Thomas C. Schelling and James M. Sakoda: The Intellectual, Technical, and Social History of a Model

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Abstract: The Journal of Mathematical Sociology (JMS) started in 1971. The second issue contained its most cited article: Thomas C. Schelling, "Dynamic Models of Segregation". In that article, Schelling presented a family of models, one of which became a canonical model. To date it is called the Schelling model – an eponym that affixes the inventor's name to the invention, one of the highest forms of scientific recognition. In the very first issue of JMS, James Minoru Sakoda published an article entitled "The Checkerboard Model of Social Interaction". Sakoda's article more or less went unrecognized. Yet, a careful comparison demonstrates that in a certain sense the Schelling model is just an instance of Sakoda's model. A precursor of that model was already part of Sakoda's 1949 dissertation submitted to the University of California at Berkeley. A substantial amount of evidence indicates that in the 1970s Sakoda was well known and recognized as a computational social scientist, whereas Schelling was an unknown in the field. A generation later, the pattern of recognition almost completely reversed: Sakoda had become the unknown, while Schelling was the well-known inventor of the pioneering Schelling model. This article explains this puzzling pattern of recognition. Technical and social factors play a decisive role. Some contrafactual historical reflection suggests that the final result was not inevitable.

Keywords: Schelling, Sakoda, Checkerboard Models, Tipping Models, Threshold Models, Agent-Based Modeling

Contents

I've never been sure why my little simulation got so much attention after so many years. I discovered twenty-five years later that I'd been some kind of pioneer. It must be some limitation of my scientific imagination that I'd no idea I was doing something generic, something with beyond my neighborhood application.

Thomas C. Schelling in "Some Fun, Thirty-Five Years Ago" [2006, 1642f.]

Of all the things I've done, I think the best thing I've done is the social interaction model, which solved the problem in social psychology of going from the individual level to the group level.

James M. Sakoda in an interview with Arthur A. Hansen on August 10, 1988 [Hansen, 1994, 417]

1 Introduction: A Famous and a Forgotten

In 1971, *The Journal of Mathematical Sociology* (*JMS*) started to appear. The most cited and most read article of *JMS* to date appeared in its *second* issue: *Thomas Schelling, Dynamic Models of Segregation* [1971a].[1] Schelling's title uses the plural form: "Dynamic Models", *not* "A Dynamic Model". The plural is neither a misleading accident nor an overstatement—Schelling presents a *family of different models*. All members share four joint features: *First*, the models address the kind of segregation that "results from the interplay of individual choices that discriminate" [1971a, 143]. *Second*, the primary focus is on choices related to residential segregation by color. *Third*, the basic structure of the models is extremely simple. A few assumptions, easy to grasp and easy to recall, are sufficient to define the models. *Fourth*, despite their simplicity, all the models generate surprising results—eye-openers for a better understanding of segregation.

One member of the family "really made it", and became—singular—*the Schelling model*: In that model the world is a two-dimensional checkerboard. It is inhabited by two groups, whites and blacks. The inhabitants perceive as their neighborhood a small area around their actual location on the checkerboard. They are content if, and only if, a certain color composition is realized in their personal neighborhood. If people are discontent, they try to move to a location that meets their composition demands. Schelling shows that a massive residential segregation can result from only mildly segregationist preferences. It is sufficient that people wish to avoid minority status. The main and *ex ante* counter-intuitive lesson is that massive segregation does *not necessarily* require massively segregationist preferences, i.e. a preference for living in a neighborhood with high percentages of like colored people. What nowadays *normally* is called *the Schelling model*, is this simple model.[2]

Schelling's family of segregation models contains a one-dimensional version of the two-dimensional checkerboard model. Therein blacks and whites live in a kind of

[1] Cf. the *JMS* website at `http://www.tandfonline.com/toc/gmas20/current`.

[2] We discuss the relations between the models later in section 6.2.

"lineland-world". Again the inhabitants have a self-centered neighborhood definition (a couple of cells to their left, a couple of cells to their right), and again, avoiding minority status, is sufficient to generate a massive segregation. *Sometimes* the two- and one-dimensional versions are packaged together, and, then, *together* called *the Schelling model*—but only sometimes. If not stated otherwise, in the following *the Schelling model* is always Schelling's two-dimensional checkerboard segregation model, and a phrase like "Schelling's model" always refers to the Schelling model.

Except for an interesting preface, Schelling's *Dynamic Models of Segregation* [1971a] originated in and is basically identical with a memorandum that he had written for the RAND corporation (issued in May 1969), entitled *Models of Segregation* [1969a].[3] That memorandum was also the source for a first short article on models of segregation that Schelling published 1969 in the *American Economic Review* [1969b]. Material of the RAND memorandum was also used and published as a book contribution in [1972b] and in [1974]. Later in 1978, Schelling published his best selling book *Micromotives and Macrobehavior*. Chapter 4 is entitled *Sorting and mixing: Race and sex*. It is a shortened version of the Schelling model as presented in *Dynamic Models of Segregation* [1971a].[4] All in all, Schelling's *JMS* article is the first and most comprehensive *journal* publication of his original RAND-memorandum *Models of Segregation* [1969a].

In the following decades Schelling's model has become a classic reference in many (partially overlapping) scientific contexts: explanation of residential segregation,[5] unintended consequences,[6] micro-macro relations,[7] clustering, attractors, social phase transitions,[8] invisible-hand explanations,[9] emergence of spontaneous order and structure.[10] In philosophy of science Schelling's model is a (and often *the*) paradigmatic example for the study of mechanisms, or for reflections on the status of models more generally.[11] Furthermore, Schelling's model is considered an early and pioneering ex-

[3] That is stated right at the beginning of Schelling [1971a]. An eBook version of the memorandum can be downloaded for free at `http://www.rand.org/about/history/nobel/schelling.html`. For a remarkable difference between the article and the memorandum see footnote 211. More details on the RAND Cooperation follow later in section 6.1; see there footnote 159.

[4] The chapter on segregation in a one-dimensional world is left out.

[5] [Fossett, 2006] gives an extensive overview on the literature. Additionally, the article presents in detail some extensions and modification of Schelling's models. Fossett's article is the focus article of a special issue of *JMS* (vol. 30, number 3–4, 2006). Basically, it is as well a special issue on Schelling's *JMS* article from 1971. Based upon a search using the Social Science Citation Index, Skvoretz reports in his introduction to the special issue that Schelling's *JMS* article "is the second most heavily cited article ever published by *JMS*" [Skvoretz, 2006, 181]. That was in 2006. In the mean time it has become JMS' most cited and most read article. For Schelling and the explanation of residential segregation see also the following later articles and the literature therein (alphabetical order): [Bruch and Mare, 2006], [Bruch and Mare, 2009], [Clark, 2006], [Clark and Fossett, 2008], [Hatna and Benenson, 2012], [Macy and van de Rijt, 2006], [Pancs and Vriend, 2007], [Singh et al., 2009], [Zhang, 2004], [Zhang, 2011].

[6] See [Aydinonat, 2008].

[7] The article [Raub et al., 2011] reviews the central features of micro-macro explanations. The article is at the same time the introduction to a special issue of *JMS* on micro-macro links and micro-foundations. Schelling's model plays a central role in almost all articles.

[8] [Gauvin et al., 2009], [Stauffer and Solomon, 2007], [Vinkovic and Kirman, 2006], [Cortez et al., 2015].

[9] See [Ullmann-Margalit, 1978].

[10] See [Macy and Willer, 2002].

[11] Sugden's much discussed article *Credible worlds: the status of theoretical models in economics* [2002]

Figure 1: Thomas C. Schelling receiving his Prize from His Majesty the King Carl XVI Gustaf of Sweden at the Stockholm Concert Hall, December 10, 2005. Copyright: The Nobel Foundation 2005; Photo: Hans Mehlin.

ample for an agent-based computer simulation.[12] In up to date introductory courses to agent-based computational social science, Schelling's model is included almost by default; often the model is used as the starter and appetizer. When on October 10, 2005, the Royal Swedish Academy of Sciences announced that Schelling had won the Nobel Prize for Economics (together with Robert Aumann), they cited Schelling's application of game theory to problems of the arms race and international security as well as his models of segregation as the grounds for the award of the prize.[13] All that in mind, it is no overstatement to say: The success of a model could hardly be greater!

On June 12, 2005, the year in which the Nobel Prize was awarded to Schelling, someone else died: James Minoru Sakoda. Decades ago, in 1971, he had also published in the first *JMS* volume. In the very *first JMS* issue, i.e. one issue *earlier* than Schelling, he had published an article with the title *The Checkerboard Model of Social Interaction* [Sakoda, 1971]. Sakoda starts with a fairly general claim:

used Schelling's model (together with another model of Akerlof) as his paradigm case of economic modeling. Sugden's follow-up article [2009] is the introduction to a special issue on that discussion, published by the philosophy of science journal *Erkenntnis*. Almost all contributions refer to Schelling's model. See also [Aydinonat, 2007] and [Aydinonat, 2008].

[12] See [Macy and Willer, 2002].

[13] The Royal Swedish Academy of Sciences published both, a popular *and* an advanced information. In the *advanced information* the segregation model is less important. There it is mentioned only in the chapter "Other contributions". See http://www.nobelprize.org/nobel_prizes/economics/laureates/2005/press.html

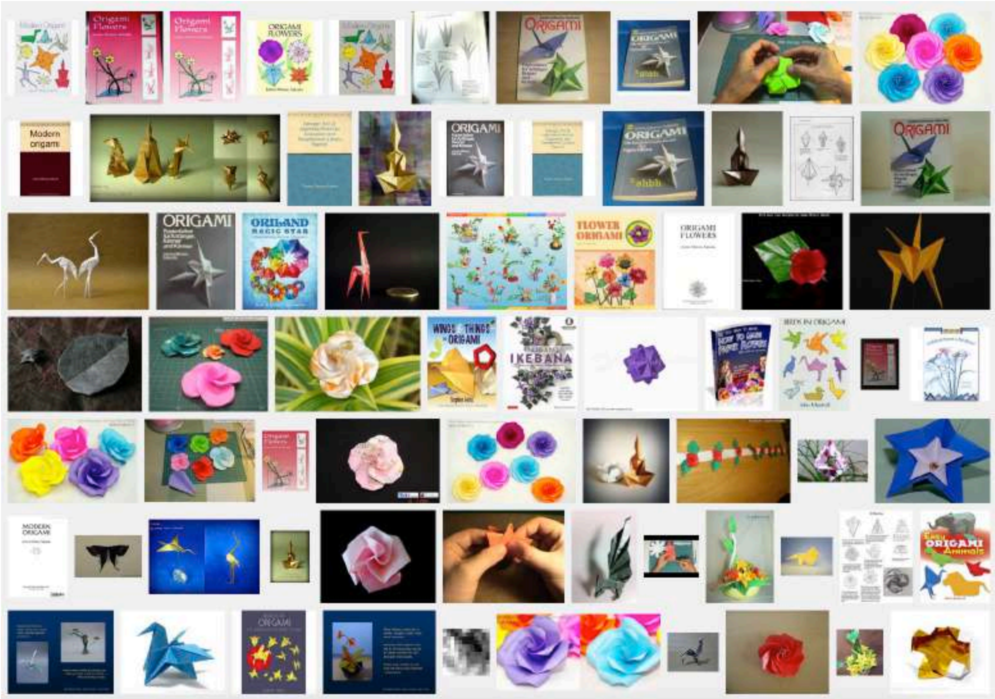

Figure 2: The result of a Goggle picture search for *"James Minoru Sakoda"* on August 24, 2015

> The checkerboard model provides a concrete means of portraying social interaction as an ongoing process among members of groups [ibid. 119].

On Sakoda's checkerboard, members of two different groups move, driven by positive, neutral, or negative attitudes towards each other. Given their attitudes, the individuals try to move to most attractive locations. For the evaluation of locations all individuals count, but more distant ones count less. The attitude driven movements generate stable or unstable patterns—a "social structure resulting from the interactional process" [ibid.].

Sakoda's claim about his checkerboard model is programmatically much more general than Schelling's. Sakoda's model *includes* segregation as brought about by the interplay of choices of individuals. As a matter of fact, one of the *specific* attitude patterns that Sakoda defines and illustrates, is named *segregation*—and, as it will turn out later, that is more than just the same word. At the same time, many other attitude patterns, the social interaction processes that they induce, and the social structures that they produce, are covered. In Schelling's (and the now very common) micro-macro jargon one might say: Sakoda proposes a flexible and plastic micro-motive engine that drives individuals in their interactions which, then, generate certain macro structures and macro effects. In Sakoda's own terminology the micro-macro distinction appears as a distinction of *two disciplines*, psychology and sociology. Sakoda considers his model as "a breakthrough in the wall separating psychological concepts from sociological ones" [ibid.].

The reception of Sakoda's article was not a success story—the publication was more a stillbirth. For the first 10 years after publication Google Scholar finds 2 citations; 20 years later there are 3. That is a long way from what one could call resonance. When Sakoda died in 2005, aged 89, his death was noticed only within the *paperfolder scene*, to which he had contributed three books, new types of folded figures, and a new folding style. He started with *Origami*, the art of paper folding, in the middle of the 1950s.[14] David Lister (1930–2013), a founder, president, and leading figure of the British Origami Society, wrote an obituary on Sakoda that was published on the web pages of the British Origami Society. The obituary values highly and in detail Sakoda's contributions to modern Origami. It contains some short remarks about Sakoda's academic career and scientific work. We learn that Sakoda became an assistant professor of psychology at the University of Connecticut in 1952, moved to Brown University in 1962 as a professor of sociology and anthropology, and "became involved with computers early" [Lister, 2005].[15] A Goggle-search for "James Minoru Sakoda" makes it very clear, that Sakoda has an afterlife: it is a remembrance as a highly gifted and inventive paperfolder (see figure 2).

There are things much worse than being remembered as a famous paperfolder. However, there is much more in Sakoda's life and work that deserves recollection, recognition, and an appropriate attribution as his pioneering invention. In the following I'll work out what (obviously unintentionally) is hidden, implicit, and understated in David Lister's short remark, that Sakoda "became involved with computers early": As a matter of fact, Sakoda was *the very first* pioneer of checkerboard modeling of social interactions.

Sakoda's checkerboard model is *much more general and flexible* than Schelling's model. In a certain concise sense, it is possible to consider Schelling's model as *an instance of Sakoda's model*. As to priority, an early version of Sakoda's model is defined already in Sakoda's dissertation, written in the 1940s, and deposited in the library of the University of California on August 1, 1949. Additionally, Sakoda was an early pioneer of computational social science in general. Driven by the same motivations that today drive the developers of NetLogo, Sakoda already developed in the 1960s a programming language named DYSTAL, designed for the special purposes of social scientists. In the early 1960s he established and directed the Sociology Computer Laboratory at Brown University. And, to make the astonishment and mysteriousness perfect, there have been times, namely the 1960s and 1970s, in which Sakoda actually was well known and recognized among his peers of computational social scientists whereas Schelling was an unknown there. If all this is true, then there is a *serious puzzle*. What happened? How could it come about that today Thomas C. Schelling is a celebrated scientist, inventor of a famous model, while James Minoru Sakoda is unknown as a scientific pioneer, and survives as a brilliant paperfolder?

This long study presents the evidence for the claims made above. And I will resolve the puzzle. My story is somewhat thrilling (at least the process of understanding, what has happened was it for me). But it is not a thriller! No crime happened, no

[14] The three books are [Sakoda, 1997, first edition 1969], [Sakoda, 1992, self-published], and [Sakoda, 1999]. The latter is a revised edition of the self-published [Sakoda, 1992].

[15] See `http://www.britishorigami.info/academic/lister/sakoda.php`. – No other obituary on Sakoda is known to me.

conspiracy was involved. No discrimination whatsoever was at work. As to the main actors, all rules of honest scientific work, citation, and giving credit were abided. By all standards, nothing "unethical" is part of the story. Something went seriously wrong, but, as to the main actors, nobody did anything wrong. In retrospect, what happened was due to an interplay of fairly simple factors and mechanisms.

In the next section (section 2), I will start with a description of Schelling's and Sakoda's model, and then demonstrate that, in a certain sense, *the Schelling model* is an instance of Sakoda's model. Section 3 goes into historical and systematic details of Sakoda's dissertation that finally lead him as early as the 1940s to a first version of his checker-board model. In this section an excursion into the history of World War II is necessary. Section 4 compiles pieces of evidence that clearly demonstrate how well recognized as a computational social scientist Sakoda once was, while Schelling, by that time, was an unknown in that community. A generation later, the pattern of recognition was almost completely reversed: Sakoda had become an unknown pioneer while Schelling was the well recognized inventor of the pioneering Schelling model. In sections 5 and 6 the puzzle will be stepwise resolved. Section 5 focuses on the *technical* preconditions for a systematic analysis of Sakoda's and Schelling's model. We analyze the differential computational "complexity" and the different status of the visualization problem in the two models. Together that had serious consequences for how knowledge about the models could spread and who could take up the models. In section 6 I discuss the Schelling-Sakoda case from a *sociology of science* point of view, namely in the light of what Merton called *the Matthew effect*. The effect regards the distribution of credit and recognition for comparable contributions: eminent scientists get a disproportionately great share, unknown scientists get a disproportionately little share. To judge the relative standing of Schelling and Sakoda in the 1970s, we have to trace and portray Schelling's career (Sakoda's career we know already from section 3 and 4). In doing that, we have to go into some details of the Cold War, the Vietnam War and Schelling's role as a strategist and defense intellectual. Additionally, we have to discuss Schelling's *Dynamical Models of Segregation* in the context of racial segregation as a hot policy issue in the 1960s and 1970s. The evidence suggests that the Matthew effect was at work—in favor of Schelling. In an excursion we discuss the question whether history did not take a revenge: In Schelling's family of segregation models is one member that he called "the tipping model" [1978, 101]. It is different from the Schelling model, without dispute *Schelling's* pioneering invention, but today better known as *Granovetter's* threshold model—the Matthew effect, now working against Schelling? In the final section 7, I pull together all the threads, give a recipe for how to become an unknown pioneer, look for guilty parties, and discuss the question of whether or not Sakoda's becoming an unknown pioneer was historically inevitable.

As a reading guide, although the main text of this study is accompanied by many footnotes, I tried to write in such a way that the central points can be understood *without* reading the footnotes. The footnotes provide evidence (especially if one may have doubts); they add additional contextual information; follow a bit a side story; or they give hints to the literature in cases an incorporation of the hints in the main text (what I normally do) would have caused inconvenience in reading the main text. There is also a network of cross references between footnotes. The reader may follow

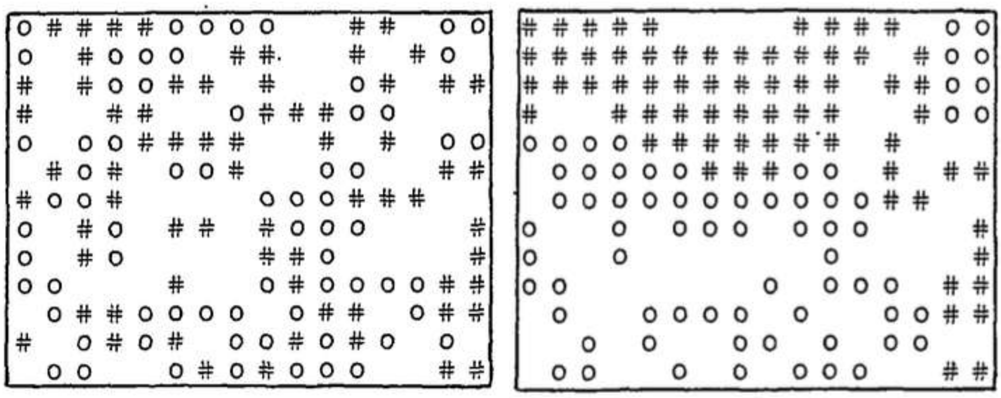

Figure 3: *Left*: random start distribution [Fig. 7 Schelling, 1971a, 155] *Right*: Segregation as an equilibrium [Fig. 8 Schelling, 1971a, 157]

such cross references—or not. Figure 32 on p. 127 shows a timeline for Sakoda's and Schelling's life. It probably helps to have, during the reading, from time to time a look at that timeline. Finally, all publications or (often manually duplicated) technical reports of Sakoda are mentioned at some point in the text. I believe I have covered all of his work (co-authorships included).

2 The Two Models

An analysis of the relations between Schelling's and Sakoda's model requires descriptions that precisely formulate the essential details of the two models (section 2.1 and section 2.2). That, then, will allow to demonstrate in a qualified and fairly precise sense, that two central variants of Schelling's model are instances of Sakoda's model (section 2.3).

2.1 Schelling's Model

In his *JMS* article, Schelling presents a one- and a two-dimensional spatial model. In the following I will focus exclusively on the two-dimensional model. The decisive features of Schelling's two-dimensional segregation model are [cf. 1971a, 154ff.]:

1. The playground is a *finite checkerboard*. The actual size is always 13×16. Other checkerboard shapes and sizes are possible.[16]

2. There are *two groups,* graphically displayed as zeros and crosses, physically realized as coins, chips, counters, aspirins etc. The primary interpretation is that the

[16] As Schelling notices, size and shape of the checkerboard have consequences for the proportion of cells (and thereby people) located at the borders. There they have less neighbors than inside the checkerboard. Schelling did not consider to use the surface of a torus as the playground of his model. On a torus we have a finite number of cells, but no borders and corners. As a consequence, *all* cells have the *same* number of neighboring cells.

tokens are people, belonging to two ethnic groups, blacks and whites. Other interpretations are possible, e.g. as boys and girls. The groups may be of different size. However, equal size is the starting point for the analysis.

3. The tokens normally are randomly scattered across the board. Starting with a specific constellation is also possible. Each cell can be inhabited by one and only one individual. About 25-30% of the cells remain empty to give enough clearance for movement.

4. All individuals define their *neighborhood* in terms of neighboring cells that surround their actual position. The standard neighborhood is the eight *other* cells in the 3×3 area directly adjacent to a cell in the center of that area. But larger neighborhoods, for instance the twenty-four cells of a 5×5 area, are also mentioned. Each individual has a *neighborhood preference* that states—in absolute or relative terms—the color composition that it wants to have in its neighborhood. The preferences are defined in a variety of ways: the standard case is a minimum demand for like colored neighbors, e.g. the requirement of not being the minority. Empty cells may be counted as others—or not [cf. ibid. 165f.]. The standard case is that neighborhood preferences are stated in terms of *lower* bounds.[17] However, having both, a lower *and* an upper bound for like colored neighbors, and a kind of scaled preferences over the possible color compositions is considered. The neighborhood preferences are the same within a group, but may differ between groups.

5. All individuals evaluate their neighborhoods at any given time. Individuals whose neighborhood does not accord with their preference are *discontent*. Otherwise they are *content*.

6. The individuals can move. Different rules for the *order of movement* are mentioned, such as working from the upper left corner downward to the right, working from the center outward or letting one group move first.[18] (Probably a computer based chance mechanism would have been implemented if Schelling had not opted for a computer-free simulation.)

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[17] Doing that is a bit more complicated than expected:

> Color preferences with respect to one's neighborhood can be defined either in absolute terms—the number of one's own color within the eight surrounding squares—or in relative terms—the ratio of neighbors of one's own color to opposite color among the eight surrounding squares. If all squares were occupied, every absolute number would correspond to a ratio; but because one may have anywhere from zero up to eight neighbors, there are eight denominators and therefore eight numerators to specify in describing one's neighborhood demands [ibid. 155].

[18] Schelling writes:

> Because what is reported here is all done by hand and eye, no exact rule for the order of moves has been adhered to strictly [ibid. 155]. ...The *particular* outcome will depend very much on the order in which discontent stars and zeros are moved, the *character* of the outcome not very much [ibid. 156].

7. Content individuals always *stay* where they are, but a discontent individual *moves* to "the nearest spot that surrounds him with a neighborhood that meets his demand" [ibid. 155]. Distance between two cells is measured in terms of the smallest number of cells that one traverses (horizontally and vertically) to get from one cell to the other.[19] Schelling gives no specification for the case that there is more than one such nearest satisfying spot. It makes a lot of sense to assume a random decision with equal probabilities for all such nearest locations.

When it comes to Schelling's model, there is usually one finding, that is mentioned first. It is a striking macro effect, unintended by the preferences of the individuals *in* the model, and unexpected by the uninitiated observer *of* the model [cf. ibid. 158]: Mildly segregationist preferences that demand not to be a minority in one's neighborhood, are sufficient to generate a massively segregated checkerboard society. Slight segregation starts already with a demand for one-third of like colored neighbors. In short: massive segregation does *not* require massively segregationist preferences.

This is a result under the condition of equal numbers in the two groups with equal minimum demands. But, Schelling found much more—and less known—interesting and *ex ante* counterintuitive results [cf. ibid. 158–66]. He experimented with equal numbers with unequal demands, unequal numbers with equal demands, congregationist preferences (they demand a certain absolute number of like neighbors, no matter whether the other neighboring cells are opposite or empty), or integrationist preferences (they demand a certain ratio or come with both, a lower *and* an upper bound for the fraction of like neighbors).[20] Among many other observations, Schelling finds the following effects:

- Under the condition of equal numbers the more demanding group ends up with an only slightly higher average proportion of like neighbors [cf. ibid. 159].
- If demands are equal, but one of the two colors is strongly outnumbered, segregation is much greater than in the equal number case [cf. ibid. 161].
- Being indifferent between empty cells and the other color while requiring significantly less than a majority of like color, causes almost the same degree of segregation as requiring a majority [cf. ibid. 165].

[19] That is the so-called *Taxicab metric*.

[20] Schelling complains that some interesting results of his original *JMS* article have been forgotten since people normally refer to the abbreviated version in ch. 4 of his *Micromotives and Macrobehavior* [1978]:

> References to my model are usually to the version in the book, not to the original. I've seen no reference, for example, to the results I got when I postulated a strong preference for neighbors of opposite type. If one is interested in the "neighborhood" effects of differences other than in color or race, especially with individuals of one type much scarcer than individuals of the other type, the "integrationist" preferences become highly plausible. (I put "neighborhood" in quotation marks because residence is not the only interpretation.) [2006, 1643].

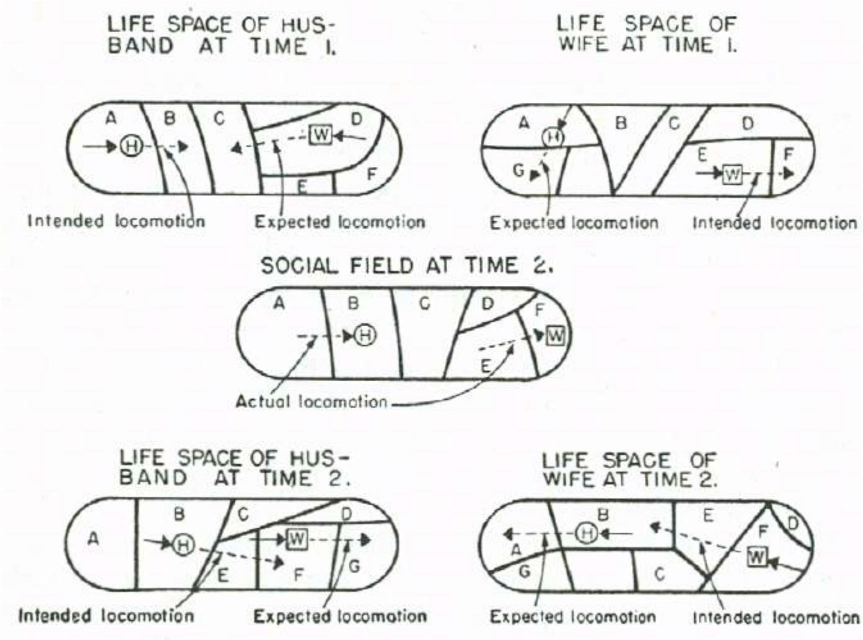

Figure 4: A representation *that does not work*: Lewin's description of a two-period two-person interaction dynamics (husband–wife); [Lewin, 1951, 196].

- Preferences that directly require integration, for instance by setting a lower *and* an upper bound to the like color, create a much more complicated patterning. It may well be the case that a minority has to be rationed. A high proportion of both colors may end up discontent [cf. ibid. 165f.].

What now is known as *the* Schelling model is a two-dimensional, finite checkerboard with two groups of moving agents, driven by a certain type of neighborhood preferences: For an agent to be content, requires a certain number or ratio of like neighbors within its 3×3 neighborhood. Only occupied cells count, i.e. empty cells are not counted as others. We refer to that model as the *standard* Schelling model. It is not an exaggeration to say that that model "may now be considered an archetype in the social sciences" [Gauvin et al., 2009, 293].

2.2 Sakoda's Model

Sakoda regards his checkerboard model as a step forward in the tradition of two authors. One was the American sociologist William Isaac Thomas (1863–1947) from whom he borrows the idea of *social attitudes* which decisively contribute to the individuals' *definition of the situation*. (We will go into personal and theoretical details in section 3.3 below.) The other, and probably more important, was Sakoda's inspiration from the psychological *field theory* of the German-American social psychologist Kurt Lewin (1890–1947).[21] Central concerns of Lewin's theory were an understanding of social dynamics by their underlying psychological forces.[22] Other central features of

[21] Kurt Lewin was originally a German. Being Jewish, he emigrated to the U.S. in 1933.
[22] Lewin writes:

Lewin's field theory are "to start with a characterization of the situation as a whole" [Lewin, 1951, 64] and a strong belief that mathematics, especially geometry and topology, provide useful means to represent the structure of psychological situations. Lewin used complicated diagrams to describe situations [cf. ibid. 195ff.]: For each actor a diagram represents his or her *life space* at time t with their respective positions, intentions, and expectations—as *subjectively* perceived. Thus, life spaces are subjective fields. The aggregate of all resulting actions is a new and more 'objective' *social field* for time $t + 1$, given by a third diagram. That field, then, leads to new subjectively perceived life spaces for each actor at time $t + 2$. As a consequence, a two-period two-person interaction requires already five diagrams. In an example analysis of a husband-wife-relation over two periods Lewin uses the diagrams given in *figure* 4. Two things are obvious (at least with the wisdom of hindsight): First, Lewin is somehow fumbling for a representation of the dynamical interplay of individuals. Second, Lewin's diagrammatical method is a dead end. Even for the *most simple* dynamical interaction with just *two* persons and *two* periods it is hard to describe and hard to understand what is going on. Sakoda saw that clearly. After a discussion of Lewin's diagrammatical approach he states that "what is needed is a less cumbersome means of relating individuals with subjective attitudes to one another" [Sakoda, 1971, 120].

The checkerboard model of social interaction is Sakoda's elegant solution to that problem. On less than two pages [ibid. 123f.], Sakoda gives a compact and precise description of his model. Replication is an easy task. The decisive details of what Sakoda calls "the rules of the game" [ibid. 122] are:

1. The playground is a checkerboard. The size can be varied from 2×2 to 12×12. The standard size in all examples is 8×8.

2. There are two groups with six members each, represented as squares and crosses. Sakoda, who wrote his article 25 years prior to the spread of the "agent" jargon, refers to the squares and crosses as *pieces* or *checkers* that represent group members.

3. Normally the checkers are randomly scattered across the board. But it is possible to setup specific configurations.

4. The members of the two groups have certain attitudes towards members of their own and the members of the other group. The attitudes can be represented by numbers in a kind of matrix (cf. *figures* 5 – 8). In each group the attitudes are the same (homogeneity) and constant over time.

   > Each checker is assigned a positive, neutral or negative valence or value. There are two sets of such values, one toward members of

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One of the basic characteristics of field theory in psychology, as I see it, is the demand that the field which influences an individual should be described not in "objective physicalistic" terms, but in the way in which it exists for that person at that time. ... To describe a situation "objectively" in psychology actually means to describe the situation as a totality of those facts and of only those facts which make up the field of that individual [Lewin, 1951, 62].

> one's own group and another toward members of the other group. These represent attitudes toward members of one's own or the opposing group [ibid. 123].

The assigned values are normally elements of the set {+1, 0, -1}.

5. The checker-agents evaluate actual or alternative checkerboard locations by the aggregated weighted sums of the values of all checkers on the whole checkerboard. In that aggregation more distant pieces count less: Sakoda defines the distance between an agent i at the position x_i, y_i and an agent j at position x_j, y_j as

$$D = \sqrt{(x_i - x_j)^2 + (y_i - y_j)^2}, \tag{1}$$

i.e. as the square of what is known as the *euclidian distance*. Depending upon i's and j's group membership, j has a certain positive, negative, or zero value V for agent i. That value counts in agent i's overall evaluation of location x_i, y_i according to

$$f = \sum \frac{V}{D^{\frac{1}{w}}}, \tag{2}$$

where w is a *distance weight*. In the runs reported by Sakoda he always uses a distance weight of 4. As a consequence, for agent i an agent j's value counts with the inverse of the square root of the euclidian distance between the two agents. The overall evaluation of x_i, y_i is, then, given by the summation over all other agents.[23]

6. The checkers can move. In one cycle each member of each group gets a migration option. They are used in a random order.

> Normally each piece takes one step on each move. A step can be up, down, or to the side one square or to one of the four diagonal cells, provided the cell in question is not occupied by another piece. If there is no advantage to making a move a piece stays where it is. To overcome a tendency of cohesive groups not to move after it is solidified, pieces which are unable to move are allowed to search a distance of two squares in all directions to find the most advantageous position. This, therefore, permits a jump over one square [ibid. 123].

7. The choice between feasible alternative positions is governed by a maximizing principle:

> Whenever it is the turn of a piece to move it checks all possible positions to which it is allowed to move and selects the move which has the highest positive value of f [ibid. 124].

[23] According to equation (1) the distance to oneself is 0. Therefore, according to expression (2) there is no defined value for being a 'neighbor of oneself'.

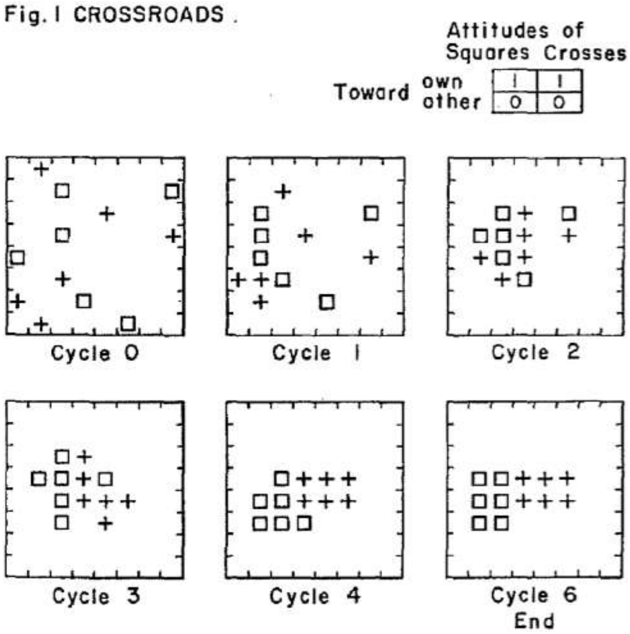


Figure 5: Crossroads [Sakoda, 1971, 125].

Figures 5 to 8 show runs based on four different attitude matrices. Sakoda calls them *crossroads*, *mutual suspicion*, *segregation*, and *couples* respectively.[24] In each case the *attitude matrix*—given top right in the figures—is the "engine" that drives the dynamics. It is interesting to see that the attitude combinations called *segregation* and *mutual suspicion* produce about the same final configuration. Obviously, dislike of another group combined with neutrality towards one's own group is sufficient to bring about radically segregated clusters. However, one should notice that the trajectories that lead to the similar final patterns, are quite different. The attitude matrix of the fourth example (*couples*) produces a final pattern that probably nobody would have expected. It is one of the many instances of Sakoda's model in which intuition fails completely. In the examples only $+1, 0, -1$ and -4 entries are used, but Sakoda's framework allows to model any degree of attraction by higher or lower positive values and any degree of repulsion by lower or higher negative values while a zero entry reflects neutrality. As a consequence, Sakoda's checkerboard model is a quite simple framework with a very rich set of applications. It permits the analysis of questions about the existence of final stable patterns, structures, and dynamics of patterns, the trajectories that lead to them, conditions under which they appear or disappear, mechanisms and interplay of mechanisms—to mention only a few.

From a philosophy of science point of view, Sakoda had a remarkably clear view on the status, potential and restrictions of conceptual models (emphasis added):

> The checkerboard model in its present form is *more of a basic conceptual framework than a model of any given social situation*. It has potentiality for

[24] In his article Sakoda presents four other attitude matrices, named *social climbers*, *social worker*, *boy-girl*, and *husband-wife*. The names indicate again the type of stylized situation that he has in mind as the "target system".

