the two extreme situations whereas in one situation (Figure 1) there is

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[8] After Duncan learned that Leo Goodman was going to give the inaugural Duncan lecture on the Rasch model, he sent the unpublished manuscript to Leo Goodman and Yu Xie on 5/10/2004. I have now posted the manuscript (in its original form) on my webpage yuxie.com.

[9] they were originally labeled as Figures 5, 6, 8, and 9, in Duncan (1982).

population heterogeneity (across persons) but a deterministic response pattern given an individual, whereas in the other (Figure 2), there is a population homogeneity so that all individuals in a population are governed by the exact same causal mechanism that is, however, probabilistic. Duncan then went on to explain that with actual data "we seldom see either extreme," and this led Converse to a mixture of two situations, as represented in Figure 3. However, Duncan did not stop here with just three classes. He applied the insight from the Rasch model that allows for subject-specific classes and argued that "in principle there are infinitely many different spinners" (p.5). For the sake of illustration, he gave an example of 7 classes in Figure 4.

While he liked to allow for individual-level differences with individual-specific classes, Duncan quickly realized that identification of the classes is data-demanding. Much of Duncan's work with the Rasch model was based on repeated measures, such as those in a panel study when the researcher does not suspect a real change. Duncan illustrated his reasoning in Table 1, based on the same example as Figures 1-4.[10] The row represents the measured responses in the first interview, and the column represents the responses for the same subjects (n=144) in the second interview. Ignoring measurement errors and sampling errors, Duncan argues that the top panel (Panel I) corresponds to the situation of extreme population heterogeneity in Figure 1, and that the second panel (Panel II) corresponds to the situation of extreme population homogeneity. In the first case, there is perfect association between row and column variables. In the second case, row and column are independent of each other. To Duncan, as to Converse, real life is likely to be a mixture of the two, as represented in the bottom panel (Panel III).

I went through the simple example discussed in Duncan's 1982 unpublished paper to demonstrate the point that Duncan was struggling with population heterogeneity. This was the reason why the Rasch model appealed to him. In a way, he anticipated and would welcome the various methods, developed later, that address population heterogeneity, such as multi-level models (Raudenbush and Bryk 1986), growth-curve models (Muthén and Muthén 2000), and latent class models (D'Unger et al. 1998) – all now under the rubric of mixed models (Demidenko 2004). However, even to this day, population heterogeneity remains the greatest threat to all causal inferences in social science using observational data, analyses which typically require strong and unverifiable assumptions (Heckman 2001, 2005; Holland 1986; Winship and Morgan 1999).

Duncan saw population heterogeneity as the problem that seemed most insurmountable. In a letter to Jonathan Kelly (April 22, 1991), Duncan remarked,

> Both the usual log-linear and linear models break down because the conditional probabilities they estimate are not actually the same for all persons having the same set of values of the predetermined variables. That is, these models do not deal with heterogeneity.

In another letter, Duncan (to Yu Xie, July 30, 1996) commented on his interest in Rasch models in this way:

> In the little thinking I do these days about the old battles I fought, it has increasingly seemed to me that one of two or three cardinal problems that social science has not yet come to grips with is precisely this issue of heterogeneity… The ubiquity of heterogeneity means that for the most part we substitute actuarial probabilities for the true individual probabilities, and therefore we generate mainly descriptively accurate but theoretically empty and prognostically useless statistics.

It was not until I went through all of Duncan's letters to me after his death in November 2004 that I began to appreciate the insight of this passage. I simply did not have the professional maturity to appreciate it when I first received the letter in 1996. That is, it has taken me almost nine years to fully appreciate Duncan's remarks. Not only was he ahead of his time, he was ahead of my time as well. How to deal with population heterogeneity remains the toughest challenge in today's quantitative social science and statistics.

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[10] It was Figure 7 in Duncan (1982).