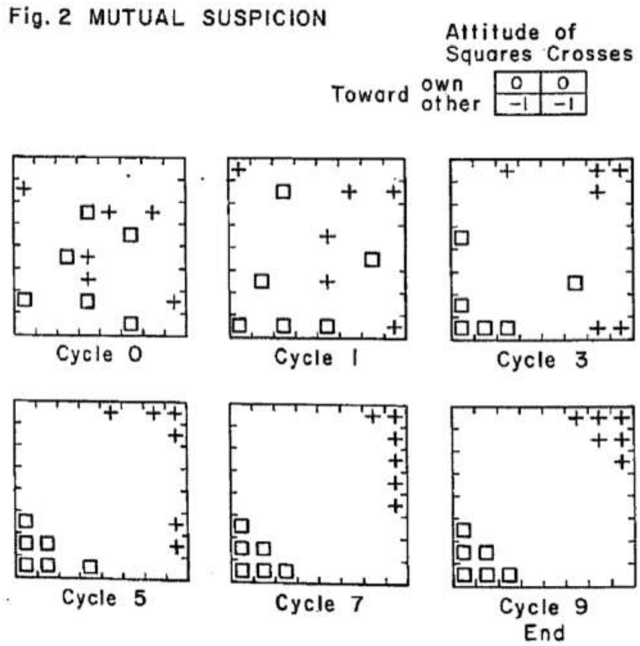


Figure 6: Mutual Suspicion [Sakoda, 1971, 126].

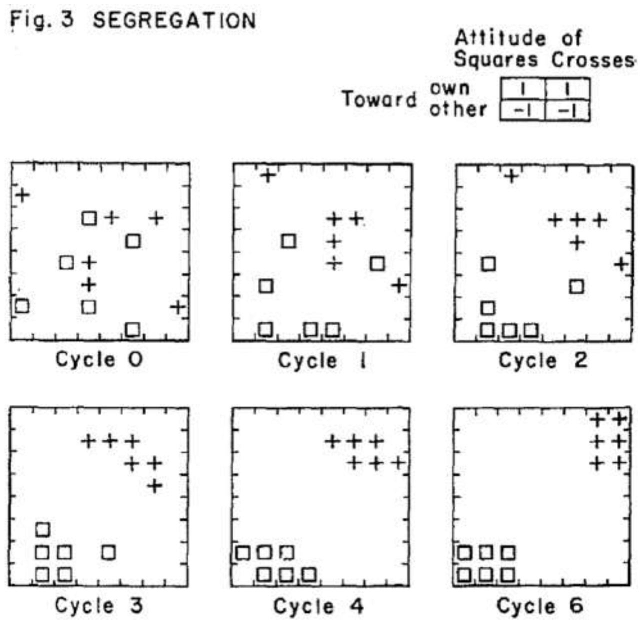


Figure 7: Segregation [Sakoda, 1971, 127].

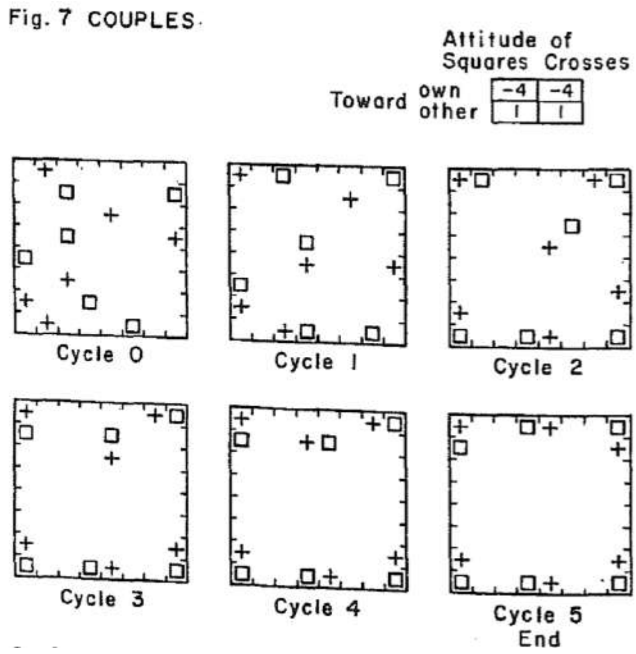


Figure 8: Couples [Sakoda, 1971, 130].

> further elaboration to fit particular situations. As it now stands, it can be used as *a visual representation* of the social interactional process, relating attitudes, social interaction and social structure. It should be particularly useful in introductory courses, not only illustrating the relationship among these concepts, but also in discussing the function of models. A model is *not necessarily used to predict* behavior in a situation. Model building is *useful in clarifying the definition of concepts and the relationship among them*. Left in verbal form, concepts can be elusive in meaning, whereas computerization require precision in definition of terms. Models can be used *to gain insight into basic principles of behavior* rather than in finding precise predictions of results for a given social situation, and it is this function which the checkerboard model in its present form provides. ...The checkerboard model provides students of social structure with a *possible explanation* of its dynamics [ibid. 121f.].

Additionally, Sakoda seems to be well aware of the danger of artifacts caused by inherent though arbitrary features of the framework. The size of the board might matter, the two dimensions, the distance weight w or the specific rules for movement on the board.[25] Sakoda's 71-model combines simplicity and richness in a way that is not often found among modelers. And it is presented with an understanding that is even rarer. That was not an accident: As we will see soon, when Sakoda published *The*

[25] Cf. [ibid. 123, 132]. Like Schelling, Sakoda did *not* realize that a checkerboard might be used as the visualization (or projection) of a two-dimensional world *without* boundaries, namely a torus, what might have a dramatic impact on the resulting structures and their dynamics. Additionally, Sakoda was not aware that the type of the cells (rectangular, triangular, hexagonal, irregular Voronoi-diagrams) might matter [cf. Hegselmann and Flache, 1998, 3.3ff.].

Checkerboard Model of Social Interaction in 1971, he must have been thinking about that model and the problems and perspectives of modeling in general for a quarter of a century.

2.3 Schelling's Model: An Instance of Sakoda's Model?

There are similarities between Schelling's and Sakoda's model. To spell them out, we can describe both models by some key ingredients:

1. *Group structure:* Both distinguish two groups.
2. *Social space:* Both Sakoda and Schelling use a checkerboard, i.e. a finite two-dimensional grid as a kind of underlying space on which agents live and move.
3. *Neighorhood evaluation:* Sakoda's and Schelling's agents evaluate their neighborhood. In Sakoda's model that evaluation is based upon an attitude matrix. The whole world counts, but more distant neighbors count less. In Schelling's model the evaluation is based upon neighborhood preferences. In the standard case only a 3×3 neighborhood around the agent in the center cell counts.
4. *Migration regime:* In both models agents can move, albeit guided by some rules. In both models migration is allowed only to empty cells within spatial limits. In Sakoda's model the agents maximize aggregated attitude values. In Schelling's model agents move to cells that meet their neighborhood preference.

Obviously, Schelling's and Sakoda's model have *four common key components*—and their specific design is partially even the same. But isn't there even more? How is Schelling's segregation model related to that *instance* of Sakoda's model, in which the dynamics is driven by the attitude matrix that Sakoda christened *segregation*? Is that *accidentally* the same word for two different social processes? Or is the occurrence of the same word an indication of something more substantial: a deep similarity, perhaps very close to identity, between Schelling's model and that particular instance of Sakoda's model? The answer to the former question will be "No"; the answer to the latter question will be a qualified "Yes". To see this requires some further specifications and a partial translation of both models into a common language. That language will be the language of *utilities*, *utility functions*, and *decision principles* based upon them. We start with the *standard* Schelling model. Crucial points are the neighborhood demands and the rules of movement.

In the *standard* Schelling model the attractiveness of a location depends upon the size of one's own group in one's neighborhood, and that with regard to the total number of *occupied* cells in the neighborhood. Neighborhood demands can be defined in absolute or relative terms. The former require a specification for all possible numbers of *occupied* cells within a neighborhood. In case of a 3×3 neighborhood, the number of other occupied cells can be any number between 0 and 8 (see fn. 17 above). Ratio

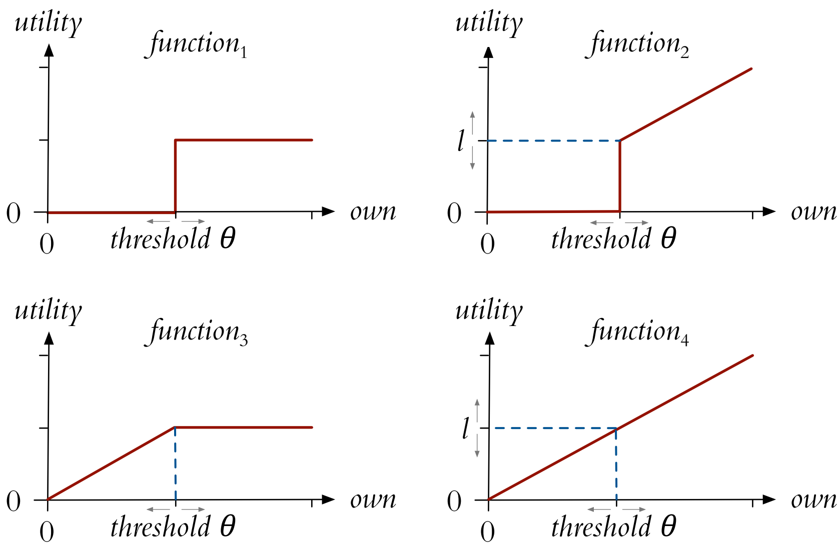


Figure 9: Alternative utility functions for a representation of Schelling's model. *Top*: The *standard* Schelling model; left: maximizing; right: satisfycing. *Bottom*: Locations to the left of θ are *not* equally bad; left: maximizing; right: satisfycing.

based demands require further specifications since rounding can easily lead to inadequate results.[26] In the following we represent neighborhood preferences by utility functions in which the utility of a neighborhood depends upon the *absolute* number of members of one's own group. Without any explicit notation, we take the number of occupied cells as given. The formal structure of the utility functions is the same for all possible numbers of occupied cells within a neighborhood.

As to the regime that governs the movement of agents, Schelling writes in his *JMS* article:

> I specify the rule for moving, which is usually to move to the nearest satisfactory square, with "nearest" measured by the number of squares one traverses horizontally and vertically [1971a, 155].

This specification is incomplete. It is not specified what to do if there is *more than one* satisfactory cell. Additionally, there is not a single word about what individuals do if there isn't any satisfactory empty cell. The most plausible and coherent interpretation is that such individuals simply do not move; they stay where they are.[27]

Figure 9, top row, shows two utility functions that both, in combination with certain decision principles, could be at work in the standard Schelling model—which one depends upon the chosen description:

[26] For example, simple rounding a demand of a 0.2 share of like color neighbors in a situation with a total of 2 neighbors implies that an agent is satisfied with having no like color neighbor at all. Is that adequate?

[27] Otherwise there is a hole in Schelling's description. The hole is irrelevant if, and only if, the existence of satisfactory locations is guaranteed. But that is not under all circumstances the case.

- According to function_1 all locations with a number of like neighbors equal or above a certain threshold θ are equally good: they are all *best* solutions. All locations to the left of θ are equally bad. Always moving to the nearest satisfactory square amounts to following a *maximize-utility-principle*. In the case of more than one nearest best location a chance mechanism could decide. Given the utility function_1 it makes sense to stay if there is no best empty square: All locations, that are no best locations are equally bad. Why, then, move around?

- We get an equivalent description of the standard Schelling model if we assume utility function_2 in combination with a *satisfycing-principle* for moving. The right part of utility function_2 is strictly increasing with an increasing number of like neighbors. However, the individuals do not maximize their utilities. As satisficers with an aspiration level l they are indifferent among all locations that yield a utility of at least l. Again, a chance mechanism could decide if there is more than one satisfactory location. Again, moving does not make sense if the only feasible locations have numbers of like neighbors in the range to the left of θ.

At this point it becomes clear that the standard Schelling model is not *per se*—as it is often assumed or suggested in passing—a model with satisficing individuals. What it is depends upon the chosen *description*. Under a description based on utility $\textit{function}_1$ the individuals are *maximizers*; under a description that is based on utility $\textit{function}_2$ they are *satisficers*.

Who thinks it is inadequate that according to $\textit{function}_1$ and $\textit{function}_2$ all locations to the left of θ are equally bad, could resort to utility $\textit{function}_3$ or $\textit{function}_4$ of figure 9, bottom. In both functions the utilities increase with the number of like neighbors. Therefore even to the left of θ moving does make sense. Utility $\textit{function}_3$ together with the maximize-utility-principle and utility $\textit{function}_4$ together with a satisfycing principle result again in the very same model. (Note that both descriptions require additional rules to break ties.)[28]

	g_1	g_2
g_1	u_{11}	u_{12}
g_2	u_{21}	u_{22}

Table 1: Translating Sakoda: From valences to utilities

To translate Sakoda's checkerboard approach into the language of utilities is easy. We simply consider the entries in Sakoda's attitude matrices as utilities. This turns

[28] In an old, but only recently published essay [cf. Hegselmann, 2012] Schelling considers other rules of movement:

> Maybe we drop the notion of satisfied and simply say that everybody compares every available vacant spot with the spot he's at, and moves to the best spot available if it's better than where he's at [Schelling, 1972a, 12].

That suggests a monotonically and strictly increasing utility function like $\textit{function}_4$, but now combined with a maximize-utility-principle.

Sakoda's attitude matrices turn into *utility matrices* that represent the externalities of neighbors of certain types. Table 1 gives the general structure. g_1 and g_2 are the two groups. u_{ij} is the utility of a member of group j for a member of group i $(i, j = 1, 2)$. In line with Sakoda's original model, the agents then follow a *maximize-utility-principle*: they move to a feasible position that maximizes the aggregated utilities as described in equation 2 in section 2.2. While for the aggregation all locations of the world count (though with distance depending weights), the feasible alternative locations are only locations close by. Moving is allowed to empty cells within a 3 × 3 neighborhood; if there is no empty cell, it is allowed "to search a distance of two squares in all directions" [Sakoda, 1971, 123]. The latter might imply a 5 × 5 neighborhood.[29] A rule for how to break ties is missing and has to be added: agents stay where they are if there is no strictly better feasible location.

	g_1	g_2
g_1	1	-1
g_2	-1	1

Table 2: Sakoda's segregation matrix. Cf. *figure* 7 above.

With this specification and translation work, we can now directly try to reformulate as much as possible of the standard Schelling model within Sakoda's framework. We do that in four steps:

1. We consider a model, that is "driven" by a special instance of Sakoda's former attitude matrices (now—after our translation—called *utility matrix*). We use Sakoda's *segregation* matrix as given by table 2.

2. We *substitute* Sakoda's continuous, distance depending weight function by a weight scheme as used by Schelling: within a 3 × 3 neighbourhood all cells have the weight 1; outside the weight is 0.

3. We translate Sakoda's aggregation procedure, given by equation 2 in section 2.2 into the language of utilities. With $U_i(x)$ for the utility of a location x for an agent of group i that translation is simply

   $$U_i(x) = \sum_{neighborhood(x)} u_{ij}. \tag{3}$$

   The simplification of Sakoda's original equation is a direct consequence of the weight scheme which assigns the same weight 1 to all cells within the 3 × 3 neighborhood of cell x as the center cell.

4. We introduce a complete set of rules for moving decisions of any agent i at any location x. In the rules that we formulate below l_i is the aspiration level of members of group i. The rules are:

[29] Since Sakoda does is not explicitly define *distance* there is an ambiguity: If for the distance count a common border *line* between squares is required, then the feasible area is much smaller than the 5 × 5 neighborhood. If a common border *point* is sufficient, then it is the 5 × 5 neighborhood.

(a) Move to the nearest empty cell y where $U_i(y) \geq l_i$.

(b) If there is more than one such locations, decide by a lottery.

(c) Stay at your actual location x if

i. $U_i(x) \geq l_i$

ii. $U_i(x) < l_i$ *and* no other location y exists for which $U_i(y) \geq l_i$.

Rule (a) makes satisficing rather than maximizing the decisive principle. Rule (b) breaks ties and (c) explicitly states when to stay.

In the following I refer to the model characterized by (1)–(4) as $Model^{Sakoda}_{segregation}$. To the standard Schelling model I will refer as $Model^{Schelling}_{standard}$. A few lines of algebra show that $Model^{Sakoda}_{segregation}$ *is equivalent* with the completed standard Schelling model.

Let n_i be the number of agents of one's own group within one's neighborhood; n_j be the number of members of the other group. Decisive for moving is rule (a). We apply the segregation utility matrix of table 2 and get

$$\begin{aligned} U_i(y) = n_i(1) + n_j(-1) &\geq l_i && (4) \\ n_i - n_j &\geq l_i && (5) \\ n_i &\geq l_i + n_j && (6) \end{aligned}$$

Now suppose an aspiration level of $l_i = l_j = 0$. As a consequence we get

$$U_i(y) \geq 0 \Longleftrightarrow n_i \geq n_j \tag{7}$$

Thus, the satisficing agents of $Model^{Sakoda}_{segregation}$ with an aspiration level of 0 do the following:

- They search for empty cells where they wouldn't be the minority.
- If there is more than one such location, then a lottery decides.
- They stay where they are, if they aren't the minority.
- They also stay if there isn't any location where they aren't outnumbered.

That behavior is *exactly* the behavior of the agents in the (completed) standard Schelling model in which the threshold θ_i $(i = 1, 2)$ is such that the number of the other color is not greater than the number of like color neighbors.

In general, for any θ_i of $Model^{Schelling}_{standard}$ the corresponding l_i of $Model^{Sakoda}_{segregation}$ is given by

$$l_i = \theta_i - max[n_j], \tag{8}$$

where $max[n_j]$ is the maximum possible number of neighbors of the other group j, given that the minimum demand of neighbors of one's own group, i.e. θ_i, is exactly met. For the special case that a group does not want to be a minority it holds that $\theta_i = max[n_j]$. As consequence we get an aspiration level $l_i = 0$.

To sum up: whatever the thresholds θ_i in $Model^{Schelling}_{standard}$ may be, whether we describe the agents in that model as maximizers or satisficers acting upon a type of utility functions as given by $function_1$ or $function_2$ in figure 9, there always exists an equivalent $Model^{Sakoda}_{segregation}$ with satisficing agents acting upon a corresponding aspiration level l_i. *Equivalence* here means a kind of *behavioral* equivalence: the agents in both models behave under the same circumstances in exactly the same way (of course, given a *joint* randomization whenever randomness is involved).

The easy translatability of Schelling's model into a Sakoda style model does not come to an end with what we called the (completed) *standard* Schelling model. The translation of the *non*-standard variant in which the dynamics is driven by what Schelling calls *congregationist* preferences [cf. Schelling, 1971a, 165] is even easier. The preferences in the standard model require a certain number of one's own group with regard to the total number of *occupied* cells in one's neighborhood. Congregationist preferences simply require a certain number of one's own group in one's neighborhood, for instance, 3 out of the 8 other squares in one's 3 × 3 neighborhood. Whether all or some of the 5 remaining cells are empty or occupied by the other group does not matter. The only concern is *congregating* with one's own group. As a consequence, all the complications that we had before, caused by different possible numbers of occupied cells, disappear. The range of the utility $function_1$ and $function_2$ is always the same, namely [0, 1, ..., 8] in case of a 3 × 3 neighborhood.

Again, the attitude or utility matrix that we need for the translation of this variant of Schelling's model is already given in Sakoda's *JMS* article. It is Sakoda's very first pattern. He calls it *crossroads*. It is given by table 3.

	g_1	g_2
g_1	1	0
g_2	0	1

Table 3: Sakoda's crossroads matrix. Cf. figure 5.

The utility of a member of the other group now is 0 (instead of −1 in the standard Schelling model). In Sakoda's model the utility of an empty cell is always 0, as well. Consequently, for the aggregation of utilities it doesn't matter whether a cell is empty or occupied by a member of the other group. For the details of the translation into Sakoda's framework we proceed exactly as we have done it in steps (1)–(4) above with the standard model—except that we now use the crossroads matrix of table 3. Let us postulate that

$$l_i = \theta_i, \tag{9}$$

where θ_i is the congregationist minimum demand of members of group i by group i within one's neighborhood. Using this aspiration level l_i we again obtain a model $Model^{Sakoda}_{crossroads}$ that is behaviorally equivalent with $Model^{Schelling}_{congregation}$, i.e. Schelling's model variant based upon congregationist preferences. The demonstration is trivial:

$$U_i(y) = n_i(1) + n_j(0) \geq l_i \quad (10)$$
$$n_i \geq l_i. \quad (11)$$

Together with $l_i = \theta_i$ we get

$$U_i(y) \geq l_i \Longleftrightarrow n_i \geq \theta_i. \quad (12)$$

Thus, the satisfycing agents of $Model^{Sakoda}_{crossroads}$ with an aspiration level of θ_i do the following:

- As members of group i they search for empty cells with a neighborhood that has at least θ_i members of group i.
- If there is more than one such location, then the new location is decided by a lottery.
- They stay where they are if they have at least θ_i members of their group i.
- They stay as well, if there isn't any location where they have at least θ_i members of their group i.

Obviously $Model^{Sakoda}_{crossroads}$ is behaviorally equivalent with $Model^{Schelling}_{congregation}$ (at least after some minor completion work).

How to interpret our equivalence results? Can we say that both the standard Schelling model and the non-standard congregationist variant are just instances of Sakoda's more general model? The answer depends on what we consider the hard core of Sakoda's checkerboard model.

- Both, $Model^{Sakoda}_{segregation}$ and $Model^{Sakoda}_{crossroads}$, give up Sakoda's original continuous locality concept. Instead, Schelling's original neighborhood scheme is used. (However, we could use a value of the parameter w in Sakoda's original equation (2) that causes a sharp decline of weights for increasing distances. That would approximate the effects of Schelling's 3×3 neighborhood within Sakoda's *unmodified* framework.)
- Sakoda's original decision principle is a maximizing principle. In $Model^{Sakoda}_{segregation}$ and $Model^{Sakoda}_{crossroads}$ that is not the case. Both use (and have to use) a satisficing principle.

Thus, if Sakoda's neighborhood concept or his maximizing decision principle are considered to be essential components of what one might call *Sakoda's model*, then, consequently, $Model^{Sakoda}_{segregation}$ and $Model^{Sakoda}_{crossroads}$ are *not even* variants of Sakoda's model. But *if* we think of Sakoda's attitude matrix as the very *heart of his model*, and that it might be combined with different decision principles or neighborhood concepts, *then* Schelling's standard model and also his congregationist variant are both just applications of Sakoda's segregation and crossroads matrices.

Figure 10: Reproduction from [Hansen, 1994, xxxiii]. In his caption Hansen writes that Sakoda is “depicted pursuing his photography hobby while living and attending school in Japan during the mid-1930s” [ibid.]. Photo courtesy of Center for Oral and Public History, California State University, Fullerton.

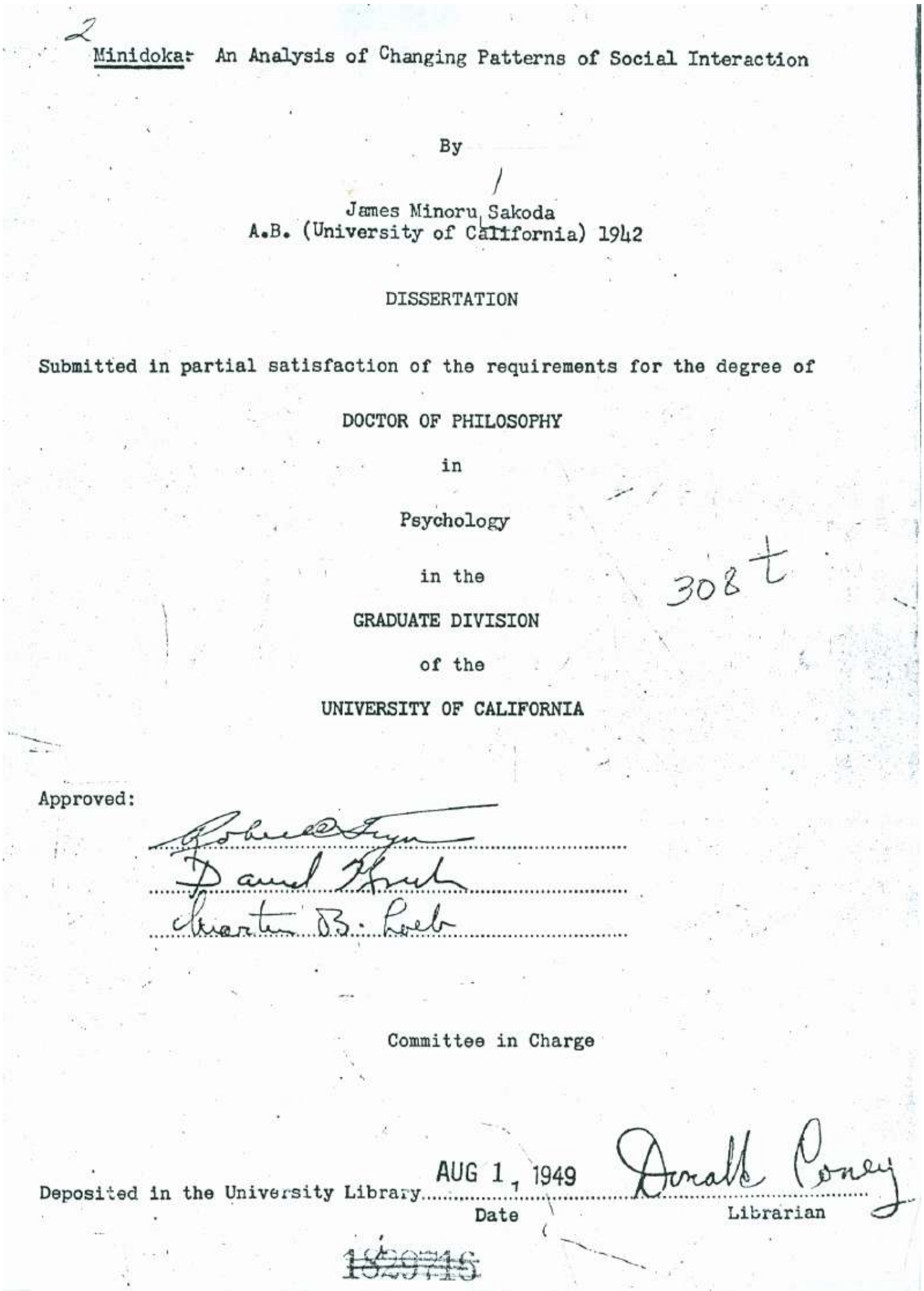
3 The Prehistory: Sakoda’s 49-Model

In 1971, the checkerboard model of social interaction was completely new for the scientific community. For Sakoda, a first version of his model was already more than two decades old. It had been a part of his unpublished PhD thesis [Sakoda, 1949]. From a stamp we know that a copy of the thesis was deposited in the library of the University of California on August 1, 1949 (see figure 11). We will refer to this early version as Sakoda’s 49–*model* and distinguish it from his 71–*model*. Sakoda presents the early version in a five-page appendix, entitled, *A Checkerboard Conceptual Model for the Study of Social Interaction* [ibid. 417–21]. The model is already announced in the final chapter *XI Conclusion* [ibid. 388]—as the last of “some next steps” [ibid. 383–89].

For an understanding of the origin, status, and functioning of Sakoda’s 49-model it is necessary to review his PhD thesis, his life, and even some details of the military and political history of World War II.

3.1 Research Behind Barbed Wires: The Political and Military Background of Sakoda’s PhD-Thesis

The title of Sakoda’s thesis, *Minidoka – An Analysis of Changing Patterns of Social Behavior*, is not self-explanatory. What is Minidoka? Minidoka was one of the centers to which all Americans of Japanese ancestry that lived at the Pacific Coast finally were “evacuated” some months after Japan’s attack on Pearl Harbor. Sakoda was one of



2

Minidoka: An Analysis of Changing Patterns of Social Interaction

By

James Minoru Sakoda
A.B. (University of California) 1942

DISSERTATION

Submitted in partial satisfaction of the requirements for the degree of

DOCTOR OF PHILOSOPHY

in

Psychology

in the

GRADUATE DIVISION

of the

UNIVERSITY OF CALIFORNIA

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Approved:

Committee in Charge

Deposited in the University Library AUG 1, 1949 Date Librarian

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Figure 11: Cover of Sakoda's dissertation from 1949. The signatures of the committee members are the signatures of Robert Tryon, David Krech, and Martin B. Loeb.

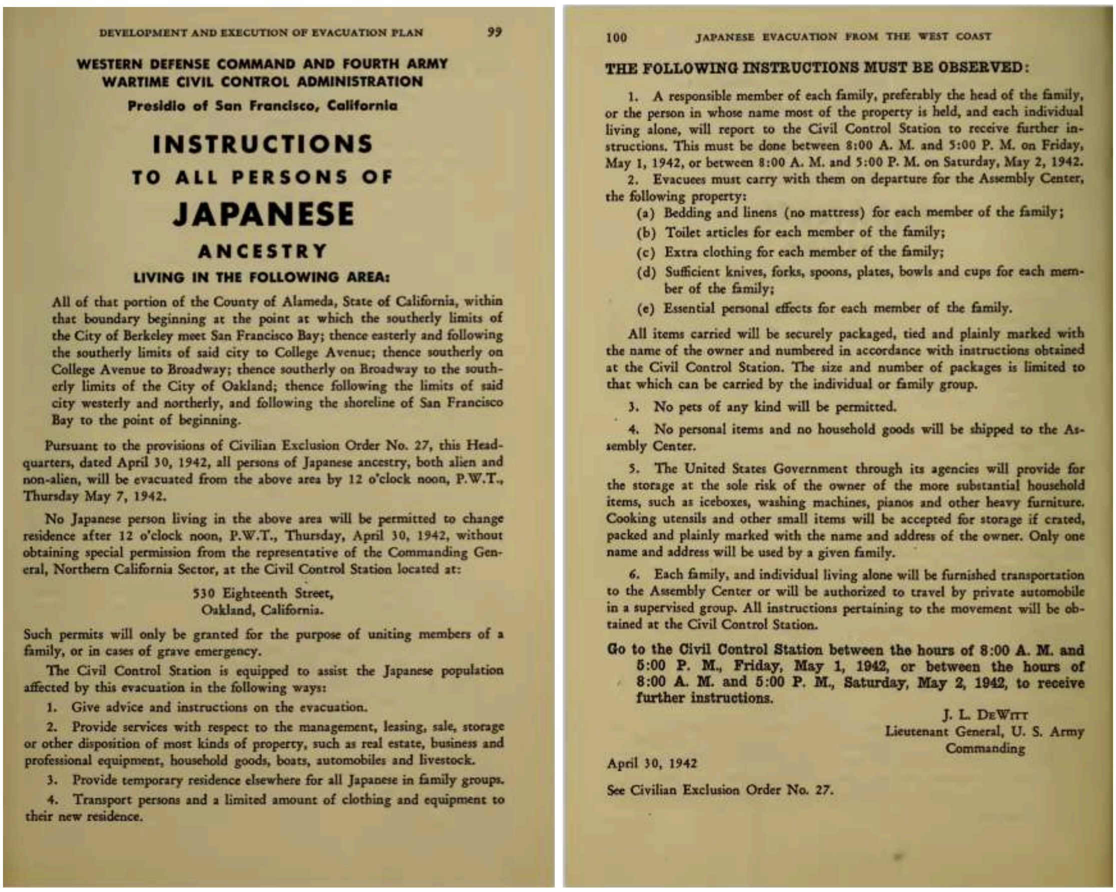
the “evacuees”—and, simultaneously, a young social psychologist doing research on the social processes in these centres. He had been born 1916 in California, an area of the US where most of the Japanese minority settled. Sakoda describes his upbringing and family background in an *autobiographical sketch* right at the beginning of his dissertation [1949, 4–8]. Four decades later, in [Sakoda, 1989b, 220f.], he does that again, now with some more details of his cultural environment. Additionally, there is a long interview with Sakoda, held by Arthur A. Hansen on August 9 and 10, 1988 [Hansen, 1994, 343–446]. The interview covers Sakoda’s family background, education, career, imprisonment as an “evacuee”, life and work during the war, post-war career, early and later views.[30]

Both of Sakoda’s parents were Japanese immigrants. Sakoda was the “third of four children, brought up within the Japanese community of Los Angeles” [Sakoda, 1949, 5]. Due to economic problems his parents returned to Japan in the early 30s. In Japan Sakoda “became highly conscious of his American-born status . . . He, as well as the other children, then decided to return to the United States since they did not believe that they could be happy in Japan” [ibid.]. (The photography figure 10 shows Sakoda at about that time.) Sakoda “arrived on the West Coast in 1939 to pursue his college education in Oriental languages and psychology” [ibid.].

In the early morning, December 7, 1941, war broke out with a massive surprise attack of Japanese fighter planes on Pearl Harbor, a naval basis near Honolulu, Hawai’i. Not surprisingly, it took only some hours and enemy aliens were subject to security measures (most of the measures—in the face of increasing political tensions—prepared already in the months and years before).[31] The initial restrictions were imposed on *all* enemy aliens, i.e. Germans, Italians, and Japanese. By all standards (then and now), the initial restrictions were basically moderate, appropriate for wartimes: selective imprisonment of suspects, travel restrictions, and contraband orders. Several hundred enemy aliens were arrested the day after Pearl Harbor. From about 16,000 enemy aliens arrested in the first months, two thirds were released again within days. Lots of politicians, political groups, newspapers, and media, explicitly warned against a generalized suspicion against enemy aliens, or trashing the civil rights of persons of German, Italian, or Japanese ancestry [cf. Stanley, 1992, 182f.]. At the same time,

[30] Hansen held the interview with Sakoda for the Japanese American Project of the Oral History Program at California State University, Fullerton. For information on the whole oral history project cf. [Hansen, 1995].

[31] The following short summary of political and administrative decisions and processes is based upon the detailed descriptions in [Thomas and Nishimoto, 1946, ch. I, ch. II], [Grodzins, 1949], [Thomas, 1952b, 78ff.] [tenBroek et al., 1954, ch. III], [CWRIC, 1982], [CWRIC, 1983], [Stanley, 1992], [Daniels, 1993], [Hayashi, 2004, ch. III], [Robinson, 2010], [Hastings, 2011]. Easily accessible are the books of Daniels and Robinson. The *Densho Encyclopedia* is a free on-line resource about the history of the Japanese American World War II exclusion and incarceration experience at `http://encyclopedia.densho.org`. The entries are sorted alphabetically and by category. The encyclopedia has an entry “James Sakoda” [cf. Niiya, 2015b]. Additionally, there are several archives with source material. Probably the two best archives are: First, *The Japanese American Evacuation and Resettlement: A Digital Archive* (The Bancroft Library, University of California Berkeley) at `http://bancroft.berkeley.edu/collections/jais/abouttheproject.html`; second, the *Japanese American Research Project collection about Japanese in the U.S. (Yuji Ichioka papers), 1893-1973* (OAC, Online Archive of California) at `http://www.oac.cdlib.org/findaid/ark:/13030/tf6d5nb3z6/`.



DEVELOPMENT AND EXECUTION OF EVACUATION PLAN 99

WESTERN DEFENSE COMMAND AND FOURTH ARMY
WARTIME CIVIL CONTROL ADMINISTRATION

Presidio of San Francisco, California

INSTRUCTIONS
TO ALL PERSONS OF
JAPANESE
ANCESTRY
LIVING IN THE FOLLOWING AREA:

All of that portion of the County of Alameda, State of California, within that boundary beginning at the point at which the southerly limits of the City of Berkeley meet San Francisco Bay; thence easterly and following the southerly limits of said city to College Avenue; thence southerly on College Avenue to Broadway; thence southerly on Broadway to the southerly limits of the City of Oakland; thence following the limits of said city westerly and northerly, and following the shoreline of San Francisco Bay to the point of beginning.

Pursuant to the provisions of Civilian Exclusion Order No. 27, this Headquarters, dated April 30, 1942, all persons of Japanese ancestry, both alien and non-alien, will be evacuated from the above area by 12 o'clock noon, P.W.T., Thursday May 7, 1942.

No Japanese person living in the above area will be permitted to change residence after 12 o'clock noon, P.W.T., Thursday, April 30, 1942, without obtaining special permission from the representative of the Commanding General, Northern California Sector, at the Civil Control Station located at:

530 Eighteenth Street,
Oakland, California.

Such permits will only be granted for the purpose of uniting members of a family, or in cases of grave emergency.

The Civil Control Station is equipped to assist the Japanese population affected by this evacuation in the following ways:

1. Give advice and instructions on the evacuation.
2. Provide services with respect to the management, leasing, sale, storage or other disposition of most kinds of property, such as real estate, business and professional equipment, household goods, boats, automobiles and livestock.
3. Provide temporary residence elsewhere for all Japanese in family groups.
4. Transport persons and a limited amount of clothing and equipment to their new residence.

100 JAPANESE EVACUATION FROM THE WEST COAST

THE FOLLOWING INSTRUCTIONS MUST BE OBSERVED:

1. A responsible member of each family, preferably the head of the family, or the person in whose name most of the property is held, and each individual living alone, will report to the Civil Control Station to receive further instructions. This must be done between 8:00 A. M. and 5:00 P. M. on Friday, May 1, 1942, or between 8:00 A. M. and 5:00 P. M. on Saturday, May 2, 1942.
2. Evacuees must carry with them on departure for the Assembly Center, the following property:
   (a) Bedding and linens (no mattress) for each member of the family;
   (b) Toilet articles for each member of the family;
   (c) Extra clothing for each member of the family;
   (d) Sufficient knives, forks, spoons, plates, bowls and cups for each member of the family;
   (e) Essential personal effects for each member of the family.

All items carried will be securely packaged, tied and plainly marked with the name of the owner and numbered in accordance with instructions obtained at the Civil Control Station. The size and number of packages is limited to that which can be carried by the individual or family group.

3. No pets of any kind will be permitted.
4. No personal items and no household goods will be shipped to the Assembly Center.
5. The United States Government through its agencies will provide for the storage at the sole risk of the owner of the more substantial household items, such as iceboxes, washing machines, pianos and other heavy furniture. Cooking utensils and other small items will be accepted for storage if crated, packed and plainly marked with the name and address of the owner. Only one name and address will be used by a given family.
6. Each family, and individual living alone will be furnished transportation to the Assembly Center or will be authorized to travel by private automobile in a supervised group. All instructions pertaining to the movement will be obtained at the Civil Control Station.

Go to the Civil Control Station between the hours of 8:00 A. M. and 5:00 P. M., Friday, May 1, 1942, or between the hours of 8:00 A. M. and 5:00 P. M., Saturday, May 2, 1942, to receive further instructions.

J. L. DeWitt
Lieutenant General, U. S. Army
Commanding

April 30, 1942

See Civilian Exclusion Order No. 27.

Figure 12: Instructions to all persons of Japanese ancestry [deWitt, 1943, 99–100].

especially the so called *Nisei*, U.S. citizens of Japanese ancestry, publicly demonstrated their loyalty to the U.S. in all forms (demonstrations, posters, ads, blood donations, buying war bonds)—all that recognized, acknowledged, and cheered by the public.

However, within little more than two months after the outbreak of war, the situation changed dramatically and extremely severe restrictions that targeted *all, and only at* persons of Japanese ancestry, regardless of U.S.-citizenship, were proclaimed and enforced.[32] One year after the attack on Pearl Harbor, all persons of Japanese ancestry who had been living on the West Coast of the U.S. were detained and put into camps behind barbed wires, watched over by armed military police. The exclusion order covered people of Japanese ancestry in the West Coast states of Washington, Oregon, and California, plus the southern third of the Southwest state of Arizona. Two thirds of the incarcerated persons were *American* Japanese, US citizens of Japanese ancestry. One of them was the advanced student James Minoru Sakoda, then aged 25.

The first Japanese-*specific* security measures included curfews, special restrictions on traveling by train, bus, plane, or vessel, exclusion from military zones and other critical areas like electric plants or airports. They were required to stay at home between 9 pm and 6 am. In between they had to be found at home, at work, moving from one of those places to the other or at a location not further away from their residence than five miles. In the first weeks of 1942, fear of espionage, sabotage, and fifth columnists, evolved and spread under the impression of anti-Japanese campaigns. The upgrowth of anti-Japanese sentiments was supported and reinforced by a very real series of military victories of the Japanese armed forces that conquered e.g. Guam, Hon Kong, Manila, and Singapore. The Military, especially the Western Defense Command, the Department of War, and the Department of Justice were operating in a crisis mode. With regard to the Japanese minority at the Pacific Coast, the main issues were the pros and cons of selective civil control, voluntary or compulsory relocation to places more inside the country, or a summary mass detention. Especially in the Department of Justice, but not only there, were many skeptical about mass internments. Initially, one of the skeptics in the military was just Western Defense Commander General John deWitt. In December 1941, he stressed that "an American citizen, after all, is an American citizen," and argued that a selective civil control would be sufficient. In early January 1942, he characterized mass incarceration as "damned nonsense" [quoted after Stanley, 1992, 182, 193]. However, only few weeks later he requested, ordered, and organized the mass detention.

In terms of numbers, the Japanese minority in the Continental U.S. consisted of about 127,000 persons, i.e. less than one percent of the U.S.-population at that time. Almost all of them, 113,000 persons, lived in the West Coast states of Washington, Oregon, and California. A minor group lived in Alaska. As to relative size, in California, the state with the heaviest concentration, the Japanese minority represented less than two per cent of the population.

On February 19, 1942, President Franklin D. Roosevelt signed Executive Order 9066.[33]

[32] President Roosevelt considered Italian enemy aliens not as a security threat but "a bunch of opera singers" [quoted from Hayashi, 2004, 77]. For the different treatment of Germans and Italians on the one side, and Japanese on the other side cf. [tenBroek et al., 1954, 112f., 120].

[33] Cf. the detailed description in [CWRIC, 1982, ch. 2]; a shorter description is given in [Niiya,

It authorized the military leaders to define areas from which "any or all persons may be excluded as deemed necessary or desirable". That authorized General DeWitt to issue a series of exclusion orders, that finally forced all persons of Japanese ancestry that were living at the Pacific Coast, regardless of citizenship, into so called *assembly centers*. What had started moderately with some restrictions, finally turned into incarceration behind barbed wires of all persons with a Japanese lineage—and that because of their Japanese lineage. With a notice of a few days, the internees had to arrive with not more than what a person can carry—after having sold, often at distress prices, most of what they had owned.

How could that happen? What were the driving forces in the escalating process? What was the relative importance and strength of the contributing factors? Why an internment of persons of Japanese ancestry at the Pacific Coast, while nothing comparable happened in Hawai'i where about one third of the population had an Japanese lineage? To date, these questions are debated in a huge (and still growing) body of literature on the mass detention of the West Coast Japanese minority. In all that literature it is taken for granted that the detention stood in a long Pacific Coast tradition of hostile attitudes and corresponding measures against "Orientals", especially Chinese and Japanese immigrants. They were considered as unfair cheap-labor competitors, not trust-worthy ("sneaky"), a "peaceful invasion" (stepwise and silently taking over fishery and agriculture), in short: a "yellow peril" [cf. tenBroek et al., 1954, ch. I, 62f.].[34] In their book *Prejudice, War and the Constitution*, tenBroek et al. describe that tradition in detail in the first two chapters, entitle the first one, "The Anti-Japanese Heritage" [ibid. 11], and start with the section "Race pride and prejudice". A blatant instance was just the case of General deWitt. In his *Final Report: The Japanese Evacuation From the West Coast 1942*, deWitt writes:

> In the war in which we are now engaged racial affinities are not severed by migration. The Japanese race is an enemy race and while many second and third generation Japanese born on United States soil, possessed of United States citizenship, have become "Americanized," the racial strains are undiluted. To conclude otherwise is to expect that children born of white parents on Japanese soil sever all racial affinity and become loyal Japanese subjects, ready to fight and, if necessary, to die for Japan in a war against the nation of their parents. That Japan is allied with Germany and Italy in this struggle is no ground for assuming that any Japanese, barred from assimilation by convention as he is, though born and raised in the United States, will not turn against this nation when the final test of loyalty comes. It, therefore, follows that along the vital Pacific Coast over 112,000 potential enemies, of Japanese extraction, are at large today. There are indications that these are organized and ready for concerted action at a favorable opportunity. The very fact that no sabotage has taken place to date is a disturbing and confirming indication that such action will be taken [deWitt, 1943, 34].

2015a].

[34] For demographic and economic details of the Japanese immigration see [Thomas, 1950].

In his study, *Americans Betrayed: Politics and the Japanese Evacuation*, an early and sharp indictment of the mass imprisonment, Morton Grodzins documented and analyzed another important and, in his view, even decisive factor: certain economic and political West Coast pressure groups and politicians, e.g. the Attorney General of California, Earl Warren.[35] Using all channels, they successfully campaigned against the Japanese minority, and reversed the initially moderate mood of both the public opinion and the Western Commander General de Witt. Along the way, some of them profited financially from the exclusion of their Japanese competitors (especially in agriculture and fishery), while others gained in terms of political influence and stature [cf. Grodzins, 1949, Part I]. More recently, and using recently declassified documents, Brian Masaru Hayashi added as a further factor for the mass incarceration, the "need for hostages" that the US-Government could use "to ensure humane treatment of over twenty-one thousand American servicemen and fourteen thousand civilians in Japanese hands by 1942" [Hayashi, 2004, xiii, 11, 81f.].

By an act of Congress in 1980 the Commission on Wartime Relocation and Internment of Civilians (CWRIC) was established. It was directed to review the facts, circumstances, and consequences of President Roosevelt's Executive Order 9066 and the subsequent military directives requiring relocation and internment [cf. CWRIC, 1982, 1]. After reviewing enormous amounts of documents, hearing testimony of more than 750 internees, former government officials, historians etc., the CWRIC stated in its report that everything that what was done to the American Japanese on the Pacific Coast,

> was done despite the fact that not a single documented act of espionage, sabotage or fifth column activity was committed by an American citizen of Japanese ancestry or by a resident Japanese alien on the West Coast [CWRIC, 1982, 3].

[35] As Grodzins indicates in his preface, Warren later regretted what he had done [cf. Grodzins, 1949, xi]. At the end of his life, Warren (1891–1974) wrote in his memoirs [Warren, 1977, 149]:

> I have since deeply regretted the removal order and my own testimony advocating it, because it was not in keeping with our American concept of freedom and the rights of citizens. Whenever I thought of the innocent little children who were torn from home, school friends, and congenial surroundings, I was conscience-stricken. It was wrong to react so impulsively, without positive evidence of disloyalty, even though we felt we had a good motive in the security of our state. It demonstrates the cruelty of war when fear, get-tough military psychology, propaganda, and racial antagonism combine with one's responsibility for public security to produce such acts. I have always believed that I had no prejudice against the Japanese as such except that directly spawned by Pearl Harbor and its aftermath. As district attorney, I had great respect for people of Japanese ancestry, because during my years in that office they created no law enforcement problems. Although we had a sizable Japanese population, neither the young nor the old violated the law [Warren cited after CWRIC, 1982, 375 fn. 109].

Warren served three terms as governor of California. In 1953 he became a quite *liberal* chief justice of the U.S. Supreme Court. Among other landmark decisions, the Warren Court outlawed racial segregation in public schools and ordered the states to start to desegregate their schools (Brown vs. Board of Education, 1954; Brown II, 1955). Warren headed the governmental commission that investigated into the assassination of President John F. Kennedy. The commission concluded that the murderer, Lee Harvey Oswald, had been acting alone.

Unanimously the CWRIC commissioners concluded:

> In sum, Executive Order 9066 was not justified by military necessity, and the decisions that followed from it—exclusion, detention, the ending of detention and the ending of exclusion—were not founded upon military considerations. The broad historical causes that shaped these decisions were race prejudice, war hysteria and a failure of political leadership. Widespread ignorance about Americans of Japanese descent contributed to a policy conceived in haste and executed in an atmosphere of fear and anger at Japan. A grave personal injustice was done to the American citizens and resident aliens of Japanese ancestry who, without individual review or any probative evidence against them, were excluded, removed and detained by the United States during World War II [CWRIC, 1983, 5].

As a kind of ultra-short summary, the CWRIC commissioners entitled their report *Personal Justice Denied*. An additional task of the Commission's official mandate was to "recommend appropriate remedies" [CWRIC, 1982, 1] for the grave injury. Following the commission's recommendations the US Government apologized officially in the *Civil Liberties Act*, signed into law by President Ronald Reagan in August 1988. To prevent future civil rights violations, the Civil Liberties Public Education Fund was established [cf. Yamato, 2013]. Each surviving victim (about 82,000) got a tax free redress payment of $20,000.

As a victimized citizen, advanced student and young scientist, James Minoru Sakoda was a very awake participant and observer of the events and processes that finally made him eligible for the U.S. Government's redress payment 46 years after the civil rights catastrophe.[36] After the outbreak of war, a sequence of incoherent orders of the Western Defense Commander General deWitt, had caused major confusions among the Japanese minority, including the community of Japanese-American students at Berkeley University where Sakoda was enrolled.[37] One of his fellow students, Charles Kikuchi, describes him as someone who emerged as a leader among the American-born

[36] The Civil Liberties Act was signed by President Reagan on August 10, 1988. That was also the second day of Hansen's interview with Sakoda [cf. Hansen, 1994]. Hansen reports about Sakoda's reaction:

> As it happens I was with him on the very day that President Ronald Reagan signed the Civil Liberties Act of 1988. He was overjoyed; in fact he invited my wife Debbie and I to have dinner with Hattie and him at their Barrington, Rhode Island home on the night of that action, a beautiful meal capped by champagne to toast the action that sanctified the achievement of redress and reparations for survivors of the World War II Japanese American World War II exclusion and detention experience [Arthur A. Hansen in a personal communication to the author on February 13, 2017].

[37] A substantial part of Hansen's article *Political Ideology and Participant Observation: Nisei Social Scientists in the Japanese Evacuation and Resettlement Study, 1942-1945* [especially Hansen, 2001, 128–35] describes in detail Sakoda's growing up, his social network as a student, and the formation of his more general political views. Hansen's article is so far the one and only article that deserves to be called an article *on* Sakoda. Some more information on Sakoda can also be found in Hansen's article *Sergeant Ben Kuroki's Perilous 1944 "Home Mission": Contested Loyalty and Patriotism in the Japanese American Detention Centers* [especially Hansen, 1998, 154–56, 158–60].

Japanese students during the first month of 1942 [cf. Kikuchi, 1989, 187]. Another of his peers says that Sakoda "was less a dreamer ... [and] more of a logician" [Kenny Murase cited after Hansen, 2001, 130]. At that time Sakoda was enrolled in Psychology 145, a course in social psychology taught by Ralph Gundlach, a liberal professor according to Sakoda [cf. Sakoda, 1989b, 221f.].[38] Sakoda wrote a paper for the course with the telling title *As They Await Evacuation*. The paper outlined different types of Japanese Americans and their different initial reactions to the outbreak of war. In retrospect, Sakoda writes in [1989b] that the types,

> were placed on a two-dimensional map with Nisei belonging to two different cultural spheres of influence, Issei on the one side and Caucasians on the other.The vertical dimension presented social class—upper, middle, and lower. The types selected for description were conservative Nisei, maladjusted Kibei, extremely conservative Nisei ("Japanesy" Nisei), rowdy Nisei, elite socialite ("Americanized" Nisei), progressive ("marginal" Nisei), and radical liberal [ibid. 222].[39]

In his retrospect Sakoda does *not* put himself explicitly into one of his categories. He describes his contacts and associations with marginal Nisei, and states their advantages, namely more links to Caucasians with their upwardly mobile careers. At the same time he stresses their predicament, namely "not being accepted fully by either the Japanese or the Caucasian group" [1989b, 221]. Anyhow, by one of his links to marginal Nisei, namely a link to his fellow student Tom Shibutani [cf. Hansen, 1994, 371], who was also enrolled in Psychology 145, Sakoda was introduced to Dorothy Swaine Thomas (1899–1977)—and that had far-reaching consequences.

D. S. Thomas had become a professor of rural sociology at Berkeley in 1940. Right after the outbreak of the war, together with some colleagues she develped, what is now known as the *Japanese American Evacuation and Resettlement Study (JERS)*. She considered the project as "a study in enforced mass migration" [quoted after Ichioka, 1989b, 4]. The study would have important policy implications for "post-war enforced mass migrations in Europe which would be necessary to rectify the population imbalances caused by the war" [ibid. 5]. From the Rockefeller, the Columbia, the Gianni Foundation and some minor other sources she obtained a total of about $100,000, "a substantial amount of money by any standard, past or present" [ibid. 6]. In 2015

[38] Gundlach (1902–1978) was at that time an associate professor at Washington University. During a leave of absence he taught at Berkeley 1942–1944. When 1948 the Interim Committee on Un-American Activities (better known as the Canwell Committee) started to work, Gundlach was among the subpoenaed faculty members that were suspected to be members or sympathizers of the communist party. In the hearing Gundlach did not answer questions about his political affiliations. In 1949 the Board of Regents of Washington University fired Gundlach because of neglect of duty (together with two other professors). Gundlach's attempts to get reinstated failed. Later he became a private psychotherapist in New York City. In 1973 he retired to Great Britain and died in London 1978. Archive material and a biographical note on Gundlach can be found at `http://archiveswest.orbiscascade.org/ark:/80444/xv11935/op=fstyle.aspx?t=k&q=Ralph+Gundlach`.

[39] A note about Sakoda's terminology: *Nisei* are the members of the first generation of ethnic Japanese born in the U.S. and, therefore, U.S. citizens. *Issei* are the members of the immigrant generation. *Caucasians* are white Americans in general.

consumer prices it corresponds to a sum of about $1,450,000; as a relative share of the GDP it corresponds to about $10,500,000.[40]

The JERS staff members were recruited from the Berkeley campus. One of them was Sakoda, hired as a participant observer of the mass incarceration. In his retrospect *"JERS revisited"*, Ichioka reports that "all staff members had to pledge not to disclose, either through publications or public lectures, any findings of the research project until the end of World War II" [Ichioka, 1989b, 8]. As an involuntary participant and paid observer, Sakoda (together with two sisters and a brother) was imprisoned in the Tulare *Assembly Center*, where he stayed a month (cf. [Sakoda, 1949, 6], [Sakoda, 1989b, 221f.]). From there he was sent to the center at Tule Lake and remained there from June 1942 to September 1943. After massive conflicts (protests, strikes, violent resistance) that camp became a special *segregation center* for families that were classified "disloyal" to the US. Since Sakoda was classified "loyal" [cf. Sakoda, 1989b, 228f.], he had to leave, and was sent to the Minidoka *relocation center*. There he stayed from September 1943 until March 1945. Additionally, and still as a JERS research assistant, he observed Minidoka's closing in late 1945. Sakoda's doctoral dissertation is based upon his extensive observations in these "centers", especially Minidoka.

At this point, we have to pause to reflect upon problems of accurate terminology. Obviously the government and their civil or military agencies systematically used euphemisms of all sorts to obscure, shade, and sugarcoat their actions. "Evacuation", for instance, sounds like rescuing, but, as a matter of fact, it was the incarceration of the West Coast Japanese American citizens for racial reasons. CWRIC, the Commission On Wartime Relocation and Internment of Civilians, already had doubts about using the "government speak". But "to avoid ... confusion and controversy," the commission "largely left the words and phrases as they were" [CWRIC, 1983, viii]. In the next sentence, the commissioners,

> leave it to each reader to decide for himself how far the language of the period confirms an observation of George Orwell: "In our time, political speech and writing are largely the defense of the indefensible. . . . Thus political language has to consist largely of euphemism, question-begging and sheer cloudy vagueness" [ibid.].

In 1949, in his book *Americans Betrayed*, Grodzins referred to what the government had called "evacuation centers" as "concentration centers" [1949, 2]. For sure, he knew to what he was alluding to. Later publications on the incarcerations made it explicit: Allan Bosworth titled his book *America's Concentration Camps* [1967], a book of Roger Daniels is titled *Concentration Camps, USA: Japanese Americans and World War II* [1972]. There was and still is a debate and conflict about an appropriate terminology [cf. Daniels, 2005]. It is out the of question that the "concentration camps USA" were *not* like Nazi death camps with their organized extinction and mass murder in gas chambers—in a very literal sense, a vitally important difference. But the

[40]The values are the results of a calculator at `https://www.measuringworth.com/uscompare/index.php`, [Williamson, 2017].

question is, whether or not, gas chambers and mass murder are constitutive meaning components of the term "concentration camp". Hayashi writes in his preface to *Democratizing the Enemy*:

> "Concentration camp" is a generic term that includes all such sites, a stance consistent with findings by Holocaust scholars. They define such sites as "camps in which persons are imprisoned without regard to the accepted norms of arrest and detention". "Concentration camp" is therefore an accurate term since Japanese Americans were forcibly removed and detained well beyond "the accepted norms" [Hayashi, 2004, xivf.].[41]

Additionally, it is worth noting, that since the very beginning of the incarceration at least the upper hierarchy of the US government *internally* often referred to the sites as "concentration camps". At the same time, they decided, officially not to use such blunt language. But there was an exception: In the public, President Roosevelt referred to the sites at several occasions as "concentration camps" [cf. Daniels, 2005].

More and more, in scholarly and public discourse, "concentration camp" is used to refer to the former assembly, evacuation, relocation, resettlement, or segregation centers. In 1998, an exhibit with the provocative title *America's Concentration Camps: Remembering the Japanese–American Experience* (organized by the Japanese American National Museum in Los Angeles) caused a major public debate about the appropriate use of the term "concentration camp". In an editorial titled *Words for Suffering*, the *New York Times* supported the use of the term to describe the mass incarceration of West Coast Japanese Americans, arguing that "it does no service to the memory of the victims of Nazi genocide to distort an ugly truth about American history".[42]

To resort to an internment terminology (internees, internment camps etc.) is problematic as well.[43] Usually "internment" regards the treatment of prisoners of war and civil alien enemy nationals. It is defined and regulated by the Geneva Convention and recognized in American and international law. The Japanese American citizens were neither combatants nor alien enemy nationals, and, therefore, not "eligible" for internment.[44]

In the text above I often used the official terminology of the government. In what follows (it is relevant especially in the later section 3.3) I will try to avoid that, but use that terminology where it is necessary to avoid confusion. Instead of evacuation

[41] However, Hayashi continues, that in his book the term is used less frequently "because some still mistakenly assume that it applies only to the Nazi death camps" [Hayashi, 2004, xv]. Without any reservations, the book jacket speaks of "US concentration camps".

[42] See the *NYT* editorial from March 10, 1998, at `http://www.nytimes.com/1998/03/10/opinion/words-for-suffering.html`.

[43] Note that the term "internment" was part of the official name of CWRIC, the *Commission on Wartime Relocation and Internment of Civilians*.

[44] For a discussion of the term "internment" see the opening section of [Daniels, 2005]. Daniels writes that it is difficult to find precise numbers about internments in the sense of the Geneva Convention. His guess is that about 11,000 persons (Germans, Italians, and Japanese) became internees. But they were *not* American citizens as most of the incarcerated West Coast Japanese were.

Figure 13: Picture TULE LAKE SEGREGATION CENTER in *The Spoilage* [Thomas and Nishimoto, 1946, between p. 42 and p. 43]. For the inside front page of the book cf. figure 15.

centers etc. I will speak of camps, but not of "concentration camps".[45] Correspondingly, instead of referring to evacuees, I will refer to inmates, detainees, imprisoned or incarcerated persons.[46]

Now back to Sakoda who, in his dissertation and later publications, always and consistently used the official terminology of the government. Late in his life, in two *very last* scientific publications, Sakoda looked back on his early research as a field worker behind barbed wires—involuntarily participating in what he observed. Both articles were contributions for the conference, *Views From Within: The Japanese-American Wartime Internment Experience*, organized by Yuci Ichioka, and held 1987 at the University of California at Berkeley.[47] The conference had two parts. Part one discussed constitutional issues; part two was devoted to a reassessment of JERS, the Japanese-American Evacuation and Resettlement Study.[48]

[45] My reasons for not using the term "concentration camp" are related to Hayashi's reasons as indicated in footnote 41.

[46] For a short and summary discussion of the terminology problem see the initial section *A Note on Terminology* in [Robinson, 2010, vii f.]. My terminology is basically the same as Robinson's.

[47] In his introduction to *View From Within*, Ichioka notices Sakoda's "unquestioning usage of WRA terminology" in his two contributions to the book—a book that appeared in the late 1980s [Ichioka, 1989b, 20].

[48] Ichioka, the editor of *Views From Within*, states in his preface that he did not insist upon a common

In his first contribution, titled *Reminiscences of a Participant Observer*, Sakoda describes difficulties and dangers [cf. 1989a, 224f.].[49] An atmosphere of suspicion prevailed in the camps. Based upon their experiences with FBI and intelligence officers, the imprisoned lived in fear of informers.[50] Using a notebook, taking notes would have caused alarm in a major proportion of internees.[51] Using questionnaires was out of question. Sakoda could not openly announce that he was an JERS research assistant. In short: subterfuge was necessary.[52] Only to some friends he revealed his role as a JERS field worker. As all other field workers, Sakoda made diary notations, wrote entries in a journal, produced reports, i.e. "more formally written accounts, collating material from different sources to present a more coherent picture than was possible in the diary or journal" [Sakoda, 1989a, 231ff.], and he wrote letters to D. S. Thomas [cf. Sakoda, 1949, 9f.]. All in all, Sakoda wrote about 1,800 report pages [cf. Sakoda, 1989a, 232].[53] In his dual role, Sakoda participated in the daily life of the camp (attending block meetings, working as a psychology teacher, interviewer, being a representative of a consumer's co-operative from his block, participating in social activities etc. [cf. Sakoda, 1949, 6]; at the same time, as a paid observer and research assistant, he collected data for JERS, that then, later, D. S. Thomas allowed him to use for his dissertation thesis [cf. ibid. ii].[54]

language, and then continues to say "while I, following my own personal preference, employ 'American-style concentration camps' and other alternative terminology" [Ichioka, 1989b, ix].

49 In Hansen's interview with Sakoda that is described and discussed in great detail especially on the second day of the interview [cf. Hansen, 1994, 385ff. et passim].

50 Their fears were not without substance. The *War Relocation Authority (WRA)*, the government agency that ran and controlled the camps, tried to get access to all data that the JERS field workers gathered. In a letter to R.S. Nishimoto, D. S. Thomas writes, that an WRA official,

> demanded that I give him carbon copies of everything that our staff was sending in from the projects. I explained the total impossibility of carrying on the study under those conditions and predicted that my workers would resign rather than submit to this policy. He said I wouldn't have to tell them that their reports were being submitted to WRA! I controlled my temper with difficulty and explained mildly that this was a cooperative study with no secrets from the staff members. Naturally, I sent in no documents and nothing happened for a couple of months [cited after Ichioka, 1989b, 14].

Finally, on February 10, 1943, Thomas signed an agreement with the Principal WRA Social Analyst, John F. Embree. (Note: The WRA had its own social scientists). Thomas did not provide copies of JERS material. But she agreed to write a monthly letter about significant findings, agreed to consultations about field data, and allowed her fieldworker "to informally cooperate with any social analysis that may be undertaken by WRA" [Ichioka, 1989b, 15].

51 Sakoda reports an episode when he was very close to being beaten by other inmates [cf. Sakoda, 1989b, 228f.].

52 Because of this subterfuge policy the JERS was later criticized as bordering to fraud. Suzuki writes that the study "was conducted as a covered operation and its subjects were kept in the dark about its goals and activities" [1989, 111].

53 In Sakoda's *Reminiscences of a Participant Observer* [1989b] several diary notes and journal entries are reprinted.

54 Based upon archival documents of all sorts, Hansen describes and analyses in detail Sakoda's dual role [cf. Hansen, 2001]. Utilizing Sakoda's diary, Hansen writes about Sakoda:

> We discover that his nonprofessional reading consisted of proletarian novels like Steinbeck's *In Dubious Battle* and Dos Passos's *USA* and social reform nonfiction literature like Louis Adamic's *From Many Lands* and Carey McWilliams's *Brothers Under the Skin*. We

3.2 Sakoda's 49-Model: The Very First Checkerboard Model of Social Interaction

Different from what one might expect from someone who had collected the data for his dissertation thesis in three years of involuntary imprisonment, Sakoda's thesis is written free of any bitterness—there is none on the lines, nor in between. Starting point is the diagnosis of a standard deficit in the analysis of interactional processes: On the one hand, their is a lot of armchair theorizing. On the other hand, there are all the descriptive studies without any theoretical framework. But what is necessary "is the application of one to the other" [Sakoda, 1949, 3].[55] That given, the title of the dissertation becomes self-explanatory: the changing interactional processes in Minidoka, the relocation center in Southern Idaho, in which Sakoda spent quite a time as a Japanese American detainee, are taken as a case study to demonstrate that data and theory can find each other, thereby giving a comparatively deep understanding of a complex dynamics. Summarizing the thesis, there at least *five* telling features:

1. The thesis focuses on "the problem of a small group of administrators with superior power and one set of values attempting to extend influence over a large group of people with a different set of values" [3]. Sakoda hopes that the analysis is illuminating for similar *asymmetric intergroup relationships* as, for instance, teacher-pupil or employer-employee relations.

2. Using a now very fashionable, but at that time not very common "complexity jargon", Sakoda describes his project as the analysis and understanding of an ongoing "complex social interactional process" [26]. Sakoda frequently resorts to the words *complex* or *complexity* in describing the structure of the processes he tries to understand.[56] Again and again Sakoda stresses that the challenging task is the understanding of *processes*—and that it "can be placed on a scientific basis" [390].[57]

3. *Participant-observation* over years and in several camps, is the source for the empirical basis of the thesis. Carefully and in detail Sakoda reflects the advantages and disadvantages of the method under the condition of detention [cf. 4–14, 380ff.].

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learn, too, that this eligible bachelor ruminated about the sort of woman that he wanted to marry—that she be more of a "companion," and not "too middle class and conventional." We are also told that he was bothered by the "bootlicking" political style of JACL camp leaders who, "played the role expected by the Caucasian group with the assumption that this would achieve the greatest amount of rights for the Japanese people." Further, we find that he believed social work harmful, "because only enough of it was done to keep the present socio-economic setup, and kept people from revising the whole system, which was really at fault," and that Nisei, if they wanted to advance, "should join labor unions" [Hansen, 2001, 131].

JACL is the *J*apanese *A*merican *C*itizens *L*eague, founded in 1929. The JACL leaders had *not* questioned the constitutionality of the exclusion of Japanese Americans from the West Coast.

[55] If not explicitly stated otherwise, all page numbers in this subsection regard Sakoda's dissertation.

[56] Cf., for instance, the very first sentence of the summary, and the pages ii, 28, 32, 49, 364.

[57] This quotation is from the *last sentence* of the thesis. After this the appendices start.

4. The thesis is strongly *theory-oriented* and driven by a “spirit of systematization” which explains the particular as an instance of general mechanisms. Sakoda formulates what he calls a “conceptual scheme" [66, cf. 32–65], consisting of 9 principles of human behaviour and 15 theorems related to social interactions, the latter informally, though not strictly, deduced from the former [cf. 31]), using a terminology explained explicitly in *Appendix A* [391–416]. Methodologically, the conceptual scheme was developed in a process of “successive approximation" [26, 32]. In that process, theorems and principles were reformulated, dropped, or introduced to *match* the data—Sakoda compares it to curve fitting [cf. 74].[58] In line with the at that time prominent approaches of Kurt Lewin and William Isaac Thomas, the principles and derived theorems regard especially the ways in which individuals (re)define situations or react to tensions. To give an example, the *Tension-Realism Theorem* states: “The greater the tension the greater the possibility of unrealistic definition of the situation” [50].

5. The main part of the thesis [ch. IV–X] analyses in chronological order the sequence of predominant patterns of behaviour in different phases of Minidoka. For the most part, in that analysis, the actors are *not* single individuals rather *groups*: administrators, evacuee leaders, and evacuee residents—all of them with characteristic attitudes towards each other that change over time. In each chapter Sakoda tries to demonstrate how the theorems apply “to explain the nature of the social interactional process” [76].

Sakoda’s 49-model is mentioned for the first time in the next to last section *Some Next Steps* [384ff.]. In the subsection *Conceptual Model* [388ff.], and as the last of such next steps, Sakoda—a bit in a tentative mood—anticipates fundamental ideas of agent based modeling. I quote the whole passage:

> Another worthwhile task is probably the development of a conceptual model to study the process of social interaction. As we have stated before, one of the difficulties in social psychology is the habit of thinking in terms of entities, such as individual, group and culture, and the difficulty of visualizing participants in interaction. We are in need of a model which will help us to visualize and analyse the interactional process. The physical scientists have employed models to advantage in the development of hypotheses, and there is every reason to believe that a good conceptual model can be of immense help to the social scientist. One advantage of working with a model is that it forces the investigator to think more rigidly about the definition of variables and the relationship among them. Lewin’s topological diagrams are useful in analysing the psychological field of a single participant, but they are difficult to apply to an interactional situation which involves a separate field for different participants. Moreno’s

[58] From a Popperian point of view, that sounds very much *ad hoc*—at least at first glance. But, presumably, one could describe Sakoda’s procedure as well as a *repeated process* of inventing and refuting hypotheses, i.e. as a procedure very much Popperian in spirit. We could also look at Sakoda’s procedure as similar to Rawls’ reflective equilibrium.

> sociograms give a better notion of the relationships among participants in a social situation, but does not provide the necessary dynamics for the analysis of continuous interaction.Von Neumann and Morgenstern's analysis of games in *Theory of Games and Economic Behavior* as a basis for studying human economic behaviour suggests the possibility of developing games with the attributes of human social behaviour. During the course of this analysis the writer experimented with a model employing checkers and a checkerboard. A brief description of some of the simpler formulations is given in Appendix B to indicate the general direction in which such an undertaking can lead [ibid. 388f.].

Visualization of interaction processes, driven by explicitly stated relations between well defined variables—that is what, according to Sakoda, developing a conceptual model amounts to. Lewin's and Moreno's diagrams visualize, but miss interaction or dynamics. However, von Neumann and Morgenstern[59] induce an idea: *games*, more exactly, checkerboard games "with the attributes of human social behaviour". What the latter means, becomes clear in Sakoda's *Appendix B*, from which we reproduce his demonstration [418] given by figure 14. The decisive features are:

1. "A six by six checkerboard was employed to represent a circumscribed social situation within which all participants must remain. This might represent a room, a public conveyance, an apartment house, a city block, a small town" [417].
2. Six checkers in red and black colours are randomly distributed on the board. Each checker has,

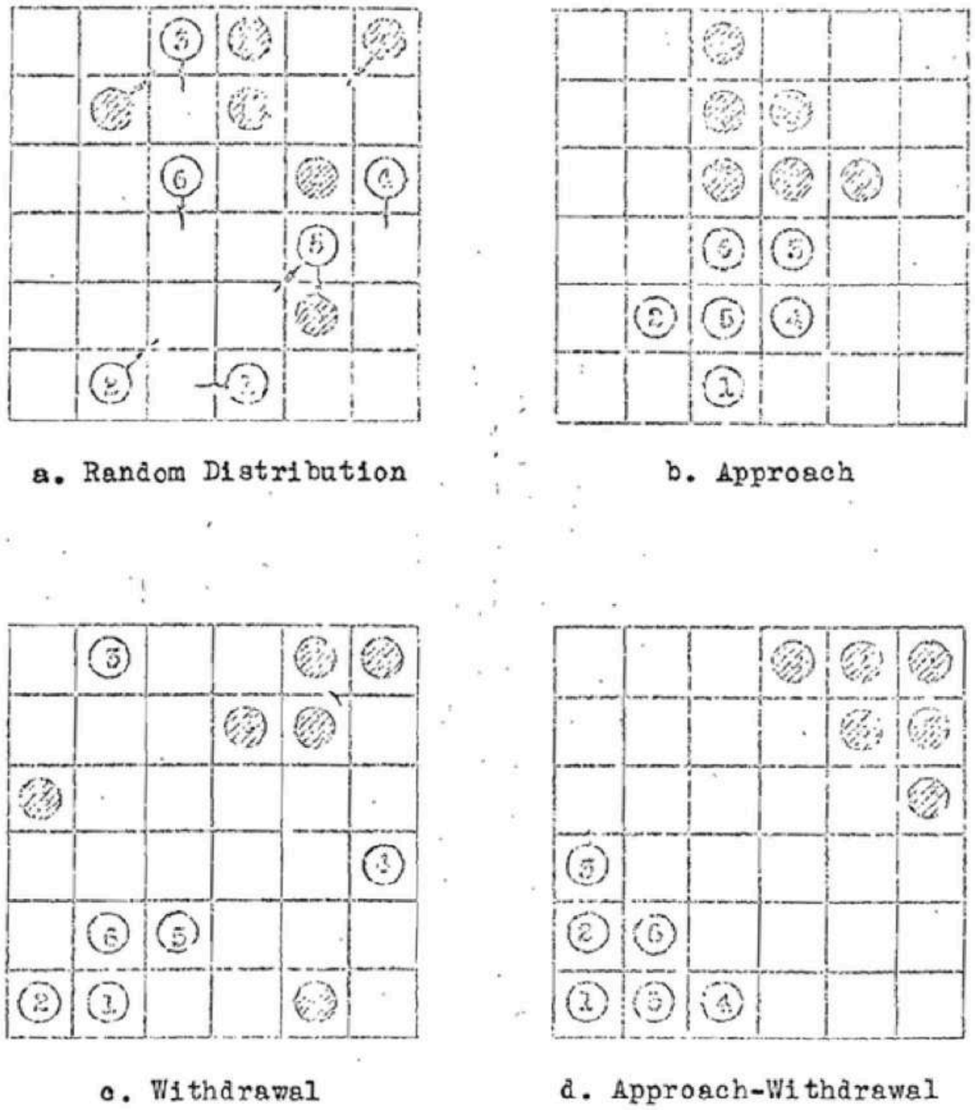
   > two kinds of moves. One is approach to a nearby participant of the same colour, which can represent the result of positive evaluation. The second is withdrawal from participants of the other colour, as a result of negative evaluation. . . . Each checker can move only one square at a time. The checkers are numbered, and they make their moves in this order, beginning with all of the reds, and each group alternating. When an advantage cannot be gained by making a move, no move is made. The game is ended when all participants cease to move, or when a state of equilibrium results in no further change in the pattern of relationships [417f.].