Conclusion

To me and to many others, Duncan was the greatest quantitative sociologist of all time. Not only was he responsible for introducing path analysis to sociology and social science in general, but he was also instrumental in setting the norms in a new demographic paradigm for quantitative sociology. His work on sociological methodology influenced a whole generation of quantitative sociologists. His scholarly contributions in substantive sociology, which was his primary occupation and his very reason for taking up methodology, were followed up and modeled after by waves of sociologists in many areas such as social stratification, residential segregation, the sociology of education, the sociology of the family, and social measurement. It is hard to find an area in today's quantitative sociology with no trace of Duncan's direct or indirect influence.

Duncan's approach to quantitative sociology was based on population-thinking, an epistemological view pioneered by Darwin and further developed by Galton. The paradigm, while empirically based and quantitatively oriented, does not seek to discover universal laws. Instead, the fruitful task of quantitative sociology is to summarize systematic patterns in population variability. In setting up this paradigm, Duncan explicitly repudiated a positivist vision that attempts to model sociology after physical science. I venture that it is mainly due to Duncan's influence that there is almost no discussion of "universal laws" in today's quantitative sociology. As Duncan's own substantive work illustrates, a good sociological study can inform readers about social processes with a quantitative analysis, however approximate it may be.

While we are today celebrating the intellectual legacy of Dudley Duncan, it is important to realize that there are limitations to the quantitative approach in sociology that Duncan so successfully advocated. Actually, I would even argue that Duncan was remarkable in being among the first who foresaw difficulties of his own approach when the whole fields of quantitative sociology and social demography seemed very happy with it. The problem that Duncan ran into is inherently unsolvable in social science -- population heterogeneity. As a result, Duncan became disappointed with quantitative sociology and social demography.

Social statistics has greatly advanced since Duncan's heyday. We now have sophisticated statistical models (such as multi-level models, spatial models, network models, and latent class models), efficient and robust estimation methods (including Bayesian and simulation methods), great computer programs, increasingly fast and inexpensive computers, and very large and rich data sets (longitudinal as well as multi-level) with easy accessibility (for example, via the internet). However, the same fundamental problem that troubled Duncan, population heterogeneity, remains the greatest obstacle in quantitative social science even today (Heckman 2001).

With observational data, quantitative analyses inevitably rely on comparisons of some group of individuals with another group of individuals for causal inference. Given the continuous presence of population heterogeneity, how can we be assured that the comparison is not biased by the possibility that the two groups are not really comparable in dimensions unobserved but relevant to the study? This fundamental problem accounts for a current renewed interest in causality questions in the social sciences (Heckman 2005; Winship and Morgan 1999). I do not believe that this problem can ever be satisfactorily solved. At least, there are no blanket methodological solutions to the problem. Instead, we should learn from Duncan to be savvy researchers who are interested in advancing our empirical understanding about substantive topics of sociological import but unwilling to make sweeping claims. For this reason, Duncan's legacy is of permanent value.

Let me now conclude this paper with a humorous comment from Duncan that contrasts economics and sociology. The comment was meant to be a joke at a dinner table, but it also reveals vividly not only Duncan's discomfort with the deductive approach often seen in economics but also his dissatisfaction with sociologists' lack of concern with statistical methods.[11] The comment reads as follows:

> Economists reason correctly from false premises; sociologists reason incorrectly from true premises. Thus they create two complementary bodies of ignorance. (Duncan to Yu Xie, June 28, 2003)

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[11] Despite the humorous comment, Duncan had very high regards for economics in general. He made the joking remark during my visit in February 2003. After he made the remark, he went through the list of Nobel Laureates in Economics with an almanac and told me about their contributions.