3. The whole interactional process is *decentralized*—"no communication, organization, or leadership is involved" [417].

Figure 14 shows in (a) a random start distribution; (b) is a the final configuration of a process in which *only* approach moves were allowed; in (c) *only* withdrawal moves were possible, and then, (d) shows the resulting structure of *alternating* approach and withdrawal moves. Starting with (d), Sakoda interprets the results as follows:

[59] Their seminal book is *Theory of Games and Economic Behavior* [von Neumann and Morgenstern, 1944]. Sakoda refers in his dissertation to the second edition from 1947.

Using a Checkerboard Conceptual Model.



In Fig. a is shown the random distribution of six participants each of two different groups, with arrows indicating the first series of approach moves. In Fig. b is shown the end result of the approach series, in which each participant approaches the closest member of its own group. In Fig. c is shown the end result of the withdrawal series, in which each participant withdraws from members of the opposing group. In Fig. d is shown the end result of the approach-withdrawal series, in which participants alternately approach the closest member of its own group and withdraws from members of the opposing group.

Figure 14: Sakoda's Checkerboard Conceptual Model from 1949 with the original caption [Sakoda, 1949, 418].

> Here we find the clustering together of the members of a group and a considerable gap between groups. This is the situation in in-group out-group relationships, which involves not only withdrawal, but withdrawal into one's own group. The formation of factions, the creation of ghettos, the organization of delinquent gangs can be considered to be the result of both withdrawal from an opposing group and approach toward their own group. From a comparison with situations in which only one of the principles was at work we can predict that such in-group out-group relationship is the result of both factors operating simultaneously.
>
> These are the simpler moves representing social interaction in its crudest form. It is probably possible to introduce the nonconforming individual, organization under a leader, difference in power and skill between groups, communication, resistance to change, etc. as additional variables to depict more complicated social interactional situations. Such a model helps to visualize a group as a system of interaction of participants with definite definitions of the social situation. By use of such a model it is possible to develop hypotheses which can be tested in experimental or life situations [420f.].

In his *JMS* article from 1971, Sakoda mentions the 49-model and gives a half a page description of how it worked and why it was developed [Sakoda, 1971, 120f.]. In two later publications [1978] and [1989a], Sakoda provides some more systematic and historical details about his 49-model.[60] The latter is the second of his two contributions to *Views From Within: The Japanese American Evacuation and Resettlement Study* [Ichioka, 1989a]. Sakoda attaches a three-page appendix, entitled *Checkerboard Model of Social Interaction*, that summarizes the 49-model and shows on one page a graphically improved version of the four pictures in figure 14.[61]

Taking all descriptions of the 49-model together, makes it very clear, that the movement of checkers in the early and in the later version of the checkerboard model works differently. In the 49-model there are two types of moves, approach and withdrawal. The former is motivated by a positive, the latter by a negative attitude. Rules are set up "for moving pieces in accordance with these attitudes" [Sakoda, 1978, 360]. The thesis says that the checkers can move "only one square at a time" [Sakoda, 1949, 417]. The numbers in the circles of figure 14 indicate the sequence in which the checkers move. The top left picture in the figure shows the random start distribution. The small arrows that are attached to the circles in that picture, indicate the direction of the checker's first move. Some of them have a diagonal direction. Obviously, "one square at a time" includes the adjacent diagonal cells. Thus, the window of allowed migrations is what nowadays is called a 3×3 Moore neighborhood. But what are the rules of movement? In the dissertation, Sakoda says about the top-right picture in

[60] Cf. [Sakoda, 1978, 359–61], [Sakoda, 1989a, 282–84].

[61] Sakoda modified two captions of figure 14: Bottom right "Approach-Withdrawal" is changed into "Segregation"; top right "Approach" is changed to "Crossroads" [Sakoda, 1989a, 283]. Obviously, in 1989, Sakoda preferred to refer to the two interaction processes of the 49-model by the names of the attitude matrices in the corresponding instances of the 71-model. Additionally, the arrows in the picture top left are omitted (the arrows indicate the direction of the first move of the checkers).

figure 14 that it "shows the results of the approach series, in which participants only approach others of their group closest to them" [Sakoda, 1949, 419]. No metric is given that specifies how to measure distances in order to determine who actually is closest. The descriptions suggest that the checkers move *in the direction* of a closest member of their favorite group. About withdrawal moves it is only said, that "participants move away from those of different color" [ibid.]. We find some more details about moving of checkers in the two later articles from 1978 and 1989. There Sakoda writes about the 49-model:

> Moves were made by considering the closest piece alone, unless the best position to move toward or away from a piece could not be determined by considering the closest piece alone. The next closest piece was then brought into consideration in order to arrive at a best move [Sakoda, 1978, 361].

> Checkers were ... allowed to move one step in accordance with its attitude, moving to a position which was most in line with its attitude—toward friends and away from enemies. Checkers which were closest were given the greatest weight in determining a move [Sakoda, 1989a, 282].

Again, there is no metric. Nor are there the details about *how* to consider next closest pieces.

In sum, the migration regime is clear for approach moves insofar as there is a unique best move (or, trivially, no possibility for movement at all). Such a move is done in the direction of a closest member of a checker's favorite group. If there is no such unique approach move, a next closest group member is considered. Whatever "consideration" here means, the result is again a direction of movement (one square a time). Probably there is some intuitive and informal aggregating, and, additionally, some intuitively applied randomness. For withdrawal moves the migration regime should be somehow analogous, though "inverted".

The problem of aggregating and balancing becomes even more severe, if positive *and* negative attitudes are working *at the same time*—as it is assumed in the case figure 14, bottom right ("Approach-Withdrawal"), where members of both groups have a positive attitude to members of their own group, but a negative one to the other group and their members (like in the attitude matrix called *segregation* for the 71-model[62]). With a move in a certain direction, then, the numbers of close-by friends and enemies, and the distances to all of them normally change simultaneously. Naturally one would expect that, under such conditions, the checkers in the model do some aggregation and overall evaluation to determine a move "in accordance with their attitudes". But that is *not* what the 49-model does. An overall evaluation is side-stepped by a trick: the simultaneous existence of positive and negative attitudes is "implemented" by simply *alternating* the two types of moves between one cycle approach, the next cycle withdrawal.[63] What, then, are the main differences between Sakoda's manually operated

[62] Cf. footnote 61.

[63] Cf. [Sakoda, 1949, 418], [Sakoda, 1971, 121], [Sakoda, 1978, 361].

49- and his computerized 71-model? In the 71-model, first, the whole world with *all* its checkers matter—whether close-by or far away. But depending upon their distance, they matter to different degrees: The further away, the less they matter. Different from the 49-model, Sakoda's 71-model has a well defined metric to measure distances. The *Euclidean distance* is used to determine the relative distance of a checker j to a checker i (cf. equation (1) in section 2.2).[64] Then, based upon that metric, all decisions of all checkers are all the time based upon overall evaluations according equation (2), i.e. a summing up of values that represent a checker's attitudes towards checkers, weighted by a (non-linear, monotonically decreasing) function of the Euclidean distance to these checkers. Moving decisions are generally made by applying a *maximization principle* to a certain well defined set of empty close-by squares whose overall attractiveness was calculated beforehand according to equation (2). Special rules for approach and withdrawal do not exist any longer.

The rules of Sakoda's 49-model were much more *local*, and, therefore, easy to apply (at least if some intuition in the determination of moves was allowed). It was no problem to run the 49-model as a manual table top exercise. But, nevertheless, there is a lack of explicitness and precision. Additionally, it is not coherent to argue in terms of fields and attitudes, and then to build into the model independent positive and negative moves that *alternate* cycle by cycle. If certain fields and attitudes are "at work", then they should be at work all the time—not only every second cycle. Sakoda's 71-model comes with an elegant, simple, and theoretically coherent solution for all these problems. However, and despite of the differences, both Sakoda's 49- and his 71-model are of the same basic structure and spirit: grid based, multi agent based, process oriented, visualization oriented, generating macro effects by decentralized choices of agents on a micro level.

3.3 The Missing Volume *The Residue*: Sakoda's Dissertation Thesis and the JERS Publications

Sakoda's dissertation was never published. Under certain conditions—conditions that Sakoda considered unacceptable—a modified version of his dissertation could have been published by the University of California Press as the third of three "official" JERS volumes. The title of the book would then have been *The Residue*. But the volume did not materialize.

JERS generated enormous amounts of data, reports, life histories, letters, diary journals, and other documents. But in terms of publications, articles or books, the outcome was poor (as all involved persons agree). A whole variety of causes and reasons contributed to that effect. There were serious problems already when the first plans for JERS were drawn up in the first weeks of the war.[65] In September 1952, when D. S. Thomas became the first female president of the American Sociological Association

[64] There are alternatives to the Euclidean distance. Other metrics that would work on a checkerboard are, first, the so called *Manhattan or City Block metric* and, second, the *chessboard distance*. Different from the Euclidean metric, the distances in the other two metrics are integer values.

[65] In 1952 D. S. Thomas reports that the first plans for the study were made by her together with Charles Aiken in February 1942 [cf. Thomas, 1952a, 666]. In the preface of the first book with results

(ASA), she described frankly some of the problems in her presidential address [cf. Thomas, 1952a, 666f.]: The project became bigger and bigger as the so called evacuation measures extended piecemeal to ever more persons and areas. To cope with the increasing size and complexity of the research project, senior members of other faculties were added.[66] But soon *all other* senior researcher were drawn into other war related work—D. S. Thomas was left alone with a project one order of magnitude bigger than originally conceived. The project, as she frankly admits,

> extended far beyond the range of my experience, could draw upon no systematically accumulated fund of knowledge, and found few realistic "models" or adequate techniques by which to guide procedures or check conclusions [1952a, 665].

D. S. Thomas was not completely alone. There was her husband, the retired sociologist William Isaac Thomas (1863–1947). In his *Reminiscences of a Participating Observer*, Sakoda points to his influence:

> Dorothy S. Thomas was a specialist in demography and most at home with statistical data. Her retired husband, W. I. Thomas, would have been the more likely person to study the evacuation and resettlement, since the situation had many similarities to his well-known classic in sociology, *The Polish Peasant in Europa and America*. While his wife was clearly in charge of JERS, W. I. Thomas' influence was extensive. Frank Miyamoto, Tamotsu Shibutani and I were greatly influenced by his theoretical orientation. Thus, the use of personal document, best exemplified by life histories collected by Charles Kikuchi, and use of participant observation as a primary method of investigation show the general approach favored by W. I. Thomas [Sakoda, 1989b, 219].

Sakoda was not the only JERS staff member to realize the enormous influence of William Isaac Thomas.[67]

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of the study we read that other senior scientists that participated in the early stages of planning were Robert H. Lowie (Anthropology), Charles Aiken (Political Science), Milton Chernin (Social Welfare), and Frank L. Kidner (Economics) [cf. Thomas and Nishimoto, 1946, vi fn. 1].

[66] Cf. the report about D. S. Thomas' role in JERS in [Miyamoto, 1989b, 37ff.].

[67] Cf. [Miyamoto, 1989b, 36]. R. F. Spencer writes:

> Throughout the years of research, W. I. Thomas remained in the background, not contributing directly perhaps, but making his theoretical position felt both by his wife as well as by her assistants [Spencer, 1989, 159].

In the preface to the first of the two later volumes with JERS results, D. S. Thomas and R. S. Nishimoto state,

> that the authors ... are incalculably indebted to W. I. Thomas who has read and critized the whole manuscript and made many suggestions for revisions. Our greatest hope is that his influence will be apparent [Thomas and Nishimoto, 1946, xv].

The second volume explicitly reaffirms in general all indebtednesses and mentions W. I. Thomas in a footnote that says that "his views greatly influenced our standpoint" [Thomas, 1952b, 136 fn., cf. vii]. Later, in 1970, D. S. Thomas recalls:

W. I. Thomas' theoretical orientation is explicitly described in the long *Methodological Note* at the beginning of *The Polish Peasant in Europe and America*. This is a monumental study that he published together with the polish sociologist Florian Znaniecki between 1918–1920 [Thomas and Znaniecki, 1918–1920, 1–86]. It pioneered field research and the use of biographical material. The study became famous immediately. It is considered a sociological classic. The momentous theoretical orientation therein is often called the "situational approach": Acting is the solution of a situation. Every situation has two decisive components. There is the *objective* situation with its totality of conditions. In addition, and somehow as a part of that totality, there is the *subjective* conception of that situation—in W. I. Thomas' words, "the definition of the situation"—selected by an individual or a group [ibid. 68]. The existence of a subjective component constitutes a decisive difference between the social and the physical sciences: Atoms do not interpret their own situation [ibid. 38]. Both the subjective and the objective component are scientifically accessible. Personal documents are good starting points on the way and that finally should lead to the goal of science: verifiable generalizations that can be used for the rational control of behavior.[68]

In her presidential address D. S. Thomas describes how their collaboration had started in the middle of the 1920s, when W. I. Thomas offered her a job as a statistician on one of his projects, an offer that she "eagerly accepted" [Thomas, 1952a, 664]. The background of the job offer was that W. I. Thomas had realized that his approach of starting with personal documents of all sorts should then, at a next stage of analysis, be continued by serious statistics. For doing that in practice he was in need of assistance. D. S. Thomas was the perfect person for that task. The first outcome of their collaboration was the book *The Child in America* [Thomas and Thomas, 1928]. In that book is stated a kind of corollary of the situational approach that became famous:

> If men define situations as real, they are real in their consequences [Thomas and Thomas, 1928, 572].

A decade later—more in passing—Robert K. Merton (1910–2003), referred to it as "W. I. Thomas' sociological theorem" [1938, 331]. After another decade, the genitive disappears in favor of an eponymy: in the second sentence of his *The Self-Fulfilling Prophecy*, Merton refers to the theorem as "the Thomas theorem" [1948, 193].[69] This

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> I shudder to think of the idiocies I might have perpetrated by way of "premature quantification" of the essentially "unquantifiable" had I not been associated with W. I. Thomas at the time. In our preparation and interpretation of behavioral data, W. I. Thomas was our counselor and guide [D. S. Thomas, cited after Murray, 1991, 132].

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[68] For a concise summary of W. I. Thomas' general views cf. [Volkart, 1951a, 2].

[69] It sounds like a justification for the upgrading from a genitive to an eponymy, when Merton writes:

> Were the Thomas theorem and its implications more widely known more men would understand more of the workings of our society. Though it lacks the sweep and precision of a Newtonian theorem, it possesses the same gift of relevance, being instructively applicable to many, if indeed not most, social processes [1948, 193].

Esser distinguishes six different readings of the Thomas theorem [cf. Esser, 1999, 170–75].

eponymy—being one of the highest forms of scientific recognition—gained acceptance, and is still established. But Merton, the pioneer and central figure of the sociology of science in the 20th century had ascribed the theorem (like many others before and after) to W. I. Thomas alone—despite of the fact that the theorem was clearly published in a book that was authored by both, W. I. Thomas *and* D. S. Thomas. In the 1980s, this caused a debate about what was called "institutionalized sexism". But at the end of a very long article on the history of "the Thomas theorem", Merton reprints a personal letter of D. S. Thomas, that Merton regarded as "the archival smoking gun" [Merton, 1995, 401]—and indeed it is. On September 10, 1973, D. S. Thomas writes to Merton:

> In regard to *The Child in America* W. I. Thomas employed me as an assistant since he had been told by the Rockefeller group to get himself a statistian. The statistical portions were mine and I am sending you under seperate cover Volkart's book which makes this clear. The concept of "defining the situation" was strictly W. I.'s. [D. S. Thomas in a letter reprinted in Merton, 1995, 401].

After *The Child in America*, W. I. Thomas and D. S. Thomas continued to collaborate on a project on the Swedish emigration and Swedish immigrants, a project similar to *The Polish Peasant*.

Contrary to what their second names suggest, D. S. Thomas and W. I. Thomas were *not* a married couple when they published *The Child in America* in 1928. D. S. Thomas was simply a *born* Thomas.[70] But on February 7, 1935, the two became a couple—in terms of age, a very unequal one: D. S. Thomas was 36 years old, W. I. Thomas was aged 72. At that time, he was a well known sociologist with a well established reputation, influential with his "situational approach". In the first sentence of his *The Self-Fulfilling Prophecy*, Merton calls him "the dean of American sociologists" [1948, 193]. W. I. Thomas had received a doctorate in sociology at Chicago University in 1896. There he became assistant, then associate, and finally in 1910 full professor. For more than two decades he had been co-editor of the *American Journal of Sociology*.

In 1918, the year in which *The Polish Peasant* was published, W. I. Thomas' professional career was irreversibly damaged by an affair.[71] Discovered together in a hotel room

[70] Cf. the letter of D. S. Thomas to Merton. There she writes that she even "swore I would never change my maiden name which I didn't" [D. S. Thomas in a letter printed in Merton, 1995, 401].

[71] For the following I use the short biographical note in [Volkart, 1951b], the detailed report in [Janowitz, 1966, xivff.], [Bulmer, 1984, ch. 4, 59ff.], [Lindstrom et al., 1988, 292ff.], and the online documents of the *Mead Project Inventory*, assorted and compiled by Robert Throop and Lloyd Gordon Ward (Brock University, Canada). The address is `https://brocku.ca/MeadProject/inventory5.html`. It contains thousands of files with documents written by or written about Georg Herbert Mead *or* persons directly linked to him. W. I. Thomas is one of them. The inventory is arranged alphabetically by author. Under "W. I. Thomas" one finds a bibliography with his articles and books. Then follows an extremely helpful section *Other Related Materials* that, among other things, contains a subsection *Newspaper Coverage of Thomas's Career* with dozens of original newspaper articles about the 1918 affair. Additionally, Loyd and Ward wrote for the inventory some short articles ("scrapbook pages" as the authors call it) on certain aspects of W. I. Thomas' life and career, namely [Throop and Ward, 2007b], [Throop and Ward, 2007a], and [Throop and Ward, 2007c]. Loyd and Ward try seriously and sys-

with the much younger wife of an army officer serving in France, the FBI arrested him on a charge of false registrations at hotels and a violation the so called *Mann Act*. That Act forbade interstate transport of "any woman or girl for the purpose of prostitution or debauchery, or for any other immoral purpose." Immediately the arrest was extensively publicized. Under the headline "Court Frees Thomas and Mrs. Granger" the *Chicago Daily News* from April 19, 1918, reports on the cover page that in front of an audience of about 500 people the charge of disorderly conduct was dismissed by the Morals court on the grounds that the offense—admitted by the defense—did not affect the public peace since it was done in a private room. W. I. Thomas and Mrs. Granger had met several times in other states, i.e. their affair involved interstate transport. But the FBI could not prove that W. I. Thomas had paid the tickets for Mrs. Granger, who insisted having paid at her own. Therefore the *Mann Act* was not applicable either. In the court, W. I. Thomas was supported by his wife Harriet Park Thomas (1865–1935). She was a child advocate, peace and woman's suffrage activist, well known in Chicago. In the days between the arrest and the court proceedings Harriet Park Thomas took care of Mrs. Granger.[72] From the point, when the affair became public, and without waiting for a court's decision, the president of the University of Chicago and supported by the board of trustees had forced Thomas to resign—to avoid being fired otherwise.[73] The University president also ordered the Chicago University Press to terminate the contract about the publication of *The Polish Peasant*. The distribution of the already printed first two volumes was stopped, the printing plates were handed over to the authors, and the complete work was then published by Richard G. Badger (The Gorham Press), Boston.[74] On request of the editor, another just finished manuscript of W. I. Thomas on immigrant adjustments (*Old*

tematically to give *evidence based*, rather than hearsay accounts. Their articles correct some details in [Bulmer, 1984] and [Lindstrom et al., 1988], and shed a different light on some events. The following details in the main text seem to be uncontroversial.

[72] In their article *A Beautiful and Impressive Southern Woman of Decidedly Individualistic Outlook: Notes on the Life of Harriet Park Thomas*, Robert Throop and Lloyd Gordon Ward discuss the assumption (or conjecture) that the arrest actually targeted at W. I. Thomas' wife because of her active pacifism. According to Kimbell Young (W. I. Thomas' teaching assistant at Chicago 1917–1918) that version, was believed by W. I. Thomas [cf. Lindstrom et al., 1988, 293]. After putting together all available pieces of evidence, Throop and Ward conclude that their quest to validate that "old rumor" *failed* [cf. Throop and Ward, 2007c, Summary].

[73] Cf. Kimbell Young in [Lindstrom et al., 1988, 294].

[74] On April 22, 1918, a few days after his dismissal, W. I. Thomas published his view on the affair in the *Chicago Herald*. It is an extremely readable article. On two pages he describes forcefully, thoughtfully, somehow distanced, and in no way broken, his view of,

> the incident which led to my dismissal from the University of Chicago ... in connection with a summary of my view of life in general, of the problems of teaching and investigation, of freedom from public oversight in the fields of private life, and in general with the problems of securing a more efficient and happy society, for only in that way can the incident have a meaning and value [Thomas, 1918, 15].

Personally, I find it incomprehensible that Janowitz declares that document,

> of little worth except as it represents a man tragically seeking to defend himself under circumstances of terrific personal pressure and therefore distorting his basic orientation both to social sciences and to contemporary social problems [Janowitz, 1966, xv].

World Traits Transplanted) was published under the names of two colleagues who had contributed almost nothing [cf. Lindstrom et al., 1988, 294]. As Martin Bulmer in his historical study, *The Chicago School of Sociology* notes, "the matter might be dealt with in different ways, but universities of that period were wholly intolerant of what they judged moral laxity" [Bulmer, 1984, 60].[75]

After the affair, W. I. Thomas and his wife Harried Park Thomas moved to New York. He taught at several places, e.g. Harvard and the New School of Social Research.[76] To support their independent work, both W. I. and H. P. Thomas received stipends from the banker family Frank Summer and his wife Ethel, the latter since long a close friend of Harriet.[77] The Laura Spelman Rockefeller Memorial Foundation commissioned W. I. Thomas to make an encompassing appraisal of the various approaches to study and control social behavior, a study, that then became his first joint project with D. S. Thomas, published as *The Child in America* [cf. Thomas, 1950, 664]. In 1927 W. I. Thomas even became president of the *American Sociological Association*. But he never regained a permanent position.

As it seems, H. P. and W. I. Thomas grew apart over the years. According to Throop and Ward that process may well have started long before the affair of 1918.[78] More and more time they spent apart. Supported by the Dummers, Harriet established her own flat, first in 1928 in Chicago, then two years later in New York. At that time Harriet had not doubts that her husband, now working on the above mentioned project on the Swedish emigration/immigration, and in that context traveling to Sweden, was living together with his research assistant D. S. Thomas.[79] After 45 years of marriage, and perhaps on Harriet's initiative, William I. and Harriet got officially divorced in 1934. The year later, Harriet died, aged 70.

D. S. Thomas and W. I. Thomas married in 1935 and moved to Berkeley in 1940, the year in which Dorothy was appointed there as a professor of rural sociology. When,

[75] In that moral climate, W. I. Thomas was not alone in his misfortune. Bulmer has a long list of other victims [cf. Bulmer, 1984, 60].

[76] The New School was founded in 1919. Thorstein Veblen, one of the founders, had been dismissed by the University of Chicago for similar reasons as W. I. Thomas.

[77] Cf. the section *Harriet's Life in Social Reform* in [Throop and Ward, 2007c]. See also [Lindstrom et al., 1988, 295].

[78] Throop and Ward suspect that the process started in 1904 when one of their sons, then aged 11, drowned in a lake during vacations. Only two of their children survived to adulthood. Cf. the section *Her life as a faculty wife* in [Throop and Ward, 2007c].

[79] In a letter to Ethel Dummer from mid-August 1930, Harriet writes:

> I will just tell you that he is now in Sweden beginning the study which you know. Dorothy Thomas is travelling with him,—went on the same boat; she is to take part in the survey, but I think there is no doubt that they are living together, and that the relationship has creative value, for him, at least.
>
> Professionally she has benefitted by the association of her name with his as coauthor of "The Child in America,"—but I'm sure she is not happy. I feel very sympathetic with her situation, but am quite helpless, of course.
>
> I have written you this without any pain except for the sad bungling in which human values are deflected and destroyed (cited after [Throop and Ward, 2007c] in the section *After Chicago*).

in 1942, the JERS staff members officially started their work under the directorship of D. S. Thomas, her old husband, now close to 80 (while she was still in her early 40s), usually accompanied her on all her visits to the camps.[80] In his *Reminiscences*, Miyamoto impressively describes the atmosphere of their visits in the following text passage. At the same time, the passage underscores a central JERS research problem, felt by almost all JERS field workers. To state it in Sakoda's words, the "nagging question of defining the problem of investigation" [Sakoda, 1989b, 222].[81] In retrospect, about four and a half decades later, Miyamoto writes:

> The research difficulty that bothered me most was the persistent feeling that JERS lacked focus. I frequently wished that Dorothy Thomas would specify our research problems more sharply. Dorothy and W. I. Thomas visited Tule Lake soon after our field staff was assembled. I was deeply impressed by these distinguished sociologists whose works I had known for a long time. Dorothy struck me as an attractive, very intelligent, and energetic woman. Curiously, one of the things about her that sticks most firmly in my memory is the habit she had of chain-smoking mentholated cigarettes. She never drew deeply on a cigarette, but pecked at it in short, quick puffs. The image sticks in memory, perhaps, because she was a good listener. She listened, and she smoked. Of course, she made comments and discussed our ideas, but more often she sat behind a haze of smoke with her sharp eyes fixed on the speaker. It seems strange how this picture still remains clear, while I recall relatively little of things she said. One trouble with her visits and conferences, I felt, was that the she did not talk enough. I wished she would tell us more about her own sociological orientation, indicate the kinds of problems which interested her, and guide us more specifically on how she wanted the research carried out. But she gave us relatively little direction.
>
> I was also surprised that W. I. Thomas did not participate in the discussions more. He too was a good listener. One of the things about his visits which my wife and I clearly recall, with amusement, likewise concerned his smoking habit. He rolled his own Bull Durham cigarettes, and perhaps because his aging fingers were no longer nimble, he spilled a fair amount of tobacco as he rolled them. He would sit on our quilt-covered, steamer trunk that served as a seat, roll his cigarettes over our makeshift coffee table, and finally carefully brush the spilled tobacco back into his pouch using the edge of a notepaper as a scoop. But after his visits, the cracks in the rough, shiplap flooring where W. I. had sat were invariably filled with spilled tobacco. On each occasion my wife and I spent some time digging out the tobacco from the cracks, but because W. I. was such a down-to-earth, grand, old man, who counseled us with pithy remarks and

[80] Cf. [Miyamoto, 1989b, 36]. W. I. Thomas also participated in staff meetings and JERS conferences; cf. [Thomas, 1952b, 136n].

[81] The problem is discussed in detail in Hansen's interview with Sakoda [cf. Hansen, 1994, 431 et passim].

> entertained us with droll stories, we actually enjoyed the small chore of digging out the tobacco he left behind [Miyamoto, 1989a, 148f.].

The problem of a lack of focus and direction in JERS, is reflected in D. S. Thomas' presidential address from 1952 (ten years after JERS had started). There D. S. Thomas frankly states that JERS—at least in part—used the "vacuum cleaner approach" [1952a, 667] of avidly collecting whatever one could get. Exploiting the wisdom of hindsight, she gives an *ex post* defense of what had frustrated all her field workers:

> This approach, undirected as it was and wasteful as it seemed at the time, paid off in the long run, for when the camps were liquidated, many of the records in the administrative files were either destroyed or buried in archives [ibid. 668].

The direct *and* intended outcome of JERS were two volumes. The *first* one, *The Spoilage*, published by the University of California Press in 1946, was authored by Dorothy Swaine Thomas and Richard S. Nishimoto "with contributions of Rosalie A. Hankey, James M. Sakoda, Morton Grodzins, Frank Miyamoto" [Thomas and Nishimoto, 1946, iii]. Figure 15 shows the inside front page. The volume was, as the two main authors write in the preface, "concerned with the short-term 'spoilage' resulting from evacuation and detention" [Thomas and Nishimoto, 1946, xii]. It was meant to cover that part of the Japanese minority that had relinquished their American citizenship, and returned to the defeated Japan. This group was concentrated in the Tule Lake Segregation Center,[82] that, therefore, is the main focus of volume one. In detail it studies the repressive measures, stigmatization as disloyal, martial law, and protest movements, that, finally, culminated in a mass withdrawal from American citizenship.[83] In terms of numbers over all evacuees, *The Spoilage* covered one out of six.

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[82] Revealing and underscoring some of their methodological views, D. S. Thomas and R. S. Nishimoto often refer to the centers as their "laboratories"; cf. e.g. [Thomas and Nishimoto, 1946, vii].

[83] The last page of *The Spoilage*, written in 1946, gives a bitter summary:

> With mass renunciation of citizenship by Nisei [*American born and educated children of Japanese immigrants; R. H.*] and Kibei [*Japanese immigrants' children born on American soil, but wholly or partially educated in Japan; R. H.*], the cycle which began with evacuation was complete. Their parents had lost their hard-won foothold in the economic structure of America. They, themselves, had been deprived of rights which indoctrination in American schools had let them to believe to be inviolable. Charged with no offense, but victims of military misconception, they had suffered confinement behind barbed wire. They had been stigmatized as disloyal on grounds often far removed from any criterion of political allegiance. They had been at the mercy of administrative agencies working at cross-purposes. They had yielded to parental compulsion in order to hold the family intact. They had been intimidated by the ruthless tactics of pressure groups in camp. They had become terrified by reports of the continuing hostility of the American public, and they had finally renounced their irreparably depreciated American citizenship.
>
> Many of them have since left the country, voluntarily, to take up life in defeated Japan. Others will remain in America, in the unprecedented and ambiguous status of citizens who became aliens ineligible for citizenship in the land of their birth [Thomas and Nishimoto, 1946, 361].

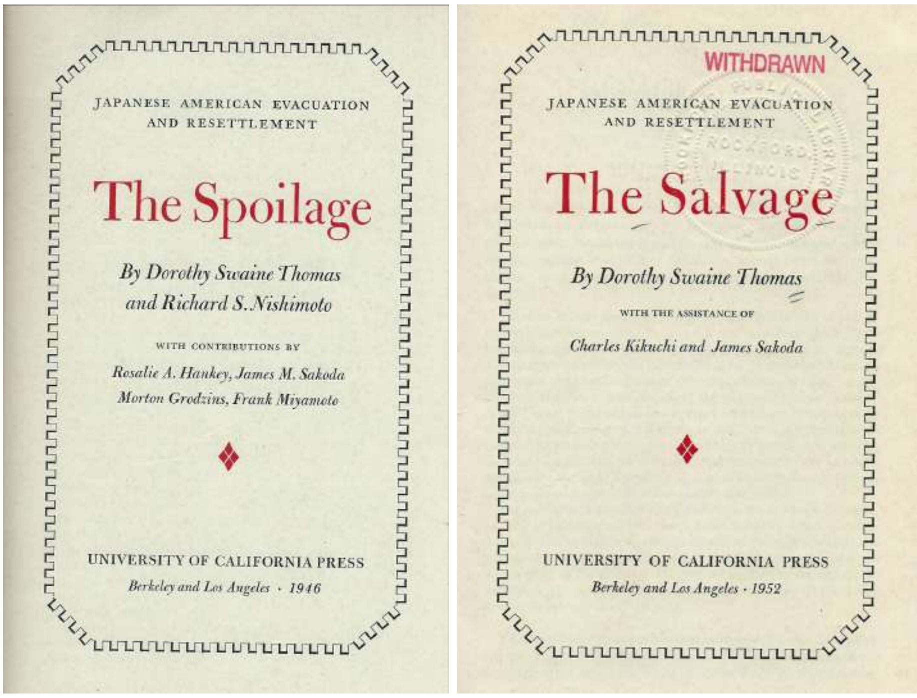


Figure 15: The inside front pages of *The Spoilage* and *The Salvage*. (Pictures from my second hand copies. The stamp "WITHDRAWN" indicates that it has been a library copy.)

The *second* volume, *The Salvage* was published in 1952. It is focused upon the resettlement policy that the War Relocation Authority (WRA) began in 1943. Continuing the policy of forced mass migration by other means, detainees, classified as "loyal", were released from the camps but had to resettle in the Mid-West and East of the US. Part I deals with the political, social, and economic development in the course of immigration and settlement from the turn of the century to 1945. Part II, about 450 pages, is a collection of fifteen life histories. In terms of numbers, *The Salvage* covered one out of three incarcerated persons—numbers, as they were by the end of 1944, when the exclusion orders of the Japanese from the West Coast were rescinded, and the closing of all camps between June and December 1945 was announced [cf. Thomas, 1952b, v]. Sakoda is mentioned as a contributor on both, *The Spoilage* and *The Salvage*. And indeed, he had gathered statistical data in Tule Lake and in Minidoka that were used in the volumes. But, as Sakoda reports in his *Reminiscences*, he was not consulted on the writing of the books [Sakoda, 1989b, 244].[84] All in all, the coverage of the two volumes was less than a half of the detainees. Obviously, there was a major residue, consisting of all the inmates that stayed in the relocation camps until their closing. Since an evaluation of their lives and fates after the camps would make sense only after a considerable passage of time, the preface of *The Spoilage* states that a study of

In 1949, a decision of a United States Court of Appeals provided a legal basis for restitution of their citizenship. To some degree that relativised the just quoted bitter summary; cf. the later remarks in *The Salvage* [Thomas, 1952b, 105].

[84] Authorship and co-authorship issues, Sakoda's status as a contributor are described and discussed in detail in Hansen's interview with Sakoda [cf. Hansen, 1994, 438ff.].

the residue is "not included in the present publication plans" [Thomas and Nishimoto, 1946, xiii]. That was a view on the residue under the perspective: "What will happen with them in the future?" But there was also another instructive question: the residue, that were all the imprisoned that could have left the camps. Why didn't they leave the camps earlier? Taking that perspective did not require any passage of time, all the necessary data and documents were already collated, and it brings to focus a huge group of evacuees, whose sheer existence comes as a surprise—at least at first glance. In his retrospective article *The "Residue": Unresettled Minidokians, 1943–1945*, Sakoda writes that, besides the two volumes *The Spoilage* and *The Salvage*,

> Dorothy Thomas had planned a third volume entitled "The Residue," which however was never published. This book, possibly based on my fieldwork at the Minidoka Relocation Center in Idaho, was supposed to deal with those evacuees who declared themselves "loyal" to the United States, but stubbornly resisted War Relocation Authority pressures to leave the relocation centers. When the WRA announced camp closure at the end of 1944, many wanted to remain "like Indians" in the security of the camps. Under the thread of expulsion, some had fantasies of being rescued by victorious Japan. The vast majority of the Issei, along with their dependents and some Nisei and Kibei, comprised this unresettled group of evacuees about whom hardly anything has been written [Sakoda, 1989a, 247].

The idea of a future living "like Indians" was not an ascription of the late Sakoda, it is reported already in his dissertation [Sakoda, 1949, 321]. In the end, some of the inmates, desperately clinging to the security of the camps, had to be evicted by brute force. The reasons were twofold. First, the expected hostility outside the camps, combined with all the expected difficulties to reestablish a life, and to find a home and a job. Many thought that they could not survive in the outside world. Second, many imprisoned had become accustomed to living as wards of the government. Sakoda describes how the work ethics of previously hard working people deteriorated, and a demand for aid became the norm. In Sakoda's view, for these inmates the tragedy was not the guard towers or the tar-papered barracks, rather than "the loss of their sense of independence" [Sakoda, 1989a, 262]. Section X of Sakoda's dissertation analyses the tragical process of Minidoka's closing. The section's telling title is *Eviction* [cf. Sakoda, 1949, 321ff.]. Based upon his observations while participating in the closing of Minidoka, section X contains moving descriptions of personal tragedies.[85]

As Sakoda reports, D. S. Thomas made an offer to publish his dissertation. But it was a *conditional* offer:

[85] Cf. Sakoda in the interview [Hansen, 1994, 405ff.]. Sakoda reports that to avoid leaving the camp some people even went into hiding. Then he continues:

> One fellow they caught hiding under the house. What they would do would be they would take them in the car and take them to the station and just dump them out at the station. The administration at Minidoka felt that that was where their responsibility ended [ibid. 407].