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Figure 1

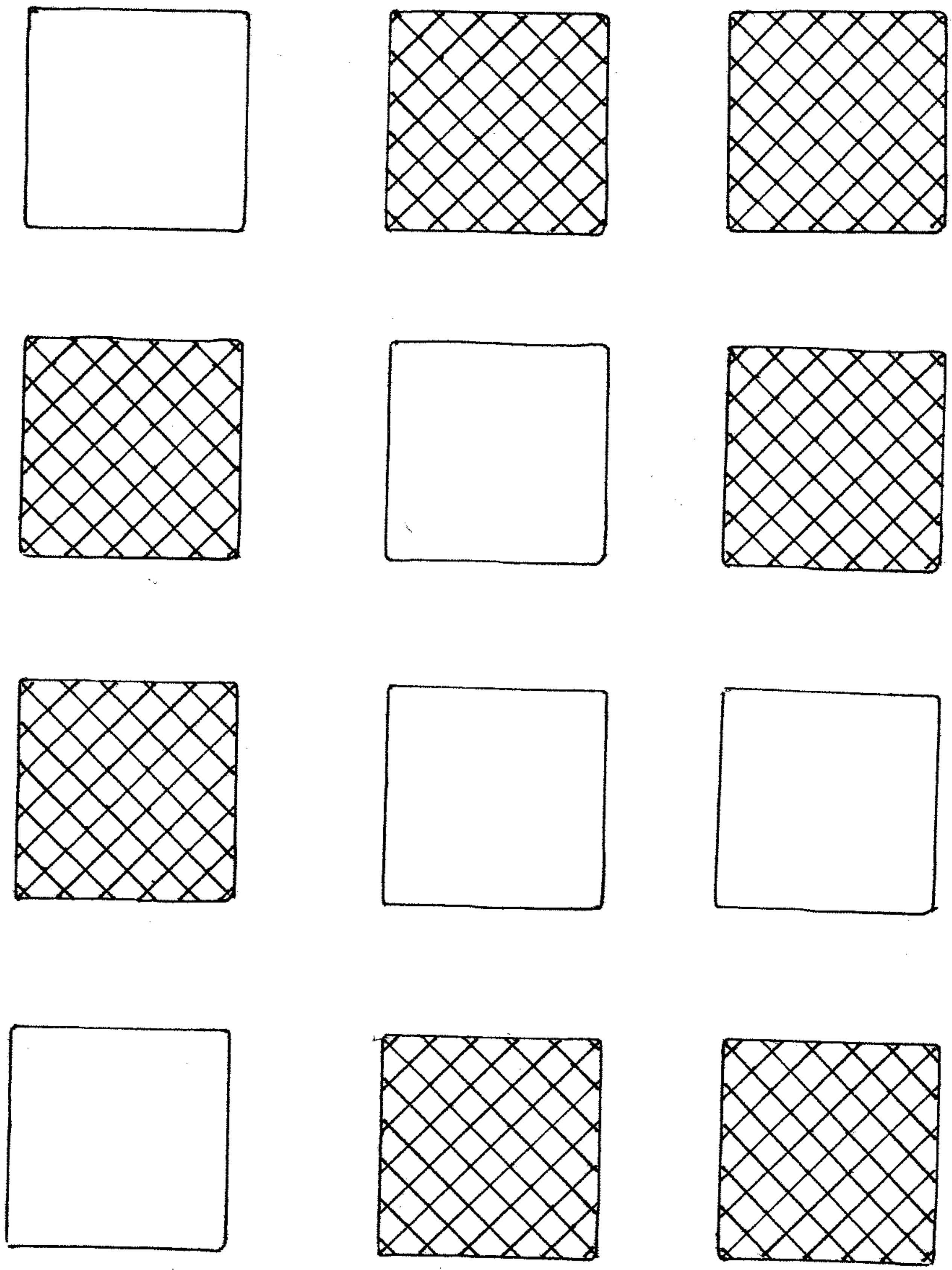


Figure 2

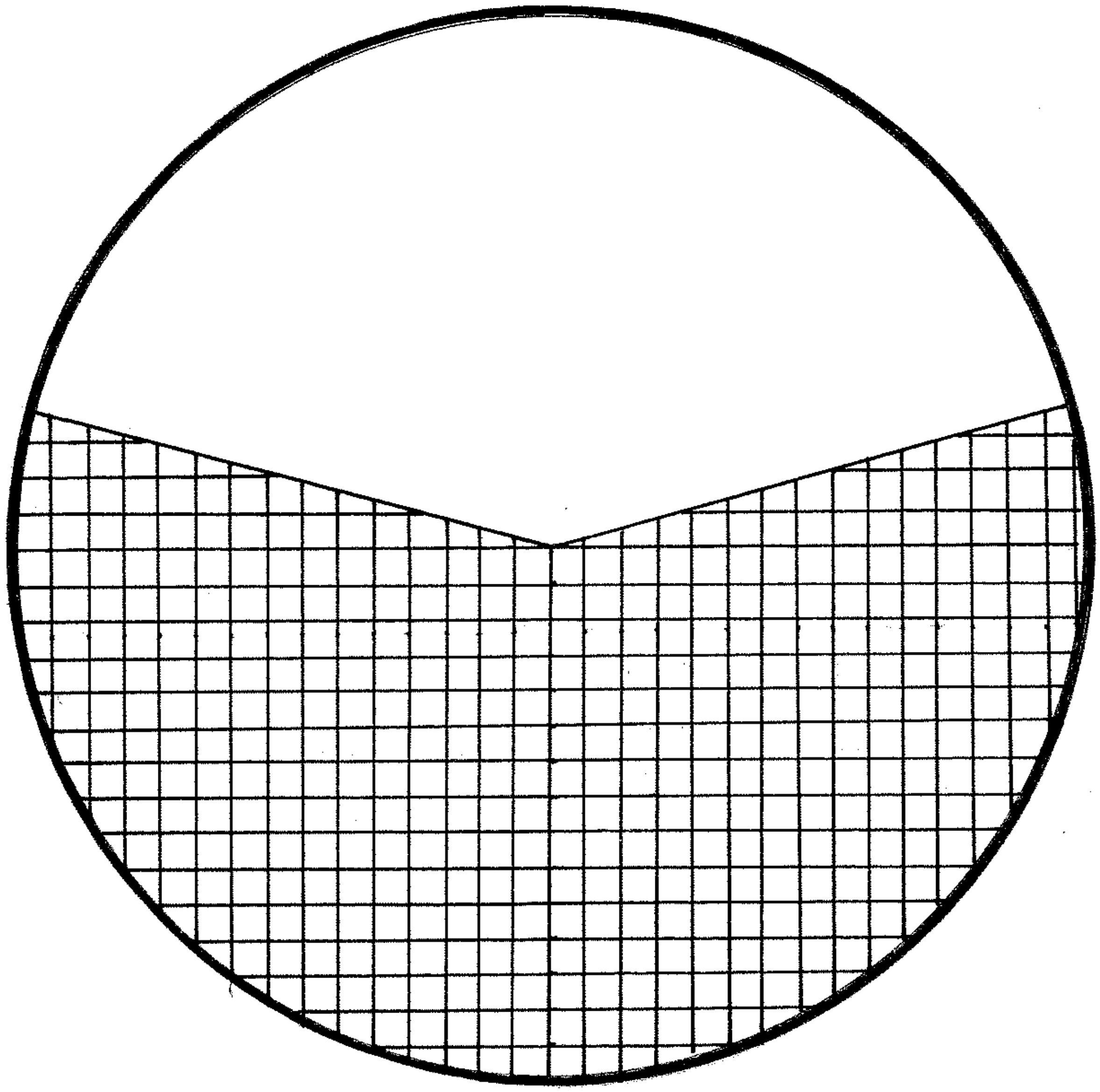


Figure 3

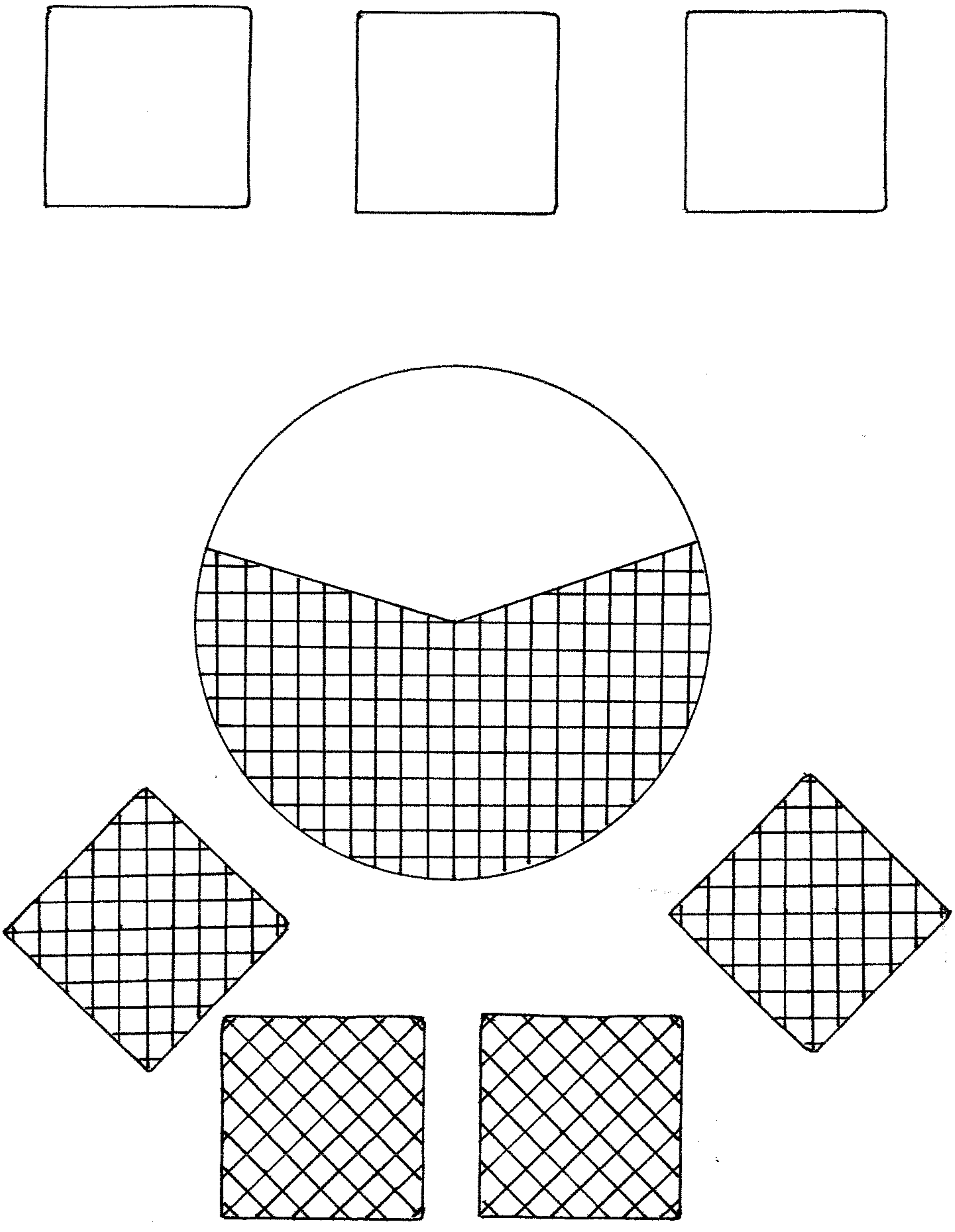