> She once offered to publish my dissertation if I would take out the theoretical section, but I was unwilling to do that and she did not bring up the matter again. Why she did not write a book about those who remained in the relocation centers is still a mystery to me, since they constituted the largest proportion of the evacuated population and the subject of the largest amount of field research [Sakoda, 1989b, 243f.]

When making her publication offer, D. S. Thomas—skeptical against theory as she was—may have had in mind a volume very similar to *The Salvage*, i.e. a comparatively short chronological analysis with some statistics, followed by a collection of extensive life histories.[86] In his *Reminiscences of a Participant Observer*, Sakoda—and that is obviously critically—annotates that *The Salvage* presented its material "without theoretical explanations or classification into types" [1989b, 243].[87]

That he was unwilling to do something similar with his material by taking out his theoretical part, is no surprise. It would have meant to cut out the heart of his dissertation project: theorizing based upon field data, integration of theory and observation. In all likelihood, D. S. Thomas considered Sakoda's checkerboard model as part of the theoretical section that he had to take out for a publication. In the interview with Hansen, Sakoda reports that he talked to some commercial publishers. Later, after the death of D. S. Thomas in 1977, he contacted the University of California press. But no one was interested [cf. Sakoda in the interview with Hansen, 1994, 417]. Summarizing his activities to publish his dissertation, Sakoda states that he "really didn't try that hard" [ibid. 444].

As a consequence, the work with the very first checkerboard model of social interaction ever developed never got published. However, Sakoda's *The "Residue": The Unresettled Minidokians 1943–1945*, was published in 1989 as one of his two contributions to the JERS retrospect *Views From Within*. It contained a 36-page summary of his dissertation [Sakoda, 1989a]. As in the dissertation, there is an appendix titled *Checkerboard Model of Social Interaction* [ibid. 282–84], which shortly describes the origin, design idea,

[86] In his *Reminiscences*, Sakoda writes:

> It would appear that if Thomas had been a social psychologist interested in theory, the JERS publications would have been quite different. She was most at home conceiving of JERS in demographic terms—in particular, as a study in migration from the relocation center, using statistical data. She was comfortable with presenting field data in historical terms, as she did in *The Spoilage*, or offering case histories without commentary, as in *The Salvage*. She chose not to use terms, such as "definition of the situation," "cultural conflict," "organization" and "disorganization," as W. I. Thomas had in *The Polish Peasant in Europa and America* [Sakoda, 1989b, 244].

Miyamoto describes D. S. Thomas' theoretical orientation as *neo-positivism* and lists four characteristics of that position: experimental basis of knowledge, nominalistic basis of knowledge, the aim of quantification, and the value-free orientation of science [Miyamoto, 1989b, 31ff.].

[87] In the interview with Hansen, Sakoda is even more critical:

> No comments, no analysis, just case studies thrown in there. ...And the summaries are terrible [Sakoda in the interview with Hansen, 1994, 440].

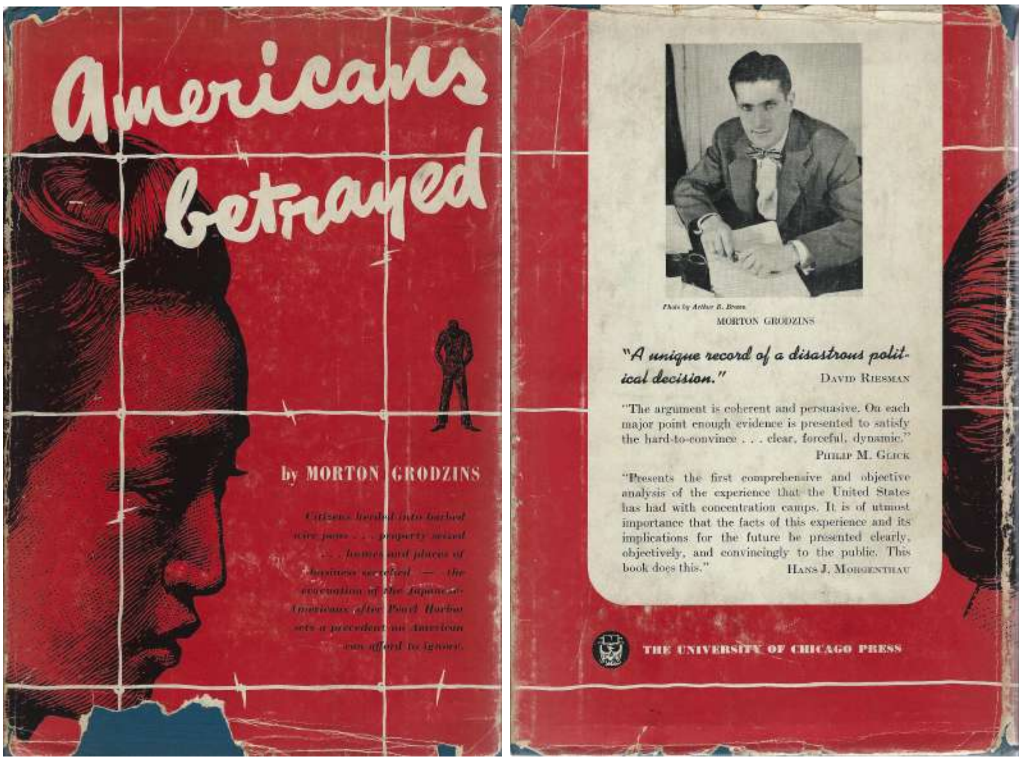


Figure 16: Grodzins's book *Americans betrayed*, published 1949 by the University of Chicago Press. Dust jacket picture of my damaged second hand copy.

and purpose of the model. Additionally, the appendix shows a graphically improved version of the originally handmade figure 14 above.[88]

While Sakoda's dissertation thesis was buried in the University of California Library's Archive, the publication of another JERS staff member's dissertation became a major affair.[89] Morton Grodzins (1917–1964) was one of the few "Caucasian" (i.e. white) JERS research assistants. Basically he was D. S. Thomas' right hand in organizational matters.[90] At the same time he was a doctoral student in the Political Science Department of the University of California. In the project he had researched the events and processes that then in early 1942 had lead to the radical exclusion decision. In March 1945 he defended his thesis *Political Aspects of the Japanese Evacuation*. In the preface to the slightly revised publication of the thesis in 1949, Grodzins frankly states that there was

> one pertinent bias of importance with which I came to this work. This bias holds that there is, in America, no inferior class of citizens or of noncitizen residents according to criteria of race or religion; that, stated positively, Americans must be accorded their legal rights and privileges as individuals

[88] For other differences cf. footnote 61.

[89] The following is mainly based upon [Suzuki, 1986], [Suzuki, 1989], and [Murray, 1991].

[90] Different from the Japanese staff members, he could freely move. In a letter to D. S. Thomas, after a visit of Grodzins to Tule Lake, Sakoda considered Grodzins as Thomas' "personal emissary" [cf. the reprint in Sakoda, 1989b, 233].

> and not as units of a group with real or imagined special characteristics [Grodzins, 1949, ix].

The book sharply attacked West Coast politicians and pressure groups for having stirred the military and the central government towards the decision to evacuate the Japanese from the West Coast; it sharply condemned that persons were moved to "concentration centers" without any charges filed against them and no guilt ever attributed to them. Grodzins' main point was the legal and political precedent. For him it was a threat to *all* Americans [cf. 1949, 1ff.].

D. S. Thomas was a member of the dissertation committee; the chair was the political scientist, Charles Aiken. Already in September 1944, in reaction to the draft of the thesis, D. S. Thomas had written that, after its approval, the thesis would be withheld from circulation for the duration of the war. When in August 1945 Grodzins wanted to publish his thesis as a book, it came to a conflict between D. S. Thomas and himself. Both had quite different definitions of the situation. For D. S. Thomas it was a conflict about illegal and illegitimate property claims of a paid research assistant whose thesis used material that was in ownership of the University of California. Additionally, the thesis was in her view "propagandistic" and in need of a very serious revision before one could think of a publication, which, in the end, she had to allow—or not. For Grodzins it was more a conflict about censorship and the tasks of political scientists. The conflict escalated massively when Grodzins convinced the University of Chicago Press—compared to the University of California Press, the more prestigious publisher—to publish his manuscript. As it seems, the conflict was *not* a clear case in all the relevant scientific, moral, legal, or political dimensions.[91] But at the same time, it is pretty clear, that D. S. Thomas (by usual standards probably the most successful female sociologist of here generation[92]) did *not* thoroughly act reasonably in the conflict. She did not distinguish the ownership of the JERS materials and the ownership of the dissertation manuscript; she approved the dissertation in the committee and later doubted the scholarship of the manuscript. The conflict became a serious matter between the University of California and the University of Chicago with its University of Chicago Press. Due to the massive support from William Terry Couch, the chief editor of the University of Chicago Press, Grodzins' slightly revised dissertation was published as *Americans Betrayed: Politics and the Japanese Evacuation* by the University of Chicago Press [Grodzins, 1949] (see figure 16). It was one of the very first books with a sharp indictment of the mass incarceration of the Japanese minority. The book was nominated for the Pulitzer Prize and received very good reviews [cf. Suzuki, 1989, 107].

When in 1951 Couch was fired, Morton Grodzins became the new chief editor of the Chicago University Press. Grodzins' inofficial JERS publication induced another official JERS publication. It took D. S. Thomas a lot of time and effort, but she finally succeeded assembling a team of authors that wrote *Prejudice, War and the Constitution* [tenBroek et al., 1954]—in parts a kind of "counter Grodzins", arguing against

[91] Cf. the fairly balanced discussion and conclusions in [Murray, 1991, 145–49].

[92] Cf. the study [Bannister, 1998]. The reference includes a link to an English translation.

the decisive influence of political and economic pressure groups in the process that had lead to the detention of the West Coast Japanese minority [cf. ibid. 183–208].

However, even a superficial reading of *Prejudice, War and the Constitution* makes it very clear, that despite the disagreement about Grodzins' causal claims, the authors were as critical about the mass imprisonment as Grodzins was: It was "a blow at the liberties of us all" [tenBroek et al., 1954, 334]. In the text above (section 3.1, *Research Behind Barbed Wires*, 28ff.), we used the books [Grodzins, 1949] and [tenBroek et al., 1954] for our short history of the mass incarceration. As contributions to contemporary history, both books could obviously not avoid, in their coming to existence, to become affected by effects and consequences of the processes that they described.

JERS' vacuum cleaner approach produced enormous numbers of documents and data, but only three official books plus Grodzins' unofficial *Americans Betrayed*. When D. S. Thomas left the University of California in 1948, she organized that all the JERS documents went to the Bancroft Library, University of California, Berkely. There they are till now.[93] As Sakoda wrote in his late *Reminiscences of a Participant Observer*,

> it is difficult not to reach the conclusion that a great deal of valuable data was collected by JERS fieldworkers, but much more could have been published [1989b, 244].

Sakoda concludes with the hope that younger scholars "take up the task that an earlier generation failed to complete" [ibid.].[94]

3.4 Sakoda's Early Checkerboard Model: When Invented?

After Minidoka was officially closed at the end of October 1945, Sakoda returned to Berkeley.[95] He took courses in psychology and statistics (especially factor and cluster analysis, the latter being tought by Robert Tryon, who pioneered cluster analysis). An application for an assistantship in the Psychology Department did not succeed. Sakoda had the feeling "that the university was afraid to hire a Nisei coming back" [Hansen, 1994, 442]. But D.S. Thomas took Sakoda as a graduate assistant. At that time, the dissertation was still to be written. D.S. Thomas had allowed him to use the material that he had gathered as a JERS fieldworker. Sakoda applied for a Social Science Research Council (SSRC) research training fellowship. He succeeded and got a fellowship "for the writing of a doctoral dissertation on a socio-psychological analysis of field data gathered in the Minidoka Relocation Center" [SSRC, 1947, 11].[96] David Krech (1909–1977), who had joined the faculty of Berkeley as a social psychologist

[93] The address is `http://bancroft.berkeley.edu/collections/jais/`. Parts of the documents are accessible online.

[94] For that task Sakoda had especially in mind younger people with a Japanese lineage that usually do not command the Japanese language. They could profit from the fact that all the JERS documents were written in English.

[95] For Sakoda's problems and next steps after he had returned to Berkeley cf. Hansen's interview with Sakoda [1994, 442ff.].

[96] The SSRC decisions were made during the four months from October 1, 1946 through January 1947. Twenty fellowships were awarded [cf. SSRC, 1947, 10]. A fellowship was also awarded to

in 1947, became Sakoda's principal adviser and dissertation director at Berkeley.[97] In the preface to his dissertation thesis, Sakoda writes that he got the fellowship for a year and a half, which made it possible to study for a semester at Harvard [cf. 1949, ii].

But *when exactly* did Sakoda get the checkerboard idea? Was it born at Minidoka?[98] The answer is "No". As it seems, it was after the war, two to three years after the closing of Minidoka. In 1989, in the opening sentence of his appendix *Checkerboard Model of Social Interaction*, Sakoda writes:

> I developed the checkerboard model of social interaction when I was at Harvard's Department of Social Relations on a one-semester scholarship from the Social Science Research Council (with which Dorothy S. Thomas was associated) working on my dissertation under Clyde Kluckhohn's supervision [Sakoda, 1989a, 282].[99]

The Social Relations Department at Harvard—its complete name was Department of Social Relations for Interdisciplinary Social Science Studies—brought together psychology, anthropology, and sociology. The department was founded in early 1946. In Hansen's interview, Sakoda says about his time there:

> I met Dr. Gordon Allport in psychology, but I took some sociology seminars, including one by Sam Stouffer and Talcott Parsons, who was the outstanding theorist, but I couldn't understand the sociological concepts. That wasn't too helpful. But the atmosphere must have helped because I

Sakoda's fellow student Tamotsu Shibutani, who had introduced Sakoda to D.S. Thomas (see page 31 above). Shibutani got his fellowship "for completion of a doctoral dissertation on the nature and function of rumor in collective behavior" [SSRC, 1947, 11].

[97] Krech's interests covered a very broad range of areas of psychology. For Krech's biography, life, and work cf. [Ghiselli, 1978] and [Innis, 1998]. In Hansen's interview, Sakoda says about his principal adviser:

> Dr. Krech was quite helpful. He had authored a social psychology textbook in which he had propositions that he had developed. That general approach was helpful [Sakoda in the interview Hansen, 1994, 443f].

In his dissertation Sakoda refers to [Krech and Crutchfield, 1948]. That book is meant here as well. There were two more members of Sakoda's dissertation committee, Robert Choate Tryon (psychology), and Martin Bernard Loeb from outside the department (cf. the signatures in figure 11).

[98] Personally, I read *The Spoilage* long before I got access to Sakoda's dissertation. Only then I noticed the early version of his checkerboard model. Not knowing the dissertation, my idea was that the checkerboard structure of Sakoda's 71–model was somehow inspired by the "checkerboard structure" of the barracks in which the inmates of all the relocation or segregation camps had to live (cf. the aerial picture in figure 13). At the same time, *The Spoilage* shows the blockwise clustering of the loyalty issue [cf. Thomas and Nishimoto, 1946, 105]. Taking that together, it might have inspired the checkerboard model—that was my speculative thought. But there is *no* evidence in Sakoda's writings that supports such a speculation. Actually it is the contrary.

[99] There is an incoherence. In the preface to his dissertation it is a fellowship for one and a half years. Forty years later, in his article [Sakoda, 1989a], it is a one-semester scholarship. Probably the preface is right, and he spent one semester at Harvard. Clyde Kluckhohn (1905–1960), Sakoda's supervisor at Harvard, was a cultural anthropologist and an expert on Navajo culture.

> developed my checkerboard model while I was at Harvard [Sakoda in the interview with Hansen, 1994, 444].

In an answer to my search request, Harvard University Archives wrote that "after reviewing various University directories, it appears that James Sakoda was at Harvard during the Spring term of 1948."[100] Thus, the available evidence suggests that Sakoda developed his early checkerboard model sometime between January and May 1948.[101]

When reading Sakoda's dissertation, the tone and mood of his writing does not cease to astound. There is not a shred of bitterness in the writing of the voluntarily observing field worker who, at the same time, had involuntarily to "participate" as an inmate in a relocation or segregation camp (sometimes called "American concentration camp")—and that over years. How is that possible? At least part of an answer can be found in Hansen's interview with Sakoda. On the second day of his interview with Sakoda, Hansen nudged Sakoda several times to draw a balance sheet on his life during world war II [cf. ibid. 409f]. In his answers, Sakoda is well aware of the losses of a lot a people, as, for instance, the losses of the family of his later wife Hattie which he had met while teaching a psychology class at Tule center [cf. ibid. 419].[102] But Sakoda perceived his personal situation as quite different: "In a way, I didn't lose anything" [ibid. 379]. He had returned from Japan to Berkeley as an outsider, "first arriving in Berkeley without a friend and little money" [ibid. 411], "a poor, struggling student", experiencing a "very meager living" [ibid. 410].

[100] Harvard University Archives on January 31, 2017, in a mail to the author.

[101] Additional indirect evidence is the following. In the interview with Hansen, Sakoda reports that his wife Hattie "stayed with Dorothy while I was at Harvard, and we were close when W. I. passed away" [Hansen, 1994, 445]. W. I. Thomas had died on December 5, 1947, at Berkeley. Later in 1948, D. S. Thomas moved from Berkeley to the University of Pennsylvania. She was appointed to the position of professor of sociology and joined in April 1948 the standing faculty of the Wharton School. (The Daily Pennsylvanian, issue from April 23, 1948, appeared on the front page with an article titled "Dr. D. S. Thomas to be first woman prof. in Wharton". The article reports that with "Dorothy Swaine Thomas, one of the foremost women sociologists in the United States has been appointed professor of sociology at the Wharton School. She becomes the first women professor in the history of the school.") That suggests that Sakoda's wife Hattie stayed with D. S. Thomas in the first months after W. I. Thomas' death when D. S. Thomas was still at Berkeley before leaving for Pennsylvania. And that was also the time in which Sakoda was at Harvard. Sakoda kept social contact with D. S. Thomas after he later had left California and lived in the East as well [cf. Hansen, 1994, 445].

[102] Sakoda describes the "evacuation" of Hattie's mother and family:

> At the time the family was evacuated, they had to sell the rights to the store. She had to sell the piano and her living room furniture. Hattie said that a lot of things just went for twenty-five dollars. So basically, they lost everything they had [ibid. 412].

But for Sakoda, there was another side as well. The quotation continues:

> While in camp, clearly Hattie's mother was in a situation like a lot of Issei: for the first time, it was like a vacation, like living in a camp, actually, a recreational camp. It was a little rough in many ways. You lived in a little room and all that, but then you had a community dining room. But in camp, you had classes you could take and you had religious services and entertainment. I think she enjoyed life in camp [ibid.].

Cf. Sakoda's generalizing remarks [ibid. 414 and 420].

> From that, I got into being a research assistant, being paid not a great deal but being paid a little bit while I was in camp, which provided a little bit of extra money. I was also disbursing funds for people who did clerical work or typing or things like that for me, and I also became a teacher and got positions that would have been unthinkable on the outside. I got married [ibid. 410].
>
> So it was a big climb upwards. From that point on, it was a regular academic career ... The whole event put me on that particular ladder upwards, which, if it had not been for Evacuation, that probably wouldn't have happened [ibid. 411].

For Sakoda it became a climb up the academic ladder in the direction of computational social science.

4 Computational Social Science in 1950ff: A Well Known Sakoda and an Unknown Schelling

After completing his dissertation, Sakoda got a first appointment at Brooklyn College [cf. Hansen, 1994, 444]. The teaching load was high: Sakoda had to teach five classes, probably two statistics courses and three social psychology courses. A research position in the U.S. Department of Defense did not work out. In the interview with Hansen, Sakoda reports that when he "went to see the guy in charge, he said, 'I couldn't hire a Japanese' " [Hansen, 1994, 445]. In 1952, Sakoda took up an assistant professorship at the University of Connecticut, where he stayed for the following ten years, before moving to Brown University where he remained until his retirement in 1981.

After his dissertation there isn't any publication in which Sakoda comes back to participant observation—except for his JERS retrospect in [1989b]. Already during the time of the JERS study, Sakoda had done a lot of statistical analysis. Now, and since the 1950s, that became Sakoda's predominant focus, method, and topic. After the two quantitative empirical studies [Gideonse and Sakoda, 1950][103] and [Lifshitz and Sakoda, 1952], he published on factor analysis [Sakoda, 1952, 1954, 1955], and contingency tables [Sakoda and Cohen, 1957]. He authored and co-authored articles on different methodological problems such as the volunteer error in empirical studies on sexual behavior [Maslow and Sakoda, 1952], problems of significance [Sakoda et al., 1954], effects of the order in which pictures are presented [Dollin and Sakoda, 1962]. As a co-author Sakoda contributed to the articles on reinforcement learning [Brand et al., 1956, 1957a,b]. Some methodological problems of these studies are discussed in [Sakoda, 1956]. The two articles [Sakoda and Willey, 1961; Sakoda and Greenwood, 1961] describe a tricky "misuse" of punchboards as multiple choice self-learning devices.

[103] After the end of World War II, former U.S. soldiers entered massively the U.S. universities. [Gideonse and Sakoda, 1950] is about their academic success at Brooklyn College. Sakoda's co-author Harry D. Gideonse (1901–1985) was at that time the president of the college. He presided Brooklyn College over the period 1939 to 1966.

In the middle of the 1950s, Sakoda took the *computational turn*:

> I got into computer programming when IBM offered a summer course at MIT, where they established a computer facility for New England colleges [Sakoda in the interview with Hansen, 1994, 445].

As he later mentions in an article, in 1956 he "had struggled through the assembly language programming course", offered as a summer institute by the MIT Computation Center, set up by IBM "for use by New England universities and colleges" [Sakoda, 1982b, 827]. While there he learnt from someone at IBM that a language called FORTRAN was under development. In 1957, the year in which FORTRAN was made available, Sakoda attended a course on it in Boston [cf. ibid.]. Sakoda became a research associate at the MIT Computer Center where he worked on statistical programs written in FORTRAN. That had long lasting consequences.

4.1 DYSTAL: Sakoda's General Computer Language for the Social Sciences

From the early beginnings of computational social science, there has always been—and somehow still is—*one big problem*:

> Social scientists and other application people are going through a painful process of developing procedures that give them greater control over the computer. A user-oriented computer system is a promise held out by many, but as yet successes are relatively few [Beshers, 1968, 7].

That was written in 1967 (and published in 1968). It is from James M. Beshers's *Postscript* to the 2nd edition of *Computer Methods in the Analysis of Large-Scale Social Systems*, the proceedings of a conference, held in October 1964 at the Joint Center for Urban Studies of MIT and Harvard University. The conference was stimulated by the release of the 1960 1-1, 000 sample tape by the U.S. Bureau of the Census [cf. Beshers, 1965, 1].[104] The conference was meant to share experience and bring together experts on computer applications with others that had begun research that involved computers. Among the experts were, for instance, Joseph Weizenbaum (1923–2008), a latter-day AI-sceptic and pioneer of computer ethics, as well as Guy H. Orcutt, who had pioneered an "agent-based" microsimulation approach.[105] Sakoda was another participating expert. His contribution, *A General Computer Language for the Social Sciences* [Sakoda, 1965c] is the second in the first section of the proceedings. What the

[104] Later, in 1972, Sakoda wrote an article together with his son William on how to modify the coding of the U.S. census public data samples in order to make the data more accessible for a computational analysis [cf. Sakoda and Sakoda, 1972]. For the computational analysis of census data cf. as well [Sakoda and Karon, 1973].

[105] Orcutt did not use the term *agend-based*. But he argued already in 1957 that models of socio-economic systems should be "stated in terms of the behavior and interaction of the elemental decision-making units. Then, and only then, could ways be found of aggregating relationships without a disastrous loss of accuracy of representation" [Orcutt, 1957, 116].

Figure 17: James M. Sakoda (first row, second from left to right) with the Brown Faculty 1963/64.

title announced was *not* a plea to develop such a programming language in the future; the title was Sakoda's ambitious claim that he had actually developed a general programing language designed for the specific requirements of social scientists.

At the time, FORTRAN was the most popular programming language in science. It was one of the first high-level, general purpose languages. As such, it allowed to write programs in a language above the machine or assembly language level. The name of the language reveals its central objective and design idea. "FORTRAN" is an acronym for *for*mula *tran*slator. The language allows to code formulas and calculations in a way very close to their mathematical form. A built-in compiler translates the code into the machine language.[106] FORTRAN allowed to write a very efficient code. Numerical calculations were very fast. But at the same time, by his working with FORTRAN, Sakoda had painfully realized the obstacles, stumbling blocks, and difficulties in doing computational social science with FORTRAN. For instance, a dynamic creation of matrices of an unanticipated size was not possible, no operations for sorting, ranking or string comparisons were provided. Sakoda's list of inadequacies of FORTRAN is long [cf. Sakoda, 1965c, 31]. Additionally (but *not* mentioned by Sakoda in his conference contribution), FORTRAN allowed for GOTO commands with their inherent seduction to write a source code with opaque branching structures—the infamous *spaghetti code*: error-prone, hard to understand, and even harder to debug.[107] In sum, and putting it mildly, FORTRAN was a language that "does not lend itself well to many of the kinds of programs the social scientist desires" [Sakoda, 1965c, 31]. With his DYSTAL Sakoda meant to change that.

"DYSTAL" is an acronym generated from *dynamical storage allocation*. A technology for a dynamical allocation of memory was a precondition for many programming tasks. IPL-V, a new language, allowed for that. Furthermore, it had some list operations that were missing in FORTRAN. But IPL-V had other major disadvantages:

> Even a simple device like a checkerboard could not be easily represented. ...Moves on a checkerboard could not be specified by incrementing two subscripts as one could in FORTRAN [Sakoda, 1982b, 827].

Sakoda's DYSTAL achieves dynamical storage while keeping lists or matrices in their consecutive order in the memory—an unusual approach that, compared to the use of a chained word list, makes computing much faster. At least that was the hope.[108] But, surprisingly, DYSTAL was written in FORTRAN, and Sakoda had good reasons for that: A general computer language for *social scientists* had necessarily to be a high-level programming language. Such a language requires a compiler or a translator, whose development and maintenance is a hard and an expensive task. If, instead, FORTRAN is used as a *host language* for the social science programming language, then that

[106] For the history of FORTRAN (later: fortran) see [Backus, 1979].

[107] But Sakoda knew that problem very well. His article *Structured programming in Fortran* [Sakoda, 1974a] analyses the problem and gives advice how to avoid it. See also Donald E. Knuth's paradigm of *literate programming* [Knuth, 1984].

[108] He writes: "This achievement is DYSTAL's chief contribution to the programming art" [Sakoda, 1965c, 32]. For technical details cf. ibid., [Sakoda, 1979], and [Sakoda, 1982b]. For the discussion of Sakoda's approach during the above mentioned conference in 1965 cf. [Stone, 1968].

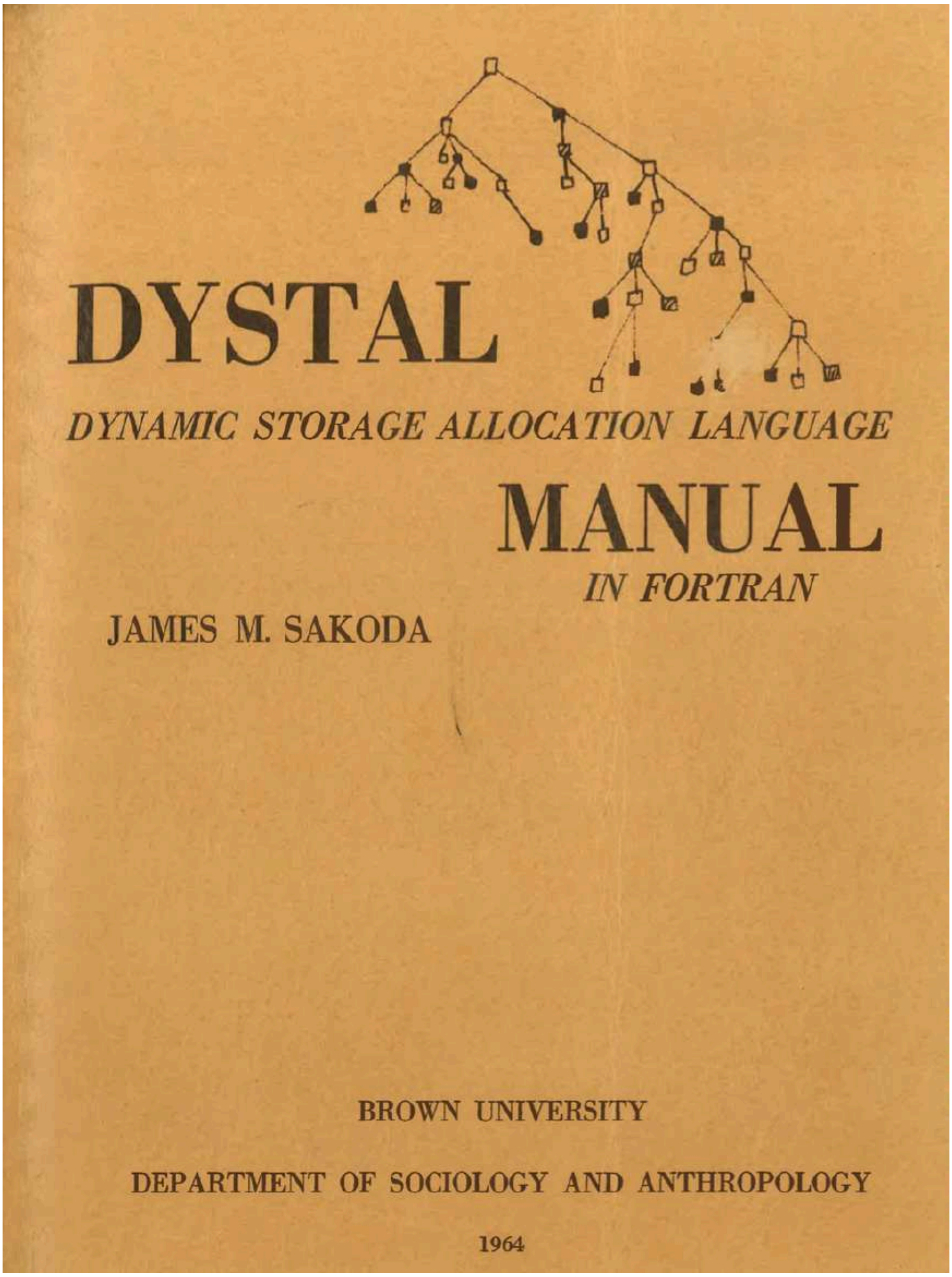


Figure 18: Sakoda's first DYSTAL Manual from 1964. "Those who desire a copy of the DYSTAL FORTRAN subroutines should send a blank tape to the author. ... Additional copies of the manual will be made available to those writing for them as long as the supply lasts" [Sakoda, 1964, iv].

PREFACE

DYSTAL stands for DYnamic STorage ALlocation, and is pronounced "dis-tal." DYSTAL, at present writing, is based on some 90 Fortran subprograms.

This is a teaching manual which describes how to write computer programs in DYSTAL. It assumes a basic knowledge of Fortran programming. The more elementary concepts which are basic to the language are introduced in the earlier chapters, and these should be taken up in sequence. DYSTAL provides a wide range of capabilities, and it is expected that a programmer will not have need for all of the routines at any given time.

DYSTAL has list-processing capabilities, and it is indebted to IPL-V and SLIP for many of its routines. However, DYSTAL's method of organizing storage and lists is basically different from that used by other list-processing languages. This method can be considered to be DYSTAL's contribution to the list-processing field.

The development of DYSTAL was supported by two PHS research grants, M-6127 and MH-8177, from the National Institutes of Health of the U.S. Public Health Service.

An early version of DYSTAL, called SAL, was developed at the Social Science Research Council sponsored Cognitive Processes Summer Institute in 1963 at the Rand Corporation, Santa Monica, California, and first run on the IBM 7090. The present version was tested out on the Brown University Computing Laboratory's IBM 7070 with 5000 words of core storage. For any extensive programming, however, 10,000 words of memory is desirable. With only minor changes it should be possible to use most of the DYSTAL subprograms on any machine with a Fortran compiler having subroutine compiling capabilities. Possible exceptions are the packing and unpacking routines. The Appendix contains a complete listing of the Fortran subprograms.

ii

DJG

Figure 19: Preface of Sakoda's DYSTAL Manual [Sakoda, 1964, ii].

language can be used on all computers that can run FORTRAN. The same strategy was used in the 1960s by Joseph Weizenbaum when he developed the programming language SLIP, the Symmetric List Processor.[109] Sakoda explicitly states that he picked up ideas from SLIP and IPL-V [see Sakoda, 1965c, 32].[110]

Given the fundamental design idea, it is clear how the DYSTAL syntax works. DYSTAL commands are written as FORTRAN subroutines or functions, and using FORTRAN operations as primitives. The use of functions means that there are almost no restrictions on arguments of functions. Another advantage is, that functions can be nested (within *one* line of code). In 1982, and looking back, Sakoda writes that he made "use of FORTRAN IV to accomplish unFORTRAN-like operations, while integrating numeric and nonnumeric procedures" [1982b, 827]. In the preface to the first manual, Sakoda reports that an early version of DYSTAL—at that time called SAL[111]—was developed at a summer institute at the RAND Corporation, Santa Monica, in 1963 (see figure 19) [cf. Sakoda, 1964, ii].

A first version of DYSTAL was "released" in 1964:

> Those who desire a copy of the DYSTAL FORTRAN subroutines should send a blank tape to the author [Sakoda, 1964, iv].

A description of the language was given by the *DYSTAL Manual* (see figures 18 and 19).[112] The preface of the *DYSTAL Manual* is dated from July 1, 1964 [cf. ibid.]. A first summer institute on DYSTAL took place in July 1964 and was advertised in *The Brown Daily Herald*, Saturday, May 30, 1964 (see figure 20). An extended version of the manual appeared in 1965 [Sakoda, 1965b].

There were two versions of DYSTAL. But already in the early version the list of DYSTAL operations is long:

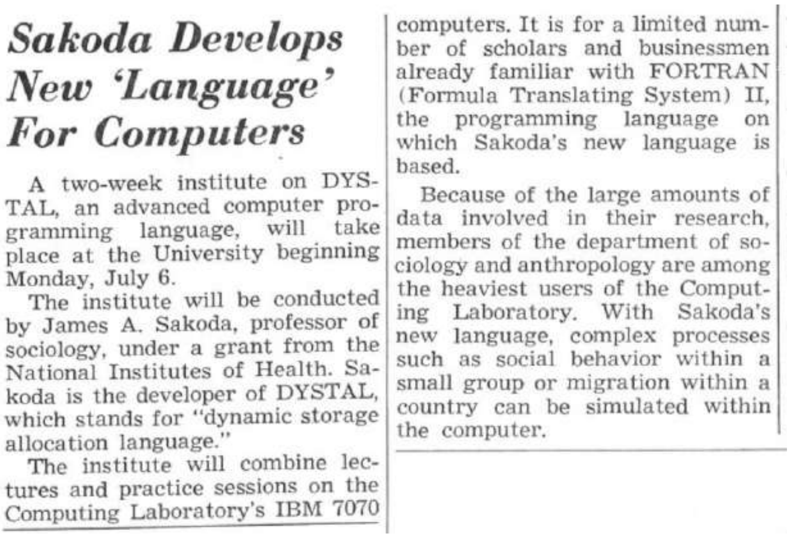
> So far, some 90 functions have been written to cover not only the basic procedures but also special routines in a variety of areas. In list processing it is possible to insert and delete items, locate items on a list, and create or read in complex data structures. ...Existing tree structures can be traced down to the end branches and back up again, each time returning the name of a list. String operations include unpacking characters of a word, packing characters into words, searching for patterns in a string of characters, and replacing patterns with other combinations. In data processing, a list can be changed to a set of ranks or sorted by size. Lists can be treated as a two-dimensional matrix, and a set of matrix operations is available. Routines are available for common statistical operations on a list, such as

109 The first version of ELIZA, the early and famous computer program for the study of natural language communication between man and machine, was written in SLIP; cf. [Weizenbaum, 1966].

110 See [Sakoda, 1965c, 32] and [Sakoda, 1968, 303]. The memory organization of SLIP is completely different from DYSTAL. [Sakoda, 1968] contains a discussion of the pro and cons of Sakoda's approach.

111 "SAL" was probably an acronym for storage allocation.

112 Sakoda had some assistance in the development of DYSTAL. On the first page of the *DYSTAL Manual* Sakoda is listed as the director, Robert H. Cohen as programmer, and then follow as staff William E. Feinberg, Peter A. Morrison, Owen T. Thornberry Jr., and Dianne J. Mathews.



Sakoda Develops New 'Language' For Computers

A two-week institute on DYSTAL, an advanced computer programming language, will take place at the University beginning Monday, July 6.

The institute will be conducted by James A. Sakoda, professor of sociology, under a grant from the National Institutes of Health. Sakoda is the developer of DYSTAL, which stands for "dynamic storage allocation language."

The institute will combine lectures and practice sessions on the Computing Laboratory's IBM 7070 computers. It is for a limited number of scholars and businessmen already familiar with FORTRAN (Formula Translating System) II, the programming language on which Sakoda's new language is based.

Because of the large amounts of data involved in their research, members of the department of sociology and anthropology are among the heaviest users of the Computing Laboratory. With Sakoda's new language, complex processes such as social behavior within a small group or migration within a country can be simulated within the computer.

Figure 20: *The Brown Daily Herald,* Saturday, May 30, 1964, page 5.

> taking a sum, sum of squares, sum of cross-products, variance, and mean and standard deviation. Boolean operations have also been written to permit combining of Boolean functions, using AND, OR, or NOT. Obviously, many other routines can be written; it is expected that users of DYSTAL will make contributions of their own [Sakoda, 1965c, 35].

Reading that quotation one can't but think of DYSTAL as an early version of NETLOGO. In a more general perspective, DYSTAL is meant to be the chance of doing computational social science while "*avoiding* the middleman—the professional programmer" [Sakoda, 1965c, 35, emphasis added].[113] Obviously, that is the same intention as the one that drives the development of NETLOGO. However, and that is important to note, it is a NETLOGO *without* any graphics and graphical output routines.

Additionally, Sakoda did a kind of advertising. He published a two-pages description of DYSTAL in *Behavioral Science*. At the end of the brief article he refers to the manual, "a limited number of which are available for distribution". He then offers again a two-week summer institute to learn DYSTAL at Brown University in August, 1965 [cf. Sakoda, 1965a, 183]. In 1967 Sakoda presented DYSTAL at a conference on symbol manipulation languages in Pisa [cf. Sakoda, 1968]. There, right at the beginning, he makes an ambitious claim:

> DYSTAL attempts to achieve the status of a universal programming language by adding to FORTRAN dynamic storage allocation and features of list-processing and string-manipulation languages [Sakoda, 1968, 302].

[113] It meant to eliminate the kind of problems that Schelling later had at RAND: "I quickly learned something crucial: programmer and experimenter must work closely, the former understanding what the latter wants, the latter understanding how programs work" [Schelling, 2006, 1642]. More details follow in section 5.3 below.

Sakoda and Robert G. Potter Jr. (a demographer and colleague from Brown University) used DYSTAL to develop FERMOD, a simulation model of human reproduction [Potter and Sakoda, 1966]. A technical description of FERMOD is given in the extended DYSTAL manual [Sakoda, 1965b, ch. xxii]; the FORTRAN listings are available in an appendix to the manual.[114] The model was "designed to follow the changing distribution of children ever born and birth intervals of a large homogeneous population of couples as they move through the reproductive period" [ibid. 450]. FERMOD was *not* an agent-based model. It was based upon expected values: it assumed a population of 10 million couples and then calculated the monthly fraction of women that conceive, miscarry, give birth etc. by multiplying the numbers of women in a category by an appropriate probability [cf. ibid. 452]. In two follow-up articles, [Potter and Sakoda, 1967] and [Potter et al., 1968], FERMOD is used to discuss family planing policies.[115]

Sakoda used DYSTAL (and the lessons he had learnt by developing it) to write a statistical package XTAB9.[116] At that time SPSS started to become predominant. But it was slow and could be used only on major machines. In a co-authored article (with Sakoda as the first author) XTAB9 ("XTAB" seems to be an "abbreviation" for cross-tabulation while the "9" is a version number) is described as a competitor of SPSS:

> The chief virtue of XTAB9 is its small size and portability. It was developed by Sakoda on a 32K byte IBM 1130 system using Basic FORTRAN IV. It is a miniature system, compared with SPSS, but still providing for data conversions, lables for tables, six-way cross-tabulations, means, standard deviations, and correlation coefficients. It achieves efficiency through use of overlays, dynamic storage allocation, virtual memory, and two-byte integers. The most recent addition to it has been the ability to process a variety of hierarchically-structured files, including public use sample files and others structured in a similar fashion. It provides for creation or merged records as well as aggregation and tabulation of information from subrecords [Sakoda et al., 1978, 94].

The photograph in figure 21 depicts Sakoda in front of "his" IBM 1130. Sakoda invested a lot of time and effort in the development and maintenance of DYSTAL. He wrote several versions of his central DYSTAL application XTAB9. On his horizon was already a kind of DYSTAL user and developer group. In 1970, DYSTAL II was released that made arrays relocatable [Sakoda, 1970]. It was delivered on a 200-foot magnetic tape with the FORTRAN source code together with a new manual [cf. Sakoda, 1974b,

[114] In a footnote we find some DYSTAL advertising: "The manual can be obtained from Dr. Sakoda. The reproduction cost is $3" [Potter and Sakoda, 1966, 454 fn. 14].

[115] Sakoda co-authored two more article on contraceptive effectiveness, namely [Potter et al., 1970] and [Potter et al., 1972]. Both employ rigorous statistics. But FERMOD does not play a role. DYSTAL may have been used for some calcuations, but is not mentioned.

[116] For the history of XTAB9 see [Sakoda, 1979, 78f.]. It may be a version of the program documented in the *XTABS User Manual* [Sakoda, 1977b]. But I could not find any copy of a XTAB9 or a XTABS manual. The existence of XTAB9 is verified by being mentioned in the two publications [Sakoda et al., 1978] and [Sakoda, 1979]. "XTABS" may be a typo: I found it only in Sakoda's (unpublished) CV and the list of publications that is included therein [Sakoda, 1982a, 3]. See for further information footnote 147.

Figure 21: Photograph from [Hansen, 1994, xxxiv]. There Hansen's caption is: "This photograph of Dr. James Sakoda, taken sometime in the 1970s in the computer room at Brown University, depicts Professor Sakoda operating an IBM 1130 computer and illustrates his joint interest in computers and origami". Photo courtesy of Center for Oral and Public History, California State University, Fullerton

479]. One had to contact Sakoda to buy the manual ($10) and the tape ($100) [cf. Sakoda, 1979, 90].

4.2 Sakoda's Checkerboard Model in Early Introductions to Simulation and Modeling

Like Schelling, Sakoda had started his checkerboard modeling as a table-top exercise. But by 1971 his checkerboard model of social interaction was a *computational* model: it was programmed in Basic FORTRAN IV,[117] and ran on an IBM 1130 [cf. Sakoda, 1971, 119 fn.]. For unknown reasons the version of the FORTRAN program that Sakoda used for his 71-article, was *not* written by Sakoda himself. It was written by his son, William James Sakoda, as we know from a footnote in that article.[118] Obviously, the reason for that can't be a lack of programming competence on the father's side—Sakoda's development of DYSTAL proves his excellent command of FORTRAN. Sakoda's son William James became a computer scientist. Maybe the father considered the programming of the checkerboard model as a nice exercise for the son.

In 1977, Sakoda's computerized model found its way into Richard S. Lehman's (1939–2004) book *Computer Simulation and Modeling: An Introduction*. The first sentence of the preface states that the book is meant to be an "introductory book about computer simulation and modeling in the social and behavioral science" [Lehman, 1977, ix]. In the 1970s the literature on computer simulation was growing rapidly. But, as Lehman reports, "no book has been written for the social scientists who wish to write a simulation but cannot conceive of how to begin. This books attempts to fill that need" [Lehman, 1977, ix]. In that book, Sakoda's checkerboard model—now called CHEBO[119]—has a very prominent status. It is one of two examples that serve as paradigms throughout all the chapters on planning a project, flowcharting, data structures, problems of coding, efficiency, accuracy, and validation etc. The other paradigm model is SIMSAL, a model of learning paired-associates. A third model, called CITY, is dealt with in detail, but in an appendix only. The model is about urban development, and has nothing to do with Schelling's model of residential segregation. Schelling is *not mentioned at all* in this book—a book which appeared six years after Schelling's *JMS* article and eight years after his RAND memorandum.

For access to the *original* program code that was used by Sakoda for his *JMS* article, Lehman's book is a piece of luck. In an appendix we find a complete listing of the FORTRAN code of CHEBO [Lehman, 1977, 256-293]. Lehman made some minor modifications in the code. He describes them as follows:

[117] Except for the use of DATA statements [cf. Sakoda, 1978, 366].

[118] Sakoda reports: "The most recent version of the program was written by William J. Sakoda" [Sakoda, 1971, 119 fn.]. At the beginning of the 1970s, there was a scientific collaboration of Sakoda and his son. Together they published [Sakoda and Sakoda, 1972]. William James Sakoda was educated as a computer scientist.

[119] That was probably already the name of the FORTRAN program used for Sakoda's 1971 *JMS* article. More about the programcode below. Cf. the first line of the code in [Lehman, 1977, 256]. Sakoda himself refers to the checkerboard model as CHEBO in [1978].

> The program was written in ANSI Fortran by William J. Sakoda and modified slightly by myself. The modifications consisted mainly of editing to the specifications used in this book, changes in routine names, and removal of materials specific to the IBM 1130 on which it was originally run. This version of the program was compiled and executed on the Univac 70/46 computer system at Franklin and Marshall College [Lehman, 1977, 254].

From comments at the head of the code we learn that a capability for difference in speed of movement was added to the version of the *JMS* publication from 1971. It looks as if that extension was programmed as well by William J. Sakoda [cf. Lehman, 1977, 256]. In sum: At least something very close to the original program that Sakoda used for his *JMS* article was published and is still accessible.

In 1978, Sakoda's model had another prominent placement in the edited volume *Computer Science in Social and Behavioral Science Education* [Bailey, 1978]. The volume contains 35 contributions, results of three NSF-funded summer workshops that were held in 1974. In the opening sentences of his introduction Bailey writes that "the coverage of this volume . . . is representative of the main stream of current developments" [Bailey, 1978, ix]. Obviously, in 1978 Sakoda was considered to be an important part of that stream.

Sakoda contributed two articles to Bailey's collection. He is the first author (out of four) of a lead paper for the book's section *Statistics and Methodology*. The article *Social Science Data Processing: An Overview* starts with the observation, that "the bulk of computer usage in the social sciences is in data analysis, rather than in areas such as model building" [Sakoda et al., 1978, 81]. The four authors describe and discuss different languages and social science statistics packages, their pros, cons, and hardware requirements. (It is *this* article from which we quoted above the short description of XTAB9 on p. 66.)

Sakoda's second article is titled *CHEBO: The Checkerboard Model of Social Interaction* [Sakoda, 1978]. Now written under a more didactical and educational perspective, it is is a slightly modified and slightly extended version of Sakoda's 71 article and model. The extension regards the above mentioned capability for a difference in speed, realized by "migrations windows" of different size for different groups.[120] This article is the only time that Sakoda writes some lines about more convenient input and output devices for a more "playful" analysis of his checkerboard model:

> CHEBO, as it is presently written, uses cart input and printer output and is written for batch processing.[121] A student once wrote a graphics terminal version of the checkerboard model, but it has not been put to general use because of the scarcity of graphical terminals. A more feasible project is the implementation of the program on a typewriter terminal with a video screen display. This will allow students an opportunity to explore and examine changes in social patterns immediately [Sakoda, 1978, 366].

[120] Sakoda interprets differences in speed as differences in *power* [cf. Sakoda, 1978, 362, 364f.].

[121] CHEBO consisted of about 863 source cards [cf. Sakoda, 1978, 366]. Footnote by R.H..

From today's point of view, it would not come as a surprise to find an article on Schelling's models of segregation in Bailey's book—a book with the programmatic title *Computer Science in Social and Behavioral Science Education*. But again, Schelling is *not* discussed or mentioned in any article of the book. And even worse: the book includes a *Bibliography of Social Science Computing*, compiled by Ronald E. Anderson, with some 550 titles [Anderson, 1978].[122] In the years between 1969 and 1978 Schelling had published at a collection of articles on his family of models of segregation. But *none* of them is included in Anderson's bibliography. At the same time Anderson had a good overview. In his introduction Bailey writes that Anderson "has been a leader in the field of computer applications development in the social sciences over the last years" [Bailey, 1978, xii]. Before and after 1978, Anderson wrote a large number of articles (often highly informative surveys) on computer applications and the use of computers in the social sciences and related problems. As a member and consultant of many committees and institutions that in these days were dealing with the "computerization" of science, education, and society in general, he had—besides of overview—impact, and influence on the ongoing development. In later years Anderson got several rewards for his outstanding and pioneering work.[123] In sum, we have to look at Anderson as a person that was very well informed about what was going on in the evolving field of computational social science. Probably he has been one of the representativs with the broadest overview.

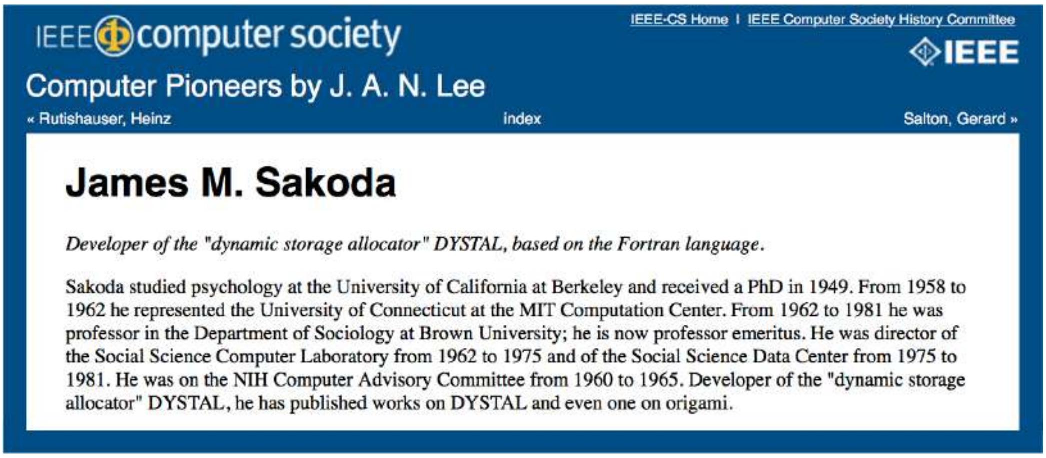
In 1980, Anderson published a short survey on computer simulation games. The peg of the article is that the *boards* of parlor games had inspired the development of simulation games for serious learning. Several examples are described. One of them is Sakoda's CHEBO. A picture illustrates CHEBO runs [cf. Anderson, 1980, 41]. From the article we learn that CHEBO could be bought for $15. To contact Sakoda, his address is given in a footnote. Schelling and his model is *nowhere mentioned*. 1981, ten years after Schelling's and Sakoda's *JMS* article had appeared, and three years after the publication of Schelling's *Micromotives and Macronehavior*, Anderson published a new survey, now on *Instructional Computing in Sociology: Current Status and Future Prospects* [Anderson, 1981]. Sakoda's article and model is mentioned and recommended among the topics and materials for possible courses [ibid. 188, 193]. But again: *Schelling does not exist*.

Obviously, by the middle of the 1960s, Sakoda was an experienced computational social scientist with an excellent command of FORTRAN, working on different projects in the field that we call now *computational social and behavioral science*. He was well recognized, embedded, and a leading figure in the evolving, though still small, network of scientists working in that field.

There is even more evidence for this. A founder and editor of a new journal thinks a lot about whom and what to include in the very first issue—it is "a statement".

[122] The bibliography is an update of a bibliography that he had published earlier in a special interest group publication of the Association for Computing Machinery (ACM); cf. [Bailey, 1978, xix].

[123] In 2001 the special interest group *Computers and Society* of the Association for Computing Machinery (ACM) awarded him the Outstanding Service Award. In 2012 the Communication and Information Technology Section of the American Sociological Association honored him with the William F. Ogburn Lifetime Achievement Award. Cf. Anderson's Wikipedia entry at `https://en.wikipedia.org/wiki/Ronald_Anderson`.



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Computer Pioneers by J. A. N. Lee

« Rutishauser, Heinz   index   Salton, Gerard »

James M. Sakoda

Developer of the "dynamic storage allocator" DYSTAL, based on the Fortran language.

Sakoda studied psychology at the University of California at Berkeley and received a PhD in 1949. From 1958 to 1962 he represented the University of Connecticut at the MIT Computation Center. From 1962 to 1981 he was professor in the Department of Sociology at Brown University; he is now professor emeritus. He was director of the Social Science Computer Laboratory from 1962 to 1975 and of the Social Science Data Center from 1975 to 1981. He was on the NIH Computer Advisory Committee from 1960 to 1965. Developer of the "dynamic storage allocator" DYSTAL, he has published works on DYSTAL and even one on origami.

Figure 22: Sakoda, the computer pioneer [Lee, 1995]. `http://history.computer.org/pioneers/sakoda.html`

The best explanation for Sakoda's *The Checkerboard Model of Social Interaction* being published in the very first issue of *JMS* is Sakoda's reputation. At Brown University he was the director of one of the earliest centers for computational social science, the Sociology Computer Laboratory (1962–1975), and later of the Social Science Data Center (1975–1981). In 1999, J. A. N. Lee published his book *Computer Pioneers*. Lee considered pioneers to be those "who introduced a new element, concept, or direction to the field" [Lee, 1995, 1]. There are about 300 pioneers in Lee's Hall of Fame, among them Noam Chomsky, Donald Ervin Knuth, John von Neumann, or Herbert A. Simon. On page 599 Sakoda is honored with an entry as a computer pioneer (see figure 22). By contrast, Schelling was neither a member of the network of computational social scientists. Nor, as it seems, was he known in that field. Schelling and his model remained unknown for long even after he had presented his family of models in a series of publications in the 1970s.

But one generation, three or four decades later, the situation was *reversed*. Sakoda had become an unknown pioneer while Schelling was upgraded to an early pioneer of computational agent based modeling, who had invented the paradigmatic Schelling model. The problem is exhibited in the following sample of introductory and overview literature on social simulation and agent-based modelling, which, incidentally is all very useful, thoughtful, and carefully written: In their book *Agent-Based Modeling. Modeling Natural, Social, and Engineered Complex Systems with NetLogo*, Wilensky and Rand devote a chapter to Schelling's model [Wilensky and Rand, 2015, 128ff.] acknowledge his pioneering role [cf. ibid. 432].[124] Sakoda is not mentioned. The

[124] Wilensky and Rand describe and discuss in their book what they call *The Segregation Model* [2015, 128ff.]. That model is since long one of the models in the very helpful *Models Library* of NetLogo (copyright 1997 Uri Wilensky). The description there (under the *info* tab) says that "this project was inspired by Thomas Schelling's writings about social systems (such as housing patterns in cities)". The phrase "inspired by" has to be taken seriously in the sense that the model is definitely *not* a correct implementation of Schelling's two-dimensional spatial model as described in [Schelling, 1971a, 154ff.]. The main reason for that are the *different migration regimes* in both models. As the description and the code of Wilensky's model consistently show, discontent agents make a *random* move to an empty cell.

same holds for Railsback and Grimm's, *Agent-Based and Individual-Based Modeling. A Practical Introduction* [cf. 2012, ch. 22.2].[125] In Squazzoni's *Agent-Based Computational Sociology*, Schelling is among the "founding fathers" of agent-based computational sociology [2012, 2]. And he is the most cited author of the book.[126] Sakoda is mentioned in one footnote. It is in a context in which Schelling's model is discussed as a means for a better understanding of micro-macro relations. The footnote says: "This was also the intuition of the neglected Sakoda's checkerboard model" [Squazzoni, 2012, 5 fn.]. Gilbert's *Agent-Based Models* describes, illustrates, and discusses Schelling's model as his very first example of agent based modeling [Gilbert, 2008, 6ff.]. In brackets, after references to [Schelling, 1971a] and [Schelling, 1978], one finds the remark "see also Sakoda, 1971" [ibid. 6]. In *Complex Adaptive Systems: An Introduction to Computational Models of Social Life*, Miller and Page use (and extend) Schelling's model as "a classic 'computational' social model" [Miller and Page, 2007, 143].[127] There isn't any hint to Sakoda in the bibliography. The book *Generative Social Science: Studies in Agent-Based Computational Modeling*, authored by Epstein together with a huge number of co-authors, refers to Schelling's "pioneering models of racial segregation" [Epstein, 2006, 16], mentions Schelling every now and then, but does not know Sakoda. Also in 2006, exactly 35 years after Sakoda's and Schelling's *JMS* articles were published, the *Handbook of Computational Economics* appeared. The second volume is subtitled *Agent-based Computational Economics*. In the author index we find twenty-four references to Schelling and one to Sakoda, who is mentioned in Vriend's article *ACE Models Of Endogenous Interactions*.[128] The first

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By contrast, in Schelling's model discontent agents move to "the nearest spot that surrounds him with a neighborhood that meets his demand" [Schelling, 1971a, 155]; see also page 9 above. In Schelling's original model there is some randomness, but it regards the *order* of the moves, not the locations (maybe except for cases that there is more than one nearest spot that meets the demand). It is an open question whether or not the two quite different migration regimes lead to similar results. If so, that would be an interesting finding. A correct and direct NetLogo implementation of Schelling's model is much more complicated than Wilensky's model, since at all times all discontent agents have to identify and to evaluate all alternative locations within a migration window of a given size (3×3, 5×5, etc.). (An efficient way to avoid a lot of redundant calculations that slow down the model dramatically, is, to let empty cells calculate their attractiveness and, then, to correct these values if, and only if, some movement changed an empty cell's neighborhood. Agents, then, do not calculate rather than "see" or "read" the attractiveness of cells as one of the cells' properties. I learnt this trick from Daniel Mayerhoffer. It makes a correct NetLogo implementation of the Schelling model a relatively easy task even for a beginner. At the same time, the beginner learns an important lesson: sometimes a more indirect programming approach makes things much easier.) In the last years I noticed at several occasions that people take the easily readable NetLogo code of Wilensky's segregation model as a short and nice description of the Schelling model—no need to seriously read Schelling's original article.

[125] The authors describe Wilensky's segregation model and note correctly that it "was inspired" by Schelling's model [ibid. 278]. As described in footnote 124 above, different from Wilensky's segregation model, in Schelling's model discontent agents do *not* make random moves to empty cells. In their ODD (Overview, Design Concepts, Details) description Railsback and Grimm motivate Wilensky's random moves with the argument, that "modeling the details of movement is unnecessary for this model" [2012, 279]. But the question is: How do you know? At least Schelling did not resort to random moves.

[126] Namely on 31 pages of the book, and up to six times per page.

[127] The authors claim to describe Schelling's model and write that "when the number of same type neighbors falls short of an angent's threshold, it randomly relocates to a new spot on the board" [Miller and Page, 2007, 144]. That is simply wrong (cf. footnote 124 above).

[128] The author index of the *Handbook of Computational Economics* distinguishes *Schelling, T.* and

example in Vriend's survey is Schelling's model. A footnote refers to Sakoda "for a very similar model of endogenous interactions" [Vriend, 2006, 1052 fn.]. *Simulation for the Social Scientist*, authored by Gilbert and Troitzsch, appeared in two editions, namely [1999] and [2005]. Schelling's model is discussed in both. But there isn't any hint to Sakoda. Epstein and Axtell, the authors of *Growing Artificial Societies: Social Science from the Bottom Up*, report that "the first concerted attempts to apply, in effect, agent-based computer modeling to social science explicitly are Thomas Schelling's" [Epstein and Axtell, 1996, 3]. Sakoda does not exist. Existence in a hedging and misleading footnote, or no existence at all, that seems to be Sakoda's destiny. Only few get it right: Cioffi-Revilla's book, *Introduction to Computational Social Science. Principles and Applications* has a short chapter *History and First Pioneers* that explicitly mentions Sakoda's dissertation from 1949 for a pioneering modeling approach [Cioffi-Revilla, 2014, 274].[129]

These puzzling findings suggests a more general inquiry on the citations of Schelling's *JMS* article, citations of his *Micromotives and Macrobehavior*, and the citations of Sakoda's *JMS* article. Table 4 gives the figures of a *Google Scholar* citation search.[130] The results look bad for Sakoda: In the first five years after its publication, Sakoda's 71-article was not mentioned in any publication that is found by *Google Scholar*. After ten years, there are two, after twenty years there are three citations of Sakoda's article. Google Scholar has difficulties to find citations in older books, but in all likelihood, taking that into account will not change the figures significantly. Forty-five years after their publication, references to Schelling's 71-article outperformed Sakoda's article by more than one order of magnitude. And it makes a big difference even bigger, if we notice that Schelling's *Micromotives and Macrobehavior* with its fourth chapter *Sorting and Mixing: Race and Sex*, got another 6,440 citations up to 2016. In sum, Schelling got about 50 times more "checkerboard related" citations than Sakoda. Even worse, a growing number of Sakoda citations are hedging references, often close to an also-ran remark, that mislead the reader about Sakoda's priority as a checkerboard modeler.[131]

Schelling T. C.. I added up the number of references. *ACE* is an acronym for Agent-based Computational Economics.

[129] Cioffi-Revilla describes and classifies Sakoda's model as a cellular automata. The remark that the model in Sakoda's dissertation was "published in 1971 in the *Journal of Mathematical Sociology*" [ibid.], is misleading (see page 40ff. above).

[130] There are three databases that we could use for such a citation analysis: Web of Science (Thomson Reuters), Scopus (Elsevier), and Google Scholar. Google Scholar has the broadest coverage, but it is not exactly known what it covers. Because of a less well defined basis, Google Scholar suffers more than the other two data bases from what is known as *stray citations*—minor errors in referencing duplicate records for the same paper [cf. Harzing and Alakangas, 2016, 795, 802]. Another problem are phantom authors that Google Scholar may generate [cf. Jascó, 2010, 178ff.]. Thus, the use of Google Scholar is *not* uncontroversial. For contributions to that controversy cf. the references in [Jascó, 2010] and [Harzing and Alakangas, 2016]. In their case study *Google Scholar, Scopus and the Web of Science: a longitudinal and cross-disciplinary comparison*, Harzing and Alakangas demonstrate that a cautious use of Google Scholar as the data source makes a lot of sense [Harzing and Alakangas, 2016, 800ff.]. They distinguish five major disciplinary fields: humanities, social sciences, engineering, sciences, life sciences. Our comparisons are within *one* disciplinary field, namely social sciences. That makes them even less problematic. For our purposes the coverage should be as broad as possible, including old books and book chapters. Therefore, all in all, Google Scholar seems to be the best choice.

[131] This type of citations shows that mere citation figures may hide what really is going on. For an

year	Schelling-71	Schelling-78	Sakoda-71	$\sum_{Sch} / \sum_{Sak}$
1976	11		0	undefined
1981	30	24	2	27
1986	42	149	2	95.5
1991	73	355	3	142.7
1996	127	668	11	72.3
2001	277	1,240	32	47.4
2006	724	2,380	64	48.5
2011	1,650	3,920	128	43.5
2016	3,440	6,440	198	49.9

Table 4: Results of a *Google Scholar* citation search for Schelling's and Sakoda's 71-article in the *Journal of Mathematical Sociology*. The numbers are the aggregated citations in time steps of five years after publication (as found on June 16, 2012 and February 19, 2017). The right column is the ratio of Schelling-71 *plus* Schelling-78 citations to Sakoda-71 citations.

5 Resolving the Puzzle I: Computers, Displays, Networks and Communities

The puzzling pattern of reception, recognition, oversight, and ignorance asks for an explanation. And there is one: the resolution of the puzzle is an interplay of *three* components. We will discuss them in the following subsections.

5.1 Can the Model be Run Without a Computer?

Schelling's 71-model was run as a manual table-top exercise. Sakoda's 71-model was a *computational* model, run on an IBM 1130. That was not an accident. The reasons behind become clear when we compare in detail what we have or would like to calculate in the two models.

1. To evaluate a single location in Schelling's model, *only a* 3×3 *neighborhood* has to be considered. In Sakoda's 71-model the *whole* checkerboard world matters for the attractiveness of a single location.

2. In Schelling's model the evaluation simply requires the *counting* of neighbors and *comparing* the result with a threshold value. In Sakoda's model each evaluation is a summing up of products of distance weights times an attitude value. Even if we calculate and tabulate all possible distances and their weights in advance, we will have to *sum up a large amount of long real-valued products*.

3. Schelling proposes to analyze his model for different neighborhood preferences or different sizes of the two groups. Additionally, it is a very natural idea to

explanation of Sakoda's increasing citation figures past 1996 see also footnote 252.

analyze Schelling's model for other neighborhood definitions, e.g. 5×5, 7×7 etc. But even then, that is a *small* parameter space compared to the *huge* parameter space of possible attitude matrices, group sizes, and distance weights in Sakoda's 71-model. Sakoda himself notices that the set of three attitude values $\{+1, 0, -1\}$ allows to assign nine different attitude combinations to each group. Given that set of attitude values,

> there are 45 different combinations of attitudes, when both groups are assigned attitudes for play. Nine of these are symmetric, i.e. both groups are given the same set of attitudes so that the two groups cannot be distinguished by their attitudes [Sakoda, 1978, 363].

However, a restriction to the set $\{+1, 0, -1\}$ is arbitrary (and Sakoda himself uses other values as well). All in all, Sakoda's checkerboard model is more a major simulation platform with a corresponding "curse of dimensionality".

Schelling explicitly states that he ran his model as a "manual, table-top exercise" [Schelling, 1969a, iv]; Thomas Schelling was the *human* computer. The time that is necessary for such an exercise depends heavily upon the size of the checkerboard and the number of agents on that board. But if the numbers are about the same as in Schelling's original *JMS* article, then we are speaking only about minutes of human computing and a *single run* will be stable—at least in the sense that the degree of clustering will stay about the same.

With Sakoda's model it is different: Given the points 1.–3. above, and compared to Schelling, Sakoda's 71-model takes humans at least one, but probably two orders of magnitude more time to compute a single run for a world of about the same size (with regard to the checkerboard and the groups). As a consequence, for Sakoda's model we are now speaking about hours, if not a day—and that for just *one* single run. That together with the huge parameter space of Sakoda's 71-model, a parameter space in which a human computer could easily "spend" years to end up with a very "manageable" set of single runs, makes an important point obvious: A minimum amount of time is sufficient, to run Schelling's model as a manual table-top exercise. In Sakoda's case, already a single run requires a significant amount of time and effort, if done as as *manual* table-top exercise; an extensive comprehensive research on Sakoda's 71-model is practically impossible for a human computer—such research requires more than a human computer: it requires a *real* computer.

With Sakoda's 49-model it was different. It was actually run as a manual table-top exercise. As we have seen in section 3.2, it was easy to do so: no distance and distance depending weights were calculated, an overall evaluation of locations was consequently avoided. A questionable trick was used to cope with the simultaneous existence of positive and negative attitudes—even at the expense of a theoretical incoherence. And probably, Sakoda was aware of the predicament. Retrospectively, he writes in 1978 about the 49-model:

> It was difficult to take both positive and negative attitudes into account when playing without calculation of distances. Hence, one cycle was run

> with positive moves, and the next with negative moves. In making moves it was easy to make errors and it became apparent that the use of a computer was called for. [Sakoda, 1978, 361].

Another remark from 1989 sounds as if the distance based overall evaluation of locations was the direct motivation for the computerization of the model.

> Later the procedure was computerized to calculate moves *based on distances from all checkers* [Sakoda, 1989a, 282, emphasis added].

Different from Sakoda's 49-model, his 71-model does cohere with its theoretical background. Additionally, it is much more explicit and flexible, and makes attitudes to the driving forces in a very simple way—by all standards, an elegant solution. But this came at a cost: in practice, the 71-model was no longer tractable by a manual table-top approach. Research on, and with Sakoda's 71-model, required access *and* command of a computer.

5.2 Computing Without a Visual Display

Sakoda's 71-model was a computerized model, programmed in FORTRAN and implemented on an IBM 1130. That computer (see figure 23), introduced in 1965, was IBM's least expensive computer at the time and became popular in price-sensitive areas.[132] It was a 16 bit computer with a maximum of 32 Kbyte core memory.[133] The minimum price was $32,280, with a disk $41,230.[134] A careful look at the picture of an IBM 1130 in figure 23 reveals an important point. Something is missing—a screen. That is no accident, it simply reflects the state of the art at that time.[135] Today's usual computer-screen combination was not yet available. The standard output device was a printer. Another expensive though primitive drawing device might have been a plotter.

That had consequences: at the end of the 1960s or beginning of the 1970s neither Schelling's nor Sakoda's model could easily be visualized on a screen. One could program a printer to print patterns of circles and crosses (Schelling) or squares and pluses (Sakoda) in certain box on a piece of paper. But that gives a picture for just one period—not the "continuously" evolving dynamics. One could think of a flip-book. But that is not an inviting solution of the visualization problem, neither on the production side, nor on the "consumption" side.

For Schelling that was no problem. His model could easily be run without a computer as a table-top exercise. The corresponding moving of pennies and dimes on a piece

[132] There still exists a group of enthusiastic IBM 1130 veteran users that operate a web site at `http://ibm1130.org/`. On their web site one can download an IBM 1130 simulator that allows to emulate the decades old computer on 21st century hardware.

[133] A computer with 16 GB memory has 524,288 times more memory than the IBM 1130 from 1965.

[134] It exists a group of enthusiastic IBM 1130 veteran users that operate a web site at `http://ibm1130.org/`. On their web site one can download an IBM 1130 simulator that allows to emulate the decades old computer on 21st century hardware.

[135] A good guide to the technological state of the art "in the old times" until the end of the 1980s is [Brent and Anderson, 1990]. For a detailed history of computer graphics see [Ryan, 2011].

Figure 23: Sakoda's computer: an IBM 1130. Picture from: Computer Museum der Fakultät Informatik, Universität Stuttgart

of paper would *eo ipso* visualize the dynamics with a speed that was fast enough to be impressed by the evolving cluster structure. At the same time, the speed was slow enough to understand how the surprising macro effects were brought about by micromotives. Schelling knew that exactly. In his *JMS* article he states explicitly that "what is reported here has all been done by hand and eye" [Schelling, 1971a, 155]. There is *nowhere* an invitation to study segregation computationally. On the contrary, in 1974, in *On the Ecology of Micromotives*, Schelling warns *against* running his model on a computer:

> I cannot too strongly urge you to get the nickels and penies and do it yourself. I can show you an outcome or two. A computer can do it for you a hundred times, testing variations in neighborhoods demands, overall ratios, sizes of neighborhoods, and so forth. But there is nothing like tracing it through for yourself and seeing the process work itself out. It takes about five minutes – no more time than it takes me to describe the result you would get. In an hour you can do it several times and experiment with different rules of behavior, sizes and shapes of boards, and (if you turn some of the coins heads and some tails) subgroups of nickels and pennies that make different demands on the color compositions of their neighborhoods [Schelling, 1974, 48].