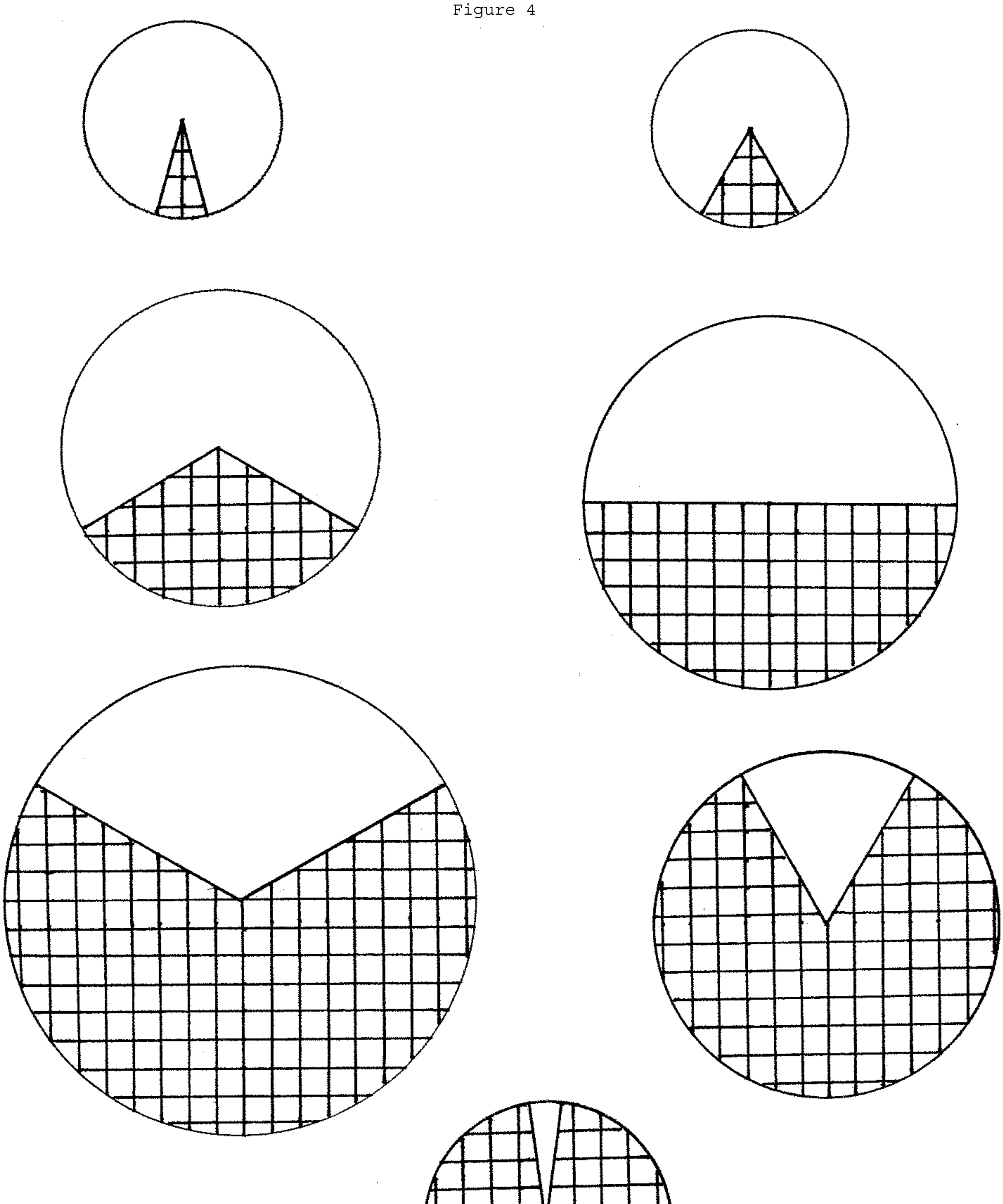


Figure 4



(N = 144)		□	▩
I.	□	60	0
$p = 7/12$	▩	0	84
II.	□	25	35
$p = 7/12$	▩	35	49
III.	□	46	14
$p = 7/12$	▩	14	70

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