Explicitly Schelling recommends:

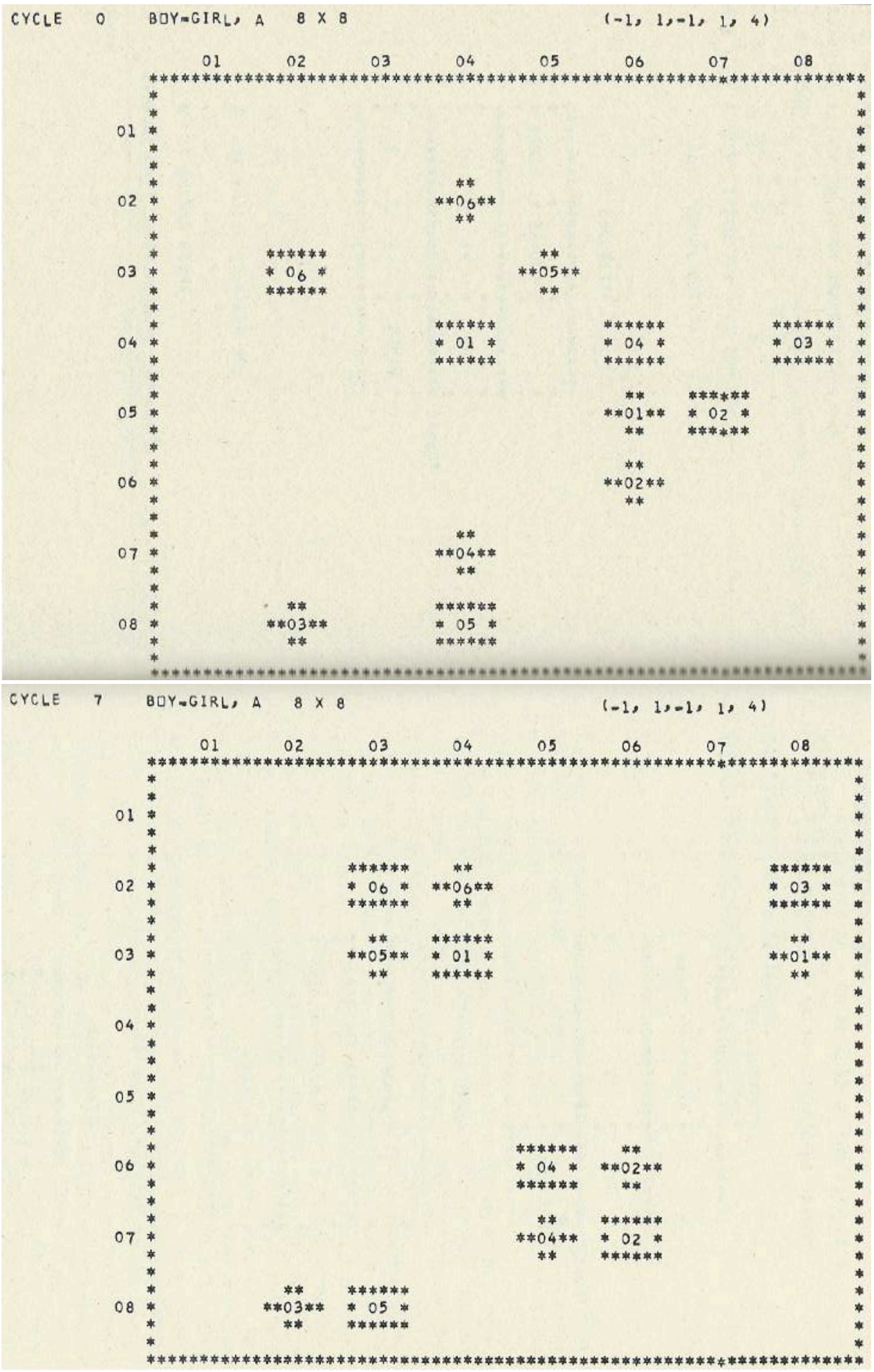


Figure 24: The line printer output for cycle 0 (*top*) and the final cycle 7 (*bottom*) of a boy-girl dynamics on an 8×8 checkerboard. The figures are from [Lehman, 1977, 196, 303].

> So if you have to choose, eschew the computer and do it on the coffee table [Schelling, 1974, 43].

That was a completely reasonable advice, given the very poor possibilities for the visualization of a dynamical graphical output. In a personal communication, Schelling writes:

> In those days computers—at least, those I had access to—could compute outcomes but could not display the dynamics. I could run various hypotheses and examine the results, but not watch the process work. I was glad that I'd originally done it by hand, because I could see how things worked. That's why I may have advised doing it by hand on a table, at least to do it that way for a while to watch how things worked [personal communication, July 23, 2012].

While the technological state of the art was *no problem* for Schelling's model, it was an *unsurmountable barrier* for a broader reception of Sakoda's 71-model. A recommendation to run the model as a table-top exercise would have meant to recommend the practically impossible. In practice an analysis and exploration of Sakoda's model required already a real computer only to do all the necessary calculations for all the evaluations of given and alternative locations. And even if that is done, there would still be the problem to visualize the resulting dynamics. An understanding of the dynamics would crucially depend upon the visualization. As a consequence, whoever was interested in Schelling's ideas about segregation and neighborhood formation, could immediately start his own experiments on the coffee table. Whoever was interested in Sakoda's more general ideas about attitude driven interaction dynamics had, first, to have access to a computer, had, second, to know how to use a computer, and had, third, to find ways and tricks how to cope with the visualization problem. Additionally, computing in those days required an enormous investment of time and energy. A print-out would require writing a program, producing punch-cards, and running the program in a certain time slot—often overnight—at a university computing centre, studying the error messages the next morning, debugging the program, producing new punch cards and so forth. As a pioneering computational social scientist, Sakoda had the necessary access to a computer, knew very well how to use a computer, and, obviously, was able to get a reasonable visual output. But, as we will see later in section 5.4, there were not that many others in such a position and with such capabilities.

5.3 Excursion: Schelling's *On Letting a Computer Help With the Work*

Both, Sakoda and Schelling, had started their modeling and simulation as table-top exercises. In his retrospect *Some Fun, Thirty-Five Years Ago*, Schelling tells a bit about the origins [Schelling, 2006]. It started "one afternoon, settling into an airplane seat, I had nothing to read" [ibid. 1641]. To amuse himself, using paper and pencil, he experimented with what later became the one-dimensional segregation model. He realized that a one-dimensional version would not take him very far. Back at home, and using his son's coin collection, he started to experiment. But with two dimensions he got into difficulties with the problem of "how to intrude a copper or a zinc into the midst of coppers and zincs" [ibid.]. It was *not* Schelling's idea to solve the problem by placing the coppers and zincs on a checkerboard and then to move them. Schelling reports to have mentioned his problem to Herb Scarf, who proposed to use a checkerboard and leaving enough blanc spaces on it to allow for movement [cf. ibid.].[136]

> So I made a 16 × 16 checkerboard, located zincs and coppers at random with about a fifth of the spaces blanc, got my 12-years-old to sit across from me at the coffee table, and moved discontented zincs and coppers to where there demands for like or unlike neighbors were met [ibid.].

That was done by hand. But somehow Schelling saw the potential of computers. There is in fact a remarkable and illuminating side story about Schelling and his view of computers. It was not noticed for a long time and became public only recently [cf. Hegselmann, 2012]. Despite his warnings about computers, he himself "unofficially" tried to work on his model with a computer in the early years. But there was a problem. Like almost all economists or social scientists in the late 1960s, Schelling was *computationally illiterate*. He "knew nothing about what computers could do, or how they did it", as he frankly states [Schelling, 2006, 1642]. So he needed a programmer's help. In the preface to his RAND-memorandum from 1969 that two years later, but *without* the preface, became his *JMS* article, Schelling writes at the end, that "John Casti has been preparing a versatile computer program that will shortly give me freedom to explore a wider variety of hypotheses" [Schelling, 1969a, iv].[137] That announcement had been a bit too early. The collaboration between Schelling and Casti did not work very well.[138] Schelling writes in a personal communication:

> John and I hadn't worked together and there were misunderstandings, mostly mine. (For example, people counted as their own neighbors, people at the edges miscounted.) I finally got a student of mine, James Vaupel,

[136] Herbert Eli Scarf (1930–2015) was a mathematician and economist at Yale University with a lifelong focus on equilibrium problems.

[137] John Casti, at that time working as a programmer at the RAND corporation in Santa Monica, reports his encounter with Schelling in [1994, 213f.].

[138] In later years John L. Casti published a number of books on modeling, simulation, and complexity. In 2002 his book *Mathematical Mountaintops: The Five Most Famous Problems of All Time, Oxford, 2001* was recalled by the Oxford University Press. Substantial parts of the book were obviously plagiarized [cf. Rothenstein, 2002]. In a second case Casti went even further: In 2001 he published under his name in *Complexity* an article that some months before had been published in *The Sciences* by Erica Klarreich. In a remarkable *Notice of Retraction and Apology* he writes in 2004:

> To the Readers of *Complexity*, I am writing to inform you that my article "Losing Games for Your Winning Plays" in the July/August 2001 issue of *Complexity* was plagiarized in its entirety from the article by Erica Klarreich, "Playing Both Sides" in the January/February 2001 issue of *The Sciences*. I apologize to the readers of *Complexity*, as well as to Dr. Klarreich and to the New York Academy of Sciences, publisher of *The Sciences*, for this action on my part [Schuster and Casti, 2004, 3].

> to teach me how to program the analysis in Basic. We spent a Sunday afternoon with a blackboard, and he taught me how to do it. He left for Europe the next day and I was on my own. But I had learned, and from then on I could run the thing on a computer [personal communication, July 23, 2012].

James Vaupel writes that Schelling was "a very quick learner" [personal communication to the author, July 27, 2012]. And Schelling himself says about the intensive course that he got that day:

> One of the exquisite learning experiences of my life occurred one Sunday afternoon when, for three hours in a room with a blackboard, Jim Vaupel completely disassembled my "model" into its smallest components and reassembled it before my eyes as a set of instructions that a computer could follow. (He sailed for Europe next day and I was on my own.) [Schelling, 1972a, 1f.][139]

The intensive course that Schelling got taught by James Vaupel had consequences: Schelling started to teach his students how to program his model. As a very good teacher, he wrote for his students a "guided tour" through a program of his model. The guide was mechanically duplicated under the title *On Letting a Computer Help with the Work* and appeared in November 1972 as No. 12 of the series *Teaching & Research Materials* of the Public Policy Program of the Kennedy School of Government of Harvard University (see figure 25).[140] It is a tour through Schelling's own computer implementation of the two-dimensional version of his model. (As it seems, the one-dimensional version was never implemented by Schelling.) On his guided tour, Schelling introduces the computer to the students in an amusing way: the computer is a "reliable high speed idiot" [ibid. 2]:

> I don't mean an ordinary idiot, I mean a "super-idiot". I mean an idiot with the characteristics that (1) he can perform a rudimentary task if he is told exactly what task to perform; (2) he can keep his place in an orderly list of instructions, doing the next task after he completes each one; (3) he will stop, and perhaps ring a bell, if the instruction is incomprehensible in his language or if the task he is told to do is unperformable; (4) he never makes a mistake or gets tired.
>
> His reliability derives from two characteristics: he never makes a mistake in doing exactly what we tell him to do; and he never thinks for himself [ibid. 4].

On his guided tour, Schelling starts with the basics of programming and computing in general (defining variables, types of commands, loops etc). Then the specific programming problems of a 2-dimensional version of Schelling's model are explained

[139] James Vaupel insists that it was a Saturday afternoon.

[140] In 2012, 40 years after the guide was written, Schelling's essay was made publicly accessible for the first time by JASSS: `http://jasss.soc.surrey.ac.uk/15/4/9/Schelling_Th_C_1972._On_Letting_a_Computer_Help_with_the_Work._JASSS.pdf`.

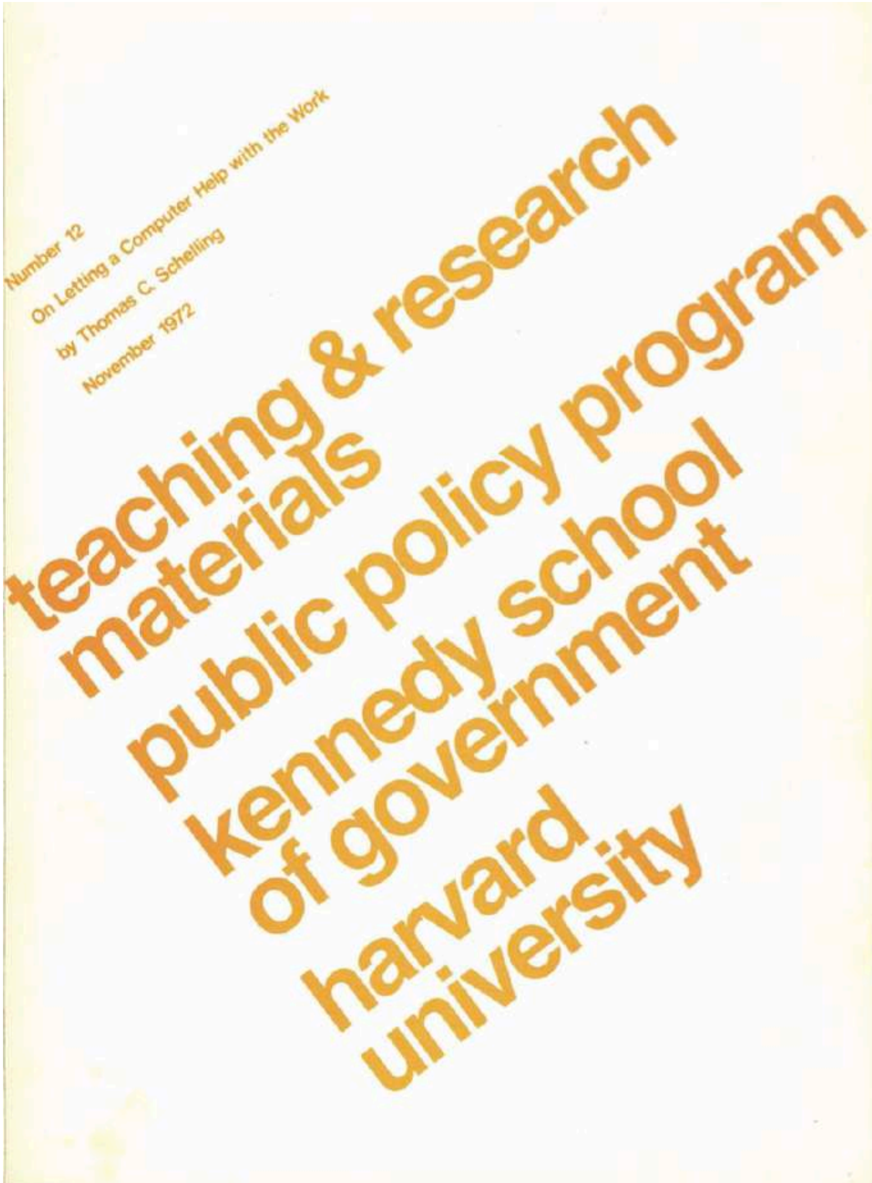


Figure 25: The cover of Schelling's *On Letting a Computer Help with the Work* [1972a].

and discussed in detail. But even in the context of his guided tour, Schelling stresses several times that experiments with his model can be run without a computer. His recommendation of the use of a computer is conditional:

> If, for this particular experiment, one had to choose between exclusive reliance on a computer and no computer at all, I would unhesitatingly recommend no computer at all. But if one had a hundred hours to devote to the experiment and a computer to help, I would recommend a few hours at the tabletop and ninety-odd hours in command of a computer [ibid. 3].

The main reason for Schelling's reservations with regard to the use of a computer are very clear in the guided tour. It is, first, the time that it takes to write the program, and, second, the visualization problem (as discussed in section 5.2). If the reliable, speedy idiot were a *real* idiot, who is physically moving dimes and pennies, then we could simply enter the room, observe the process, and eventually take a picture [cf. ibid. 33]. But the *super*-idiot of the year 1972 was connected only to a tele-printer. We could use the tele-printer to print the patterns. But again, code has to be written for the printer, and, at best, we get a sequence of pictures, not the dynamical process before our eyes.

In sum, a real computer was not necessary to get Schelling's impressive simulation results. Do-it-yourself table-top exercises with moving pennies (or aspirins, as Schelling alternatively suggested) on large sheets of papers simply generated the "better pictures". Using a computer would endanger understanding. And all things considered, doing the simulation manually was even faster. The guided tour in *On Letting the Computer Help with the Work* was written in 1972. At that time Schelling was perfectly right in his diagnosis; his conditional and cautious advise with regard to computers was completely reasonable. It would take another decade, and only then, in the 1980s, a computer screen became a standard output devise of affordable computers. In retrospect, in the year 2006, Schelling writes:

> Now that computers can display all the movements in "real time" there is, I suppose, little advantage in doing this kind of thing manually, but when I was doing it computers could compute but not display, and I often got computer results I could make little sense of until I worked it by hand [Schelling, 2006, 1644].[141]

That explains perfectly well, why in all the articles on his model in the years after 1969, Schelling did not mention any computer program. Consistently he warns against computers. Only very recently Schelling started to mention again the early computerizations of his model.[142]

[141] Richard Zeckhauser, Schelling's colleague at Harvard, reports:

> I remember well Tom playing with his model in his office, with actual coins on a checkerboard. He seemed endlessly intrigued by the possibilities [personal communication, April 15, 2017].

[142] Cf. [Aydinonat, 2005, 4].

5.4 Spreading of the Models in Different Communities and Networks

After their publication in 1971, Sakoda's and Schelling's model spread very differently. As described above in section 4.2, Richard S. Lehman, made Sakoda's model a paradigm example throughout his book *Computer Simulation and Modeling: An Introduction* [cf. 1977, ix]. That was in 1977. One year later, Sakoda himself contributed the article *CHEBO: The Checkerboard Model of Social Interaction* [Sakoda, 1978] to Daniel E. Bailey's edited volume *Computer Science in Social and Behavioral Science Education* [Bailey, 1978]. Additionally, in several articles the checkerboard model was described and sometimes illustrated by pictures. These publications will have spread some knowledge about Sakoda's model among people interested in the use and application of computers in social sciences. However, it is important to note that in the 1970s *no other* social scientists picked up Sakoda's checkerboard model for further research.

That had reasons: What Sakoda had presented was somehow the prototype of a simulator that allowed to analyze social processes driven by different possible patterns of attitudes. The power of the simulator was illustrated for some interesting attitude patterns. None of them was analyzed in detail. Sakoda's checkerboard model of social interaction was a kind of invitation to a major research project. Whatever the next steps would have been, they could be done only by computationally literate social scientist with access to computers and a fairly good command of computers. But in 1971 and the years to follow *there were not that many*. In his survey, *Instructional Computing in Sociology: Current Status and Future Prospects* from January 1981, i.e. ten years after Sakoda's and Schelling's *JMS* articles, Anderson notices:

> A number of simulations, e.g., Sakoda's (1971) checkerboard model and Levin's (1976) sociometric simulation, have been reported in the literature but due to technical problems have not been widely disseminated [Anderson, 1981, 185].[143]

There were not only few computationally literate social scientist, but in addition, early computational social science was predominantly computational *statistics*. There were, for instance, early forms of big-data projects, namely the statistical analysis of U.S. census data, that in the 1960s—with the help of computers—for the first time became really analyzable in practice. What was true for the whole field, was also true for the individual computational social scientists: predominantly, if not exclusively, they were computational *statisticians*. Sakoda was no exception.[144] And he went even further: With regard to the development of DYSTAL and XTAB9, Sakoda became what we have to call a *computer* scientist with a specialization on dynamical storage allocation, who tried to create and to improve *technical* preconditions for doing computational social science, be it statistics or modeling. In 1988, in the interview with Hansen, he says retrospectively:

[143] A personal communication (from August 5, 2012) suggests, that the technical problems that Anderson had in mind were related to the programing language and the problems with the graphical display of dynamical processes. As discussed above, even if the computer does all the calculations, that is little help if the resulting dynamics can't be displayed.

[144] See Sakoda's publications that we listed on p. 58 above.

> I went from social psychology to social statistics. I became more into the statistics, and then I went into computers. So that I still have a connection with the model, in terms of the computer. But I changed fields, actually, is what it is [Sakoda in the interview with Hansen, 1994, 418].

Sakoda's own modeling work—the checkerboard model and FERMOD[145]—does *not* seem to have been his foremost scientific activity. At least at that time technical and statistical issues had priority.[146] Sakoda's Curriculum Vitae lists his publications and his talks.[147] About half of his talks are about DYSTAL, the other half regards technical computational problems (e.g. "Use of Magnetic Tapes on the IBM 1130") or advanced statistical technicalities (e.g. "Cluster Centroid Factor Analysis"). Sakoda did some "campaigning" at around the 1970s, but it is a "technical" campaign for DYSTAL, *not* for the checkerboard model. There is just *one* talk on the checkerboard model, given 1977 during a meeting of the Association of Computer Users in Education.

In sum, only very few could take up Sakoda's model, and the few computationally literates that could do it, had predominantly an interest in computational statistics. Anderson, who wrote several survey articles on computational social science issues in the 1970s and 1980,[148] writes that at about the times of Sakoda's *JMS* article, in sociology "models interested about 50 people or less" [personal communication to the author, August 5, 2012]. At the same time the inventor of the checkerboard model of social interaction did almost nothing to spread his model and to make it accessible for computational novices.

With Schelling's model it was completely different. In 1969, with the publication of the one-dimensional version of his model in the *American Economic Review*, Schelling started a kind of "modeling-segregation-campaign". Besides the *JMS*-article, Schelling presented the two-dimensional version of his model in the same year in his article *On the Ecology of Micromotives* [cf. Schelling, 1971b, 82ff.].[149] Three years later, that article was reprinted as a book contribution [Schelling, 1974]. Another book contribution is Schelling's *A Process of Residential Segregation: Neighbourhood Tipping* [Schelling, 1972b]. Therein he analyzed segregation in depth by another member of his family of models, a model that he had only very briefly described in the *JMS* article. The underlying neighborhood concept is quite different from the self-centered neighborhood in Schelling's one- or two-dimensional segregation model that we discussed above.

[145] Cf. p. 65 above.

[146] See [Sakoda and Sakoda, 1972], [Sakoda and Karon, 1973], [Sakoda et al., 1974, revised in 1977], [Sakoda and Karon, 1974], [Sakoda, 1976], [Sakoda and Cochran, 1976], [Sakoda, 1977b], [Sakoda, 1977a].

[147] I got Sakoda's CV from Arthur Hansen, who got it from Sakoda in a letter from May 2, 1988 as part of the preparation for the interview [Hansen, 1994]. The last dated item in the CV is from 1982, namely the publication [Sakoda, 1982b]. Probably the CV was written or updated at about the date of the last dated item. Using author-year based keys to refer to the literature, I assigned the year 1982 to Sakoda's curriculum vitae and refer to it as [Sakoda, 1982a].

[148] For Anderson's overview see p. 70 above. Cf. [Anderson, 1978, 1980, 1981, 1983], [Brent and Anderson, 1990].

[149] The article is *not* a simple excerpt of the *JMS* article. The model is discussed as one of many other examples in the broader context of intentional individual actions and their unintended collective results.

(We will go into the details of that alternative model later in section 6.2). Probably there is at least a huge overlap between this book contribution and a paper that I could not access, Schelling's Harvard discussion paper *Discrimination Without Prejudice: Some Innocuous Models* [Schelling, 1972c].[150] The most important multiplicator of his model became Schelling's extremely readable and extremely successful 1978 book *Micromotives and Macrobehavior*. In chapter four (*Sorting and Mixing: Race and Sex*), Schelling reuses material and parts from his *JMS* article. In chapter three (*Thermostats, Lemons, and other Families of Models*) he does that as well, and, additionally, generalizes the model that he had analyzed before especially in [Schelling, 1972b].

In terms of citations, the book soon outperformed Schelling's *JMS* article. In 1986 Schelling's article had 42 citations; the book had 149. The catchy and memorable title *Micromotives and Macrobehavior* put the model into the perspective that then became predominant: the perspective in terms of micro-macro relations. Additionally, the title of the book coined the words and established the jargon that then became the more and more common micro-macro-terminology.

Putting the model into that perspective was nothing new for Schelling. He had that view on the model from early on. Already in the very first sentences of the preface to his RAND-memorandum, Schelling classified his work as an analysis of the relation of individual decisions to collective phenomena.[151] In the first sentence of that preface Schelling is very close to even *defining* economics as the study of micro-macro relations—though not yet using the micro-macro terminology:

> If anything characterizes economics, it is explicit analysis of the relation of *individual* decisions to *aggregate* phenomena. This method is much less common among sociologists and others who deal with the ecology of colors, religions or language groups. I discovered this in attempting to locate reading material for a class at Harvard. I found a great deal of attention to the motives and origins of individual behavior and to many collective phenomena that result from the interaction of individuals, but little explicit theory relating what goes on in the aggregate to what determines the behavior of the individuals who comprise the aggregate. So I have attempted some theory myself [Schelling, 1969a, iii].

Emphasis on the relation between individual decisions and aggregate phenomena, that is the *leitmotif* of the publication campaign, in which Schelling repeatedly presented his models of segregations from 1969 onwards. With the booktitle, *Micromotives and Macrobehavior* the keywords were found to describe the linking and interplay of players and layers that generate the unintended and often surprising results.[152] Schelling's model—counter-intuitive, illuminating, and scientifically stimulating as it was without requiring any computational expertise—slowly but surely became known to more and more scholars not only in economics, but in sociology, the social sciences in general, and in the general public. That was a very broad audience—not just, as in Sakoda's

[150] Neither the Harvard Archives, nor former colleagues that I approached, had a copy.

[151] The preface was *not* included and published in the *JMS* article.

[152] 1974 Schelling still refers to segregation as an aggregation effect; cf. [Schelling, 1974, 52f., 54].

case, a small and comparatively self-centered community of computational experts and social scientist that had to invest major amounts of time and effort to acquire, maintain, and develop further their programming and operating capabilities. Many in Schelling's much broader audience of computational illiterates will personally have done the table-top exercise that Schelling had recommended. They all had experienced how surprisingly fast, right before their eyes, certain unexpected, dramatic macro structures evolved, generated by fairly innocent looking micro-motives—an eye-opening phenomenon par excellence.

On the first half of the 1980s a technological and commercial breakthrough happened. The first mass-produced, affordable, and easy to use personal computers appeared and spread in business and science. The Apple II series, the Macintosh, and IBM's personal computer are paradigmatic instances. For input, these computers did not require punch cards. A keyboard and a mouse (Macintosh) was the convenient solution. Output was visible on a screen. The screen could display graphics—no longer tricky printing procedures or a plotter was necessary to get (static) graphical output. And, finally, the CPUs were fast enough to calculate interesting social dynamics and their ongoing graphical representation with a reasonable speed. Figures 26 and 27 show the opening pages of two articles, the first on the IBM personal computer, the second on the Apple Macintosh. Both articles were published in 1984 in *BYTE*, at that time an influential computer magazine with a wide-ranging editorial coverage.[153]

For Schelling's model fantastic times started. From now on, whoever knew about Schelling's model, was interested to join the computational turn and started to learn programming, will almost unavoidably have had the idea to start with Schelling's model. The following episode is a remarkable (if not curious) manifestation of the turning point: In 1990, Brent and Anderson published their well structured and very informative book *Computer Applications in the Social Sciences* [1990]. Sakoda's 71-model is included. An illustration shows 15 cycles of a run based upon the attitude pattern called social workers [ibid. 194]. A hint to [Lehman, 1977] with its detailed description of a FORTRAN implementation of Sakoda's model is given. But even now, in 1990, Schelling and his model is *nowhere* mentioned in the book. However, right after the description of Sakoda's model we read:

> Another independently developed computer simulation used similar principles to represent neighborhood segregation and desegregation. This small simulation program called "Neighborhood Segregation Model," or SEGREG for short, can be acquired from the National Collegiate Software Clearinghouse and was developed by Dethlefsen and Moody (1982) [ibid. 195].

An illustration of a SEGREG run is given on the following page. The reference in the quotation above is a reference to the article *Simulating Neighborhood Segregation*

[153] *BYTE* started in 1975 and appeared until July 1998. In the 1980s it was a must-read magazine for anybody who was interested in the development of software, hardware, and new application areas of computers, including scientific areas in the social and behavioral sciences. The magazine can be read in the Internet Archive at `https://archive.org/details/byte-magazine`.

The IBM Personal Computer

IBM has established itself as the nation's major microcomputer producer in the three years since it introduced the IBM Personal Computer in August, 1981. Drawing on a reputation for service and reliability with its larger computer systems, IBM has been able to quickly woo corporate America and even entice home users.

Catering to over a million IBM PC users, software publishers have created for the computer one of the largest bases of microcomputer programs, thereby contributing further to the PC's proliferation. The 16-/8-bit 8088 processor makes it possible for users to write larger, more complex programs that offer more sophisticated features yet are easier to use than previous 8-bit personal computer applications. The IBM PC's five expansion slots and the wealth of technical information available let numerous third-party vendors manufacture add-on boards that help the PC flex its productive muscle.

IBM has expanded its PC line and recently reduced its prices in an effort to grab an even larger market share by competing more aggressively with the IBM compatibles. An expanded line also means that IBM is competing directly with itself. Many people currently using larger IBM computers, such as the System 34 and the Displaywriter, will find their needs adequately filled by an IBM PC at a lower cost. The IBM PC, one of the company's most successful products, will account for 6 percent of IBM's revenues in 1984.

Name
The IBM Personal Computer

Manufacturer
International Business Machines Corporation
Entry Systems Division
POB 1328
Boca Raton, FL 33432
(800) 447-4700

Size
5½ by 20 by 16 inches; 25 pounds

Components
Processor: Intel 8088, 4.77-MHz; socket for addition of the 8087 math coprocessor
Memory: 40K bytes ROM, 256K bytes RAM standard, 640K bytes RAM maximum with memory-expansion card
Keyboard: 83-key layout with 10 function keys and numeric/cursor keypad; detachable with 6-foot coil cable; adjustable typing angle
Mass Storage: 360K-byte double-sided 5¼-inch floppy-disk drive
Expansion: Five expansion slots (one used by floppy-disk drive controller)

Software
Diagnostics, Microsoft cassette BASIC interpreter in ROM

Optional Hardware

64/256K-byte Memory Expansion Card with 64K bytes	$265
64K-byte Memory Module	$100
Monochrome Display	$275
Monochrome Display and Printer Adapter	$250
Color Monitor	$680
Color/Graphics Monitor Adapter	$244
Disk drive (5¼-inch floppy disk)	$425
Asynchronous Communications Adapter	$100
8087 Math Coprocessor	$230
Graphics Printer	$449
Personal Computer Cluster Adapter	$400
Personal Computer Cluster cable kit	$110
Fixed Disk Drive Adapter	$590
10-megabyte Fixed Disk	$1395
PC Expansion Unit	$2880

Optional Software

Personal Computer DOS 1.1 (with Advanced Disk BASIC)	$40
Personal Computer DOS 2.1	$65
Personal Computer Cluster Program	$92

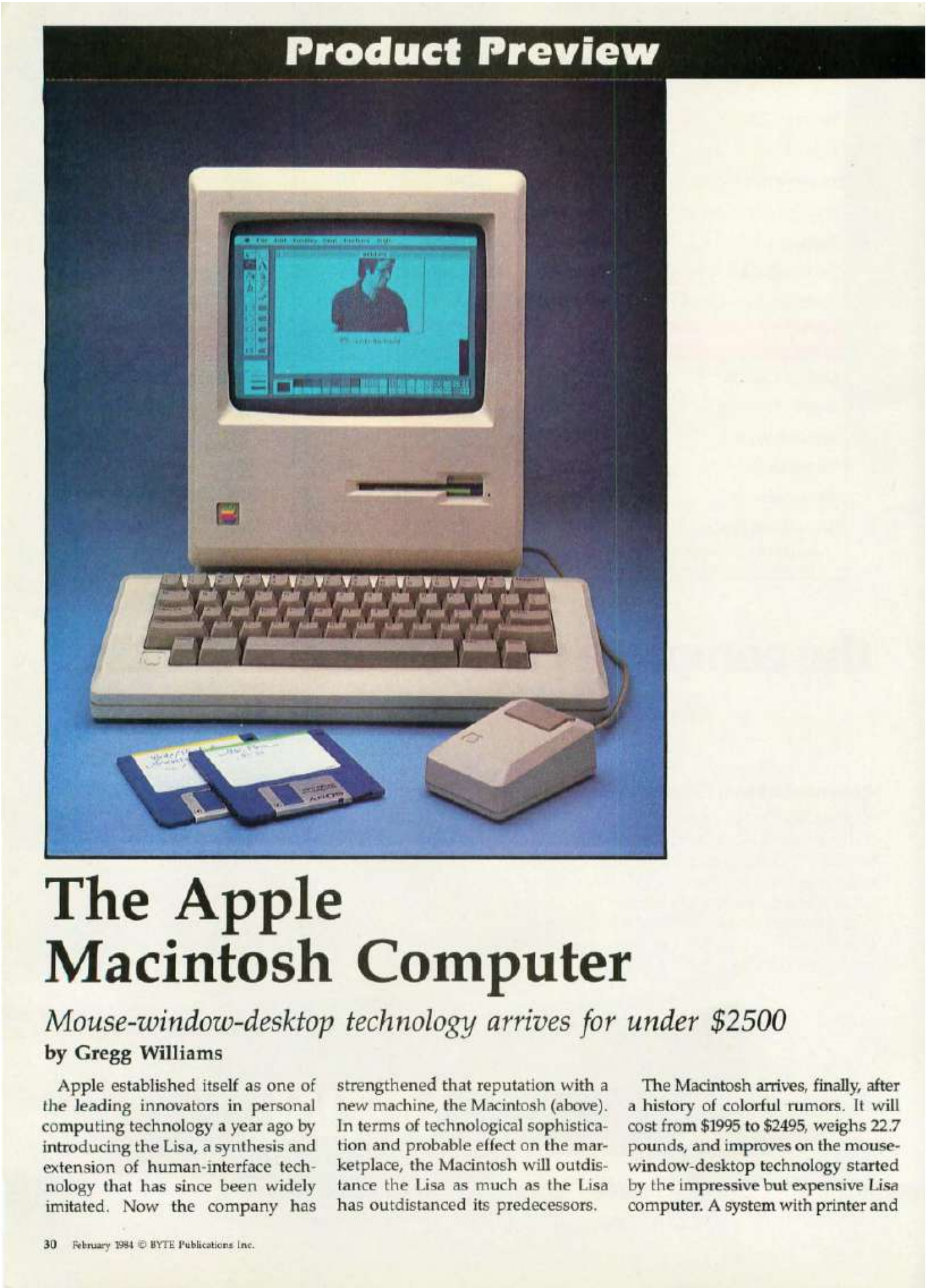
Documentation
Guide to Operations, BASIC Reference Manual

Prices

256K bytes of RAM and one floppy-disk drive	$1995
256K bytes of RAM and two floppy-disk drives	$2420

10 • BYTE Guide to the IBM PC • Fall 1984

Figure 26: The IBM Personal Computer [*BYTE*, special IBM issue, October 1984, `https://archive.org/details/byte-magazine-1984-09`].



Product Preview

The Apple Macintosh Computer

Mouse-window-desktop technology arrives for under $2500

by Gregg Williams

Apple established itself as one of the leading innovators in personal computing technology a year ago by introducing the Lisa, a synthesis and extension of human-interface technology that has since been widely imitated. Now the company has strengthened that reputation with a new machine, the Macintosh (above). In terms of technological sophistication and probable effect on the marketplace, the Macintosh will outdistance the Lisa as much as the Lisa has outdistanced its predecessors.

The Macintosh arrives, finally, after a history of colorful rumors. It will cost from $1995 to $2495, weighs 22.7 pounds, and improves on the mouse-window-desktop technology started by the impressive but expensive Lisa computer. A system with printer and

30 February 1984 © BYTE Publications Inc.

Figure 27: The Apple Macintosh [*BYTE* February 1984, https://archive.org/details/byte-magazine-1984-02].

[Dethlefsen and Moody, 1982], published in *BYTE*. In that article, Dethlefsen and Moody describe and discuss their computer implementation of *Schelling's model*.[154] The program is written in BASIC; the article includes the complete code. Dethlefsen and Moody omit a reference to Schelling's *JMS* article. As many others later, they refer to the shortened version in *Micromotives and Macrobehavior*. Thousands of readers of *BYTE* will have come across the article. It wasn't a mass movement, but many—and not only by reading *BYTE*—knew Schelling's model, and many started their own programming with it.[155] More than a few modified or extended the model. As a result, Schelling's model became a classical model in agent-based modeling and a kind of standard appetizer in the first introductory courses to computational social science.

In principle it could have been ideal times for Sakoda's model as well. However, there was a *fatal* problem. There were all the students and scholars, comparatively young for the most part, that now—inspired or "seduced" by the new and revolutionary technical possibilities—turned to computational social science as one of their subjects. *But how should they know about Sakoda's model?* Sakoda's model had been presented to the public ten to fifteen years ago in an article that basically had no resonance at all. Sakoda was known, present, and well recognized in the "old" and small network of computational social scientists. But the beginners in computational social science *after* the technological and commercial breakthrough of computers in the early 1980s, did *not* start with reading the early and old literature on computation in the social sciences. They were *not* motivated or "initiated" to the discipline by personal or educational links to members of the computational social science network that existed already in the 1960s. The new generation of computational social scientists somehow got their first computer, started on their own to use it, and applied their new equipment to the problems that they had, knew, or that—in a kind of "attention dynamics"—very soon attracted the attention of many others in the evolving community of computational modelers: cellular automata (socially interpreted in this or that way), the game of life, the evolution of cooperation, learning mechanisms of all sorts, opinion dynamics, or, last, but not least, Schelling's models of segregation.

Thus, as to Sakoda's model, all things considered, a sad and simple diagnosis suggests itself: when Sakoda's time had come, when the skills and technical equipment that were necessary to realize his project, were really there, his research program was already forgotten. Sakoda's model was simply not known and did not get known to the new generation that now was well equipped to start the research on it. Sakoda had published his article ten to fifteen years *too early*. Maybe that life punishes those that are late. But sometimes it punishes those that are early as well.

[154] Dethlefsen and Moody modify (and thereby simplify) Schelling's neighborhood definition. They do *not* use the 3×3 neighborhood, i.e. the so called Moore neighborhood. Instead, neighboring cells are the four directly adjacent cells in the north, south, east, and west. That is the so called von Neumann neighborhood.

[155] As it happened to the author.

6 Resolving the Puzzle II: Schelling—a Beneficiary (and Victim) of the Matthew-Effect

The differential recognition of their respective models raises the question whether, or in what sense, the Schelling-Sakoda case is also an instance of what Merton baptized as the *Matthew effect*. In his seminal paper with the same title [1968] Merton writes:

> Eminent scientists get disproportionately great credit for their contributions to science while relatively unknown scientists tend to get disproportionately little credit for comparable contributions [ibid. 57].
>
> This complex pattern of the misallocation of credit for scientific work must quite evidently be described as "the Matthew effect," for, as will be remembered, the Gospel According to St. Matthew puts it this way: "For unto every one that hath shall be given, and he shall have abundance: but him that hath not shall be taken away even that which he hath" [ibid. 58].

According to Merton, the Matthew effect is especially at work in cases of collaboration and "in cases of independent multiple discovery made by scientists of distinctly different rank" [ibid.].[156] Under Merton's functionalist perspective the effect is *dysfunctional* for the scientific reward system, it is an inequity that undermines the reward system. But at the same time, now with respect to science as a communication system, the effect is *functional*: it "may operate to heighten the visibility of new scientific communications" [ibid. 59].[157] Merton is not very explicit and specific about its explanation. He seems to think that normally visibility is based upon deserved reputation, and, therefore, signals quality.

Forgetting about Sakoda's early but unpublished 49-model, and taking into account only what was published, it seems fair to consider Schelling's and Sakoda's checkerboard models as a case of independent multiple discovery, i.e. as the second of Merton's typical constellations in which the Matthew effect will be at work *if* the independent multiple discovery is "made by scientists of distinctly different rank" [ibid. 57]. But were Schelling and Sakoda at the critical times, the beginnings of the 1970s, scientists of *distinctly different ranks*?

6.1 The Well Recognized Strategist and Defense Intellectual

As we have seen above (cf. section 4), Sakoda was well recognized in the (old) community of computational social scientist. Even by the end of the 1980s a *computational social scientist* Thomas C. Schelling was unknown in that network. But there was another Schelling, not "playing around" with checkerboards, rather than analyzing the

[156] Later Merton has applied the Matthew effect to problems of ressource allocation in science in general; cf. [Merton, 1988].

[157] For a critical discussion of empirical and normative ambiguities in Merton's argumentations cf. [Strevens, 2006].

grave problems of nuclear deterrence, warfare, and disarmament with newly developed approaches, methods, and theories, especially bargaining and game theory. In 1971 *that* Thomas C. Schelling was already a scientist and political adviser of considerable rank.

Like Sakoda, Schelling was born in California.[158] Like Sakoda, Schelling had studied at the University of California at Berkeley. In 1944 he graduated in economics. Unfit for military service, he started to work in the U.S. Bureau of the Budget. After completing his PhD exams at Harvard in 1948, Schelling joined the administration of the Marshall Plan. Two years later he became a staff member of the foreign policy adviser to the president of the U.S. In the following years Schelling became associate professor of economics at Yale University and senior staff member of the RAND Cooperation, the very first and archetypal U.S. version of what is now called *a think tank*.[159] Since 1958 (and until 1990) he was professor at Harvard in the Department of Economics, the Center for International Relations, and (after 1969) in the John F. Kennedy School of Government.

Having been practically involved in negotiations about the spending of Marshall Plan funds for a reconstruction of post war Europa, Schelling already developed in the 1950s a strong theoretical interest in bargaining strategies and the understanding of situations in which—different from zero sum games—the involved actors have partially conflicting, partially common interests. As Schelling frankly reports in his autobiographical notes from 2005, his professional introduction to game theory was only in 1957 by the book *Games and Decisions*, written by Howard Raiffa and R. Duncan Luce [Luce and Raiffa, 1957].[160] One year later Schelling published his *Prospectus for a Reorientation of Game Theory*.[161] In retrospect, Schelling writes about his prospectus:

> I was trying to get game theorists to pay more attention to strategic activities, things like promises and threats, tacit bargaining, the role of communication, tactics of coordination, the design of enforceable contracts and rules, the use of agents, and all the tactics by which individuals or firms

[158] For the following biographical details cf. [Schelling, 2005], [Ayson, 2004, ch. 1], [Klein, 2013, 577-580], [Freedman, 2013, ch. 13], and especially [Dodge, 2006].

[159] RAND stands for Research And Development. Originally that was an US Air Force funded project of the Douglas Aircraft Company on future weapons. In the late 1940s the Rand Corporation evolved out of that project as an non-profit organization independent of Douglas. RAND got huge sums from the U.S. Air Force. Like Google today, RAND could offer bright, creative, and highly paid minds ideal working conditions to develop a science of warfare under the dramatically new conditions of nuclear weapons on *both* sides of the Cold War. Later RAND defined its mission more generally. To date, the mission statement says: "The RAND Corporation is a research organization that develops solutions to public policy challenges to help make communities throughout the world safer and more secure, healthier and more prosperous. RAND is nonprofit, nonpartisan, and committed to the public interest" (RAND homepage at: `http://www.rand.org`. – For the early history, importance, political and scientific impact of RAND cf. [Amadea, 2003, ch. 1]. Amadea calls RAND an "archetypal Cold War institution" [ibid. 27]. For in depth studies of the scientific impact and consequences cf. [Erickson et al., 2013] and [Erickson, 2015].

[160] In his autobiography Schelling remembers to have spent "at least a hundred, maybe two hundred, hours" with the book [Schelling, 2005].

[161] The *Prospectus* was originally published in the *Journal of Conflict Resolution*. The article was so long that it filled the whole issue. It is reprinted in [Schelling, 1960].

> or governments committed themselves credibly. ...I began to appreciate that the most immediate and important application of the kind of "game theory" I was pursuing was in military foreign policy, especially nuclear weapons policy [Schelling, 2005].[162]

Soon Schelling became deeply involved in the evolving scene of nuclear strategy analysts and consultants. While he stayed in London, the founding director of the Institute for Strategic Studies, Alastair Buchan, became a close friend. Back in Harvard, together with a MIT colleague, Schelling established a Center for Arms Control and organized regular discussions, workshops, and a "summer study" of arms control [ibid.]. When, in January 1961, the Kennedy administration started to work, several participants of the Harvard-MIT study group got positions of highest ranks, e.g. as Kennedy's national security adviser (McGeorge Bundy, 1919–1996, Harvard) or as chair of Kennedy's Science Advisory Committee (Jerome Bert Wiesner, 1915–1994, MIT). Now Schelling had "real connections".

> Because of these connections I was appointed chairman of several interagency committees concerned with nuclear weapons policy over the next several years. (One of them brought into being the "hotline" between the Kremlin and the U.S. Government, another initiated the process that led, after a hiatus caused by the Soviet invasion of Czechoslovakia, to the Anti-Ballistic Missile Treaty) [Schelling, 2005].

"Conducting" a cold war had to be learnt—and the 1960s were still years in which the US *and* the Soviet Government had to learn about some basics of a peaceful stability under the condition of nuclear weapons on *both* sides. With unrivaled clarity, Schelling had worked out the decisive threat to a stable nuclear peace: the reciprocal fear of a surprise attack that might destroy the retaliation capacity, and, therefore, lead to a pre-emptive attack.[163] Together with some others, Schelling favored a solution, that only at first glance sounds counter-intuitive: the point is to guarantee the survival of a capacity to retaliate on a scale that makes the initial attack unattractive. As a consequence, it is important to make nuclear weapons *invulnerable*, while leaving the population and industrial areas *unprotected*. The latter is a modern functional equivalent of an ancient practice: the exchange of hostages. Therefore, submarine based or in some other way invulnerable missiles in combination with no or only a very limited number of anti-ballistic missile defense systems could be the core of a "technical" solution to avoid a global nuclear war. After half a decade of negotiations, the U.S. and the Soviet Union finally signed in 1972 a treaty (the Anti-Ballistic-Missile Treaty, ABMT) that basically followed Schelling's line of reasoning.[164]

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[162] See also the interview with Schelling in [Herfeld, 2017, in print].

[163] See [Schelling, 1960, ch. iv] and [Schelling and Halperin, 1961]. In 2007, in an interview, Schelling says that the latter book became "a sort of bible for a lot of people" [Carvalho, 2007, 3]. For a more general history of the strategic thought on how to use nuclear weapons see [Kaplan, 1983], [Erickson et al., 2013], and [Erickson, 2015]. – The reader should note the basic (though abstract) similarity between the reciprocal fear of a nuclear surprise attack and the situational structure of Hobbes' *state of nature* in his *Leviathan*.

[164] The treaty was prepared already under the Johnson administration, but signed under Richard Nixon's republican presidency (1969–1974).

There was another type of war, not excluded by the existence of nuclear weapons: *limited* war, fought by higher or lower intensity, but without use of nuclear weapons. The Korean War 1950–1953 had been an example. Already in 1957 Schelling wrote an article with the telling title, *Bargaining, Communication, and Limited War*.[165] (Later the article became a chapter in Schelling's book *The Strategy of Conflict*.) In that article Schelling conceptualizes limited war as an instance of *tacit bargaining*, i.e. bargaining under conditions of impossible or incomplete communication [Schelling, 1960, ch. 3]. Noisy and bloody as they may be, in this view military actions are considered and should be designed as clear and understandable signals of resolve and determination to incur even more pain and damage if the enemy does not change its behavior. Tacit bargaining becomes *coercive diplomacy*. With explicit reference to Schelling and his ideas, Robert McNamara (1916–2009), Secretary of Defense from 1961 to 1968, writes retrospectively:

> This view penetrated the civilian leadership under Kennedy, and later Johnson, to a remarkable degree. In this view, a conflict is as much *psychological* as physical, with the upper hand gained by the side with the most credible threats conditioned on *future* actions. Thus, acts of war are chosen in part for their *signaling* value as well as their capacity to disable an opponent. It is cautious when confronting a nuclear opponent because of the ever present fear of escalation to nuclear war. It is concerned with *limited objectives*: It is the *other* side that must decide whether to escalate and face the consequences. The objective is to bend an opponent's will via the threat to continue on up the ladder of escalation [McNamara et al., 1999, 160].

That was not a misunderstanding of Schelling's ideas.[166] These ideas had worked in the Cuban Missile Crisis in October 1962, though today it seems clear that the world was much closer to the brink of a nuclear disaster than *any* of the participants thought *and* intended at that time.[167] In the Vietnam War—the Vietnamese call it the *American*

[165] The article was published in the very *first* issue of the *Journal of Conflict Resolution* in March 1957. By itself that is an indicator for a very early recognition of Schelling's work. In an interview with Jean-Paul Carvalho, Schelling describes how he met Kenneth Boulding, the founder of the journal; cf. [Carvalho, 2007, 1f.].

[166] To give just one example, McNamara echoes what Schelling writes in *Arms and Influence* in a chapter with the telling title *The Idiom of Military Action*:

> War is always a bargaining process, one in which threats and proposals and counterthreats, offers and assurances, concessions and demonstrations, take the form of actions rather than words, or actions accompanied by words. It is in the wars that we have come to call "limited wars" that the bargaining appears most vividly and is conducted most consciously. The critical targets in such a war are in the mind of the enemy as much as on the battlefield; the state of the enemy's expectations is as important as the state of his troops; the threat of violence in reserve is more important than the commitment of force in the field [Schelling, 1966, 142f.].

Similar ideas can be found in Hermann Kahn's book *On Escalation* [Kahn, 1965].

[167] Cf. [McNamara, 1996, 338–43] and [Blight and Welch, 1989]. Schelling considered the Cuban Missile Crises as "a contest in risk taking" [Schelling, 1966, 96]. For details of his view cf. [Schelling,

War—the ideas lead to a disaster of major proportions: a seven digit number of dead, mostly civilians, and almost 60,000 U.S. soldiers died. A complete list of the damage would be long (and—due to long-term damages—still get longer day by day).

The Vietnam War had a long pre-history of U.S. involvement in Indochina. The first steps of escalation (after a minor U.S. involvement already since the time when post war France tried and failed to resume its colonial rule) had started in the late 1950s. Schelling himself saw his ideas at work during the dramatic further escalation of the Vietnam War after the alleged Gulf of Tonkin incident in 1964, the first year of Lyndon B. Johnson's presidency (after John F. Kennedy had been assassinated in November 1963).[168] In Schelling's book, *Arms and Influence*, the military actions right after the Gulf of Tonkin incident—American aircraft attacked five North Vietnamese naval ports, destroyed patrol boats and petroleum depots—are used as a *best practice example* of tacit bargaining and coercive diplomacy:

> If the American military action was widely judged unusually fitting, this was an almost aesthetic judgement. If words like "repartee" can be applied to war and diplomacy, the military action was an expressive bit of repartee. It took mainly the form of deeds, not words, but the deeds were articulate. The text of President Johnson's address was not nearly as precise and explicit as the selection of targets and the source and timing of the attack. The verbal message reinforced the message delivered by aircraft; and the words were undoubtedly chosen with the Communist as well as the American audience in mind. But that night's diplomacy was carried out principally by pilots, not speech-writers [Schelling, 1966, 142].

In March 1965, another operation of coercive diplomacy, operation *Rolling Thunder*, started: a graduated and sustained aerial bombing, meant to signal to the communist government in the North under the legendary and popular president Ho Chi Minh (1890–1969), better to stop the flow of aid to the Viet Cong in the South. As McNamara in his *In Retrospect*, written in regret, would later write, Rolling Thunder "would continue for three years and drop more bombs on Vietnam than had been dropped on all of Europe in World War II" [McNamara, 1996, 174]. But somehow North Vietnam did not understand the signals sent by the bombs—at least the support of the

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1966, ch. 3]. In a discussion statement during a conference that in the late 1980s reexamined the crisis, Schelling said

> that the Cuban missile crisis was the best thing to happen to us since the Second World War. It helped us avoid further confrontation with the Soviets; it resolved the Berlin issue; and it established new basic understandings about U.S.–Soviet interaction. Sometimes the gambles you take pay off. . . . I don't think the Cuban missile crisis should be repeated, but I do think it was a good crisis [Blight and Welch, 1989, 104].

[168] Allegedly an U.S. destroyer had been attacked by North Vietnamese patrol boats on August 2, 1964, and then again, two days later. It seems clear today, that the first attack was initially considered as something minor, and that it happened only after the U.S. destroyer had come very close to the coastline of North Vietnam. The second attack never happened; cf. [McNamara et al., 1999, 23f., 184ff., 202ff., 215f.]. The so called Gulf of Tonkin Resolution, that passed the Congress on August 7, 1964, basically authorized President Johnson to wage war in Indochina without a declaration of war.

Viet Cong never stopped. Robert McNamara, initially the decisive advocate of the bombardment campaign—the Vietnam War was often considered as "McNamarara's war"—, became more and more skeptical about the bombing and resigned in February 1968. Ten months later, President Johnson finally stopped Rolling Thunder.

Schelling was *not* a member of the group of political, administrative, and military officials, that made the decision to start and then to extend Rolling Thunder. But former colleagues, co-authors, and friends were—e.g. McGeorg Bundy[169], Morton Halperin[170], and John T. McNaughton. Thus, measured in terms of network distance, Schelling was very close to the official decision makers. At least one of them, John T. McNaughton (1921–1967), asked Schelling for advice on an intelligent design of operation Rolling Thunder. Schelling and McNaughton knew each other since the late 1940s when both worked for the administration of the Marshall Plan. Since 1953 McNaughton was a faculty member of Harvard Law School. When, in the early days of the Kennedy Administration, Schelling got a job offer as the arms-control deputy of the Assistant Secretary of Defense, he declined and recommended McNaughton. McNaughton had doubts about his competence, but Schelling "told him not to worry, that it was easy, that he would teach McNaughton everything he would need to know" [Kaplan, 1983, 333]. McNaughton accepted. And he moved up the hierarchy. Since 1964, McNaughton was the Assistant Secretary of Defense, i.e. a kind of general counsel, chief aide, and right-hand man of McNamara, the Secretary of Defense since 1961. Since the fall of 1964, McNaughton usually accompanied McNamara for White House meetings on Vietnam.[171] In a major controversy about Schelling's role in the Vietnam War—some influence on military thought *versus* serious co–responsibility for a major disaster—the visit of McNaughton to Schelling in December 1964 plays a role.[172] Based upon an interview with Schelling, Kaplan writes that Schelling told McNaughton "that the bombing should not last more than a few weeks; it would succeed by then or ... it would never succeed" [Kaplan, 1983, 334]. Compared to McNamara's views at that time, that was a very cautious and skeptical view.[173]

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[169] McGeorg Bundy had participated in Schelling's Center for Arms Control. He was National Security Adviser from 1961–1966, i.e. under the presidents Kennedy and Johnson. Before he had been professor and dean at Harvard.

[170] Morton Halperin was Schelling's co-author in [Schelling and Halperin, 1961]. Additionally, he was a colleague of Schelling in the Harvard Center for International Affairs. Under Johnson's presidency he worked in high official positions in the Department of Defense).

[171] For details cf. [Wells, 2001, 197ff.] and [Ellsberg, 2002, 51].

[172] For the controversy cf. [Kaplan, 1983, ch. 23] and the thoughtful and reflective discussion of Kaplan's critic in Dodge's Schelling-biography [Dodge, 2006, chs. 14, 18, and 19]. Both author's interviewed Schelling, though with two decades of distance of time. Dodge's discussion is partially based upon interviews and other personal exchanges with Schelling that directly address the difficult questions of range and limits of an adviser's responsibility. For careful and fundamental reflections on this problem see [Thompson, 2005], especially ch. 1 and ch. 2.

[173] For McNamara's initial role and arguments as the leading bombardment advocate, see Ellsberg's reports about his conversations with McNaughton [Ellsberg, 2002, 48ff.]. At that time, Ellsberg was assistant to McNaughton. Today it is clear, there was an official and a secret McNaughton, the former loyal to McNamara and Johnson, the latter obviously convinced that the U.S. should not be in Vietnam in the first place, since a huge majority of the Vietnamese had a strict preference for Ho Chi Minh and the Viet Cong; cf. [Harrison and Mosher, 2007] and [Harrison and Mosher, 2011], based upon McNaughton's secret diary. McNaughton died 1967 in a plane crash.

The final design of Rolling Thunder, who the designers, and what their arguments were, all that can be read in what later became known as *The Pentagon Papers*[174]: a secret historical study of the U.S. involvement in Southeast Asia from the end of World War II to the present, commissioned by McNamara in 1967. At the time the Secretary of Defense was already skeptical about the whole war.[175] A task force with access to secret ("classified") material from the archives of the Department of Defense, State Department and the Central Intelligence Agency (CIA), had to write the study. On completion in 1969, it was bound into 47 volumes of 3000 pages narrative along with 4000 pages of supporting documents. One of the chapters is *The Air War in North Vietnam: Rolling Thunder Begins, February–June, 1965*.[176] The initial plan was a two-phase aerial bombardment. Phase one (30 days) was meant to intensify earlier signals. Phase two (two to six months) was designed as a gradual and progressive squeezing of North Vietnam. Escalation steps and target selection were under a strict and tight political control.[177]

That was *not* what Schelling had proposed to McNaughton. But the whole design of the bombing campaign was discussed and written in the language of signaling. Three decades later, in his *Argument Without End: In Search of Answers to the Vietnam Tragedy*, McNamara wrote about Rolling Thunder (probably a bit in search for co-responsibles):[178]

> Between the lines of the statement of the overall objectives of the two-phase bombing one finds many of the principles expoused by U.S. civilian strategists such as Thomas Schelling [McNamara et al., 1999, 169].

Starting with a compliment for the formulations ("elegant, clear, coherent"), McNamara declares the plan as "wrong in every important respect" [ibid. 170]. Then he continues:

> A story circulated at Harvard during the 1960s that a missed opportunity had occurred when Harvard failed to offer a scholarship to Ho Chi Minh, in order that he might have the opportunity to study with Professor Schelling. If he had, according to the Cambridge pundits, he would have known that Washington was trying to send him a *signal* via the bombing. As it was, Ho and his colleagues, in their ignorance, thought the United States was trying to destroy their country [McNamara et al., 1999, 170].

[174] The official title was *Report of the Office of the Secretary of Defense Vietnam Task Force*.

[175] Cf. chapters 9 and 10 in McNamara's *In Retrospect* [McNamara, 1996], especially pages 256 and 280ff.

[176] Gravel Edition Vol. III, chapter 3.

[177] As it was usually the case, the Joint Chiefs of Staff (JCS), i.e. the highest military institution, proposed a much more massive approach: shocking bombing right from the start and much more ground troops. Minimizing the risks of a confrontation with China or the Soviet Union was only a minor concern.

[178] The book is based upon meetings of U.S. *and* Vietnamese scholars and military and civilian officials that were active during the Vietnam War. The meetings took place 1995 to 1998.

Less sarcastically: in the dominant view of the Kennedy and Johnson administration, the Vietnam War was a war against a threatening expansion of communism in Southeast Asia.[179] A success of the communist North, it was thought, would probably initiate a domino effect. That "Ho and his colleagues" had a massive support in both the North and the South of Vietnam, was well known in the U.S. administration. Given the realistic prospect of a 80% (or even higher) majority for Ho Chi Minh, free elections were no U.S. option.[180] In the view of North Vietnam and the Viet Cong, it was a war of independence with the U.S. as a foreign aggressor, that had installed the corrupt and incompetent "puppet government" of South Vietnam.[181] The bombardment signalled clearly that the U.S. were not willing to accept Vietnam as the *one independent country* that almost all Vietnamese wanted it to be. It was completely unacceptable for the North to stop support for the insurgence in the South and to accept the partition of the country. For "Ho and his colleagues," that, indeed, meant the destruction of the country.[182]

Taking it all together, what, then, was Schelling's influence on strategy and strategic thought in the Kennedy and Johnson years? Schelling had declined an offer for an official position within the Kennedy government (the position that, recommended by Schelling, Robert McNaughton accepted). But at the same time, he was a leading adviser, analyst, and defense intellectual.[183] Additionally, Schelling was a kind of crisis trainer: as a political and military exercise, he organized and ran several huge crisis simulation games.[184] In his Schelling-biography *The Strategist*, Dodge summarizes Schelling's position and impact in that time:

> He was among the elite group in the background, the Cold War's civilian strategists existing in the shadows, and whispering in the ears of the

[179] That had already been the view of the Eisenhower administration before and after the Geneva Accord of 1954. That accord definitively ended French colonial rule in Vietnam after France's military defeat in the battle of Dien Bien Phu in the same year. Vietnam was provisionally partitioned into North and South. Elections were scheduled for 1956. Based on these elections the country was to be unified again.

[180] In his memoirs, Eisenhower stated that frankly, and British Intelligence expected even a 90% majority for Ho Chi Minh [cf. Harrison and Mosher, 2007, 498]. Harrison and Mosher report about McNaughton's view that "it would have taken lobotomies to change that stark political reality" [ibid.].

[181] The Pentagon Papers reveal that the North's and the internal U.S. view on the South's government did *not* differ very much.

[182] For the different perceptions of the conflict see ch. 5 in [McNamara et al., 1999]. Given the different perceptions, one might reply to the McNamara quote above: Ho understood fairly well the signal sent via the bombs. But McNamara and his colleagues (in their ignorance), did not understand, what their message meant to the addressees. In his at that time confidential 1970 RAND background paper *"Coercive Diplomacy" in the Light of Vietnam: Some Preliminary Notes* Ellsberg analyses precisely the inherent problems of coercive diplomacy, the different perceptions of the war, the U.S. ignorance of their enemy, and why "the Cuban Missile Crisis ... was a poor school for the conflict with North Vietnam and the Viet Cong" [Ellsberg, 1970, 22].

[183] McNamara refers to Schelling as an example of the "so-called defense intellectuals" that exerted considerable influence during the Kennedy years [McNamara et al., 1999, 169]. The years 1955–1965 were later sometimes called the *golden age* of American strategic thought.

[184] For an exchange about the status, effects, lessons, and possible dangers of such games see the the controversial contributions of Schelling, Levine, and Jones in [Levine et al., 1991]. See also the report on a controversy about such war games with Schelling on the one side, McNamara and others on the other side in [Blight and Welch, 1989, 133f.].

> decision makers [Dodge, 2006, 149].[185]

Schelling's close relations to the U.S. Government ended in 1970. On April 30, 1970, President Nixon (1913–1995) announced that U.S. troops had entered Cambodia, a country bordering to Vietnam. Border regions, not under the control of the Cambodian government, were used for transport by the North and the Viet Cong. But the country was neutral in the conflict. As many of his Harvard colleagues, Schelling was horrified by the government's further escalation of the war. One week after Nixon's announcement, Schelling lead a group of twelve Harvard faculty members in a meeting with Henry Kissinger, a very close colleague of Schelling at Harvard, but now on leave as Nixon's National Security Adviser. The group declared their opposition to the U.S. invasion of Cambodia, resigned as government consultants (what most of the twelve were), and made their confrontation with Kissinger public, thereby supporting the already widespread and rapidly growing anti-war movement.[186] That had consequences. In his Schelling biography, Dodge writes:

> The bitterness that resulted from that meeting didn't vanish. Schelling would not see Kissinger again for several years, and Kissinger refused to ever set foot on the Harvard campus again. Schelling had lost his audience in the executive branch, his access to high-level influence [Dodge, 2006, 159].

One of Schelling's former PhD students, Daniel Ellsberg, went even further and leaked the top secret 7,000 pages study about the history of the U.S. involvement in Southeast Asia that McNamara had commissioned—a leak of historical proportions, and *the* paradigm case of whistle blowing.[187] Recommended by his mentor Schelling, Ellsberg had started to work at RAND as a "dedicated cold warrior, in fact a professional one" [Ellsberg, 2002, 4]. In his Ellsberg biography *Wild Man*, Tom Wells reports that Schelling considered Ellsberg as a genius—though a genius with a complicated personality structure.[188] Initially Ellsberg's work was focused on the problem of nuclear

[185] 1970, in a at that time confidential RAND background paper, titled *"Coercive Diplomacy" in the Light of Vietnam: Some Preliminary Notes*, Ellsberg rejects the conjecture that in the 1964–65 bombing of North Vietnam Schelling's or Kahn's formal strategies were consciously applied by policy-makers.

> What seems more plausible is that such writers as Schelling and Kahn were expressing analytically in the 60's premises and orientations that were widely shared in the official, semi-official and academic circles in which they moved. They drew, in general and abstract form, tactical conclusions, specific instances of which were quite likely to be invented independently by officials confronting particular conflict situations of that period [Ellsberg, 1970, 2].

[186] Chapter 19 (*Concluding Vietnam*) of Dodge's Schelling biography describes the confrontation with Kissinger in detail [Dodge, 2006]. For the reconstruction of what really happened, the chapter refers to a whole bunch of documents and sources.

[187] Edward Snowden's disclosures of the surveillance programs of the National Security Agency NSA are often compared with Ellsberg's disclosures four decades before.

[188] Cf. [Wells, 2001, 103, 122–25, 574]. Wells starts the preface to his book with the statement that "this is an unauthorized biography, written by someone sympathetic to Daniel Ellsberg politically but critical of the man" [ibid. vii]. The biography psychologizes heavily.

surprise attacks. Later, in the Pentagon, he became assistant to McNaugthon. There he had contributed to the top secret historical study about the U.S. involvement in Southeast Asia that McNamara had commissioned in 1967. The study made very clear that, from the beginning onwards, the history of that involvement was a history of deceit, betrayal, and illusion. Several governments had systematically lied to the congress, the public, and—last but not least—the world about what they were actually doing and what their real intentions were.

Initially Ellsberg saw Vietnam as a problem. By the late 1960s, he considered the Vietnam War "a moral and political disaster, a crime" [Ellsberg, 2002, 4].[189] In October 1969, Ellsberg, together with his friend Anthony Russo, secretly photocopied night by night in small portions the whole study.[190] Ellsberg contacted several politicians to interest them in the copy, and finally leaked the study to *The New York Times*. On June 13, 1971, *The New York Times* started to publish excerpts of the documents. Soon they were dubbed *The Pentagon Papers*. Other newspapers followed. The Nixon administration tried, but failed to interdict the publication. Ellsberg surrendered to arrest, charged for several crimes (espionage, theft, conspiracy etc.), summing up to a maximum sentence of 115 years for Ellsberg and 35 years for Russo. During the trial, the presiding judge, William Matthew Byrne Jr., was initially very hostile towards Ellsberg and Russo. But, the Nixon administration started to act illegally to "plumb the leaks" and to discredit Ellsberg. That became public as part of the so called *Watergate Affair*.[191] This included breaking in to the office of Ellsberg's psychiatrist, and offering the FBI directorship to the judge while the trial was still going on. That changed the situation in the trial dramatically. On May 11, 1973, the judge decided that "the totality of the circumstances in this case ... offend 'a sense of justice'," and dismissed all charges against Ellsberg and Russo. Ellsberg gained lots of new friends for his action; several prizes were awarded to him. But for many in the RAND corporation he became a *persona non grata*. Schelling's biographer Dodge reports (and substantiates by documents) that "one RAND compatriot who never deserted Ellsberg was Tom Schelling" [Dodge, 2006, 161].[192]

If he had only been a strategist "existing in the shadows, and whispering in the ears of

[189] For the following details cf. [Ellsberg, 2002], especially ch. 32, [Dodge, 2006], especially ch. 19, and [Wells, 2001], especially ch. 10ff.

[190] Ellsberg confidential background paper *"Coercive Diplomacy" in the Light of Vietnam: Some Preliminary Notes* [Ellsberg, 1970] must have been written at about the time when he copied over the nights the secret documents; cf. footnote 185.

[191] The Watergate Affair started in June, 1972, with a break-in at the headquarter of the Democratic National Committee (basically the headquarter of the Democratic Party) at the Watergate office complex in Washington, D.C. Five men were arrested. Stepwise the investigations led to discoveries about an involvement of the Nixon administration. Among other things, it was revealed that Nixon had a tape-recording system in his offices. By an unanimous decision of the Supreme Court, Nixon was obliged to release the tapes to the investigators. The tapes revealed that the government had tried to sabotage the investigations illegally. In 1974, the affair finally lead to Nixon's resignation in the face of a near-certain impeachment.

[192] In decision theory there is a famous paradox, called the *Ellsberg paradox*. The paradox regards systematic violations of principles of rational choice in situations that involve strict uncertainties, i.e. no probabilities can be assigned. The name-giver of the paradox and the whistle blower are the *same* person. Cf. for the paradox [Ellsberg, 1961].

the decision makers" [Dodge, 2006, 149], Schelling could hardly have profited from the Matthew effect when he published his *JMS* article. But Schelling did much more than whispering in the ears of the decision makers. He was also an influential writer who addressed *and reached* a broader audience by his articles and books. Schelling had written in the 1950s a number of articles about a reorientation of game theory, bargaining, communication, limited war, mutual fear of surprise attacks etc. In 1960 he collated them in to an extremely successful book, *The Strategy of Conflict*, published by Harvard University Press. Probably the Cuban Missile Crisis from October 1962 helped a lot to spread the book. In 1963 a paperback edition went into print. In 1964 the book was translated into Spanish. French, Korean, Romanian, Chinese, and Japanese editions followed. In the year 1971, the year in which Schelling's article *Dynamic Models of Segregation* was published, Google Scholar finds already 373 citations of *The Strategy of Conflict*. In 1980, the number of accumulated citations is 1,030. Up to 2015 it is 13,300. Schelling's biographer Dodge diagnoses that with *The Strategy of Conflict*, Schelling "began having an influence on policy analysis unsurpassed in the world of civilian consultants" [Dodge, 2006, 148]. In 1986 a group of writers and scholars, chaired by Lord Ralf Dahrendorf (1929–2009), at that time Warden of St Antony's College, Oxford, started to put together a list "of a hundred books which have influenced Western public discourse since the Second World War".[193] Schelling's *The Strategy of Conflict* is one of them. Others were, for instance, Hanna Ahrend's *The Origins of Totalitarianism*, John Rawls's *Theory of Justice*, or Albert Camus' *The Myth of Sisyphus*. A book's success can hardly be greater.[194]

At this point let's take a first balance under the perspective of the Matthew-effect. In the 1970s Sakoda seems to have been a well recognized computational social scientists, predominantly of the statistician type, and with a strong interest in the technical side of doing computational science. He had published about 20 papers, for the most part very technical, highly specialized, very short (often a one digit number of pages), published in highly specialized journals. Some of his work were more technical reports, manually duplicated, and "published" by Brown University and its Sociology Computer Laboratory. It seems fair to say that, all in all, Sakoda was a well recognized member of a small group that was working on problems that were neither known nor easy to understand nor judged as important by a broader audience inside or outside science.

By contrast, Schelling was a highly influential node in a network of persons of the highest rank in the U.S. government, administration, military, and the policy consultants around them. His ideas about strategy and conflict had diffused in that network. He had successfully made them accessible and known to a broader public, inside and

[193] *The Times Literary Supplement* (*TLS*), October 6, 1995, p. 39. The list was an initiative of the Central and East European Publishing Project (CEEPP). When the project started, there was still the iron curtain. The project aimed at fostering a "common market of ideas" (Ralf Dahrendorf, cited after *TLS*) throughout Europe. After 1989 the project was significantly extended. It financed translations into east and central european languages.

[194] Schelling published in the 1960s two more books on military strategies: in 1961, written together with Morton H. Halperin, *Strategy and Arms* [Schelling and Halperin, 1961] and *Arms and Influence* [Schelling, 1966]. They, too, were successful in terms of citations, though not as successful as *The Strategy of Conflict*.

outside science both at home and abroad. Whether critic or follower, they all couldn't but recognize with admiration his brilliance: "a genius for the telling phrase," "chapter titles little short of choices of genius,"[195] a "characteristic pithiness" [McNamara et al., 1999, 159], "so clear a thinker that he can often reach deep conclusions with almost no visible technical apparatus and so graceful a writer that he can often make these conclusions seem intuitively obvious" [Krugman, 1996, 16].[196] Compliments of this sort exist in abundance.[197] And no one had to be convinced that his work was important. In 1971, when his *Dynamical Models of Segregation* appeared, Schelling was not yet a scientific superstar. But given his already existing visibility and reputation, and utilizing his combination of analytical rigor and brilliant communication, would give him the benefit of the Matthew effect whenever he would turn towards a new scientific topic where, as a matter of fact, he would compete (of course, without knowing that) with a highly specialized computational social scientist, known by other specialized computational social scientists, but unknown otherwhere.

6.2 Segregation: Modeling a Hot Policy Issue of the 1960s and 1970s by a Family of Models

Schelling's *Dynamic Models of Segregation* was again written in his much-praised style: well-elaborated surprising consequences of clearly stated, simple and convincing assumptions, no difficult technical apparatus required, a firework of illuminating examples how the models apply to real-world problems, and, not to forget, an invitation to the reader to run her or his own segregation experiments as table top exercises with dimes and pennies.

Furthermore, *segregation* was at that time not just a nice example for an unintended macro-effect—it was considered as one of the most urgent social problems of the American society. It was for many a high-priority topic on the political agenda since Kennedy's and especially Johnson's presidency. Until today, Johnson is judged by the Vietnam War. However, he was as well the president with the plan and vision called *The Great Society*, basically a set of programs to fight poverty, racial discrimination, bad education, missing medical care, or urban problems like racial residential segregation.[198] As one of the many components in the reform agenda, the Civil Rights

[195] Colin S. Gray about Schelling in his *Editor's Preface* to [Ayson, 2004], p. viii f.

[196] That was written in 1996. At that time Krugman thought that "these virtues . . . have worked against him" [ibid.]. That diagnosis turned out to be wrong.

[197] See also [Zeckhauser, 1989] with a lot of statements of colleagues about Schelling.

[198] An important concern in Johnson's decisions on the Vietnam War always was, that the money spent on the war, could not be spent on his social-liberal Great Society reform agenda. However, the Vietnam War overshadows (may be for ever) Johnson's presidency, which, nevertheless, was the time of the most ambitious social reform agenda since the New Deal policy of Roosevelt in the 1930s. For the most part, Schelling had been satisfied with Democratic Administrations. In an interview about his "ideological profile," given in 2013, Schelling says:

> I was always a "social liberal" as well as Keynesian economist. I favored allowing abortion, treating homosexuals as equals, admitting immigrants, doctor-assisted end-of-life measures, integrating races, ameliorating the "war on drugs", protecting women and their rights, etc. I think most if not all of my colleagues and friends shared my views. I

Act of 1968 made housing discrimination—understood as the refusal to sell or rent a dwelling to any person because of his race, color, religion, or national origin—a federal crime. Given that context, Schelling's *Dynamic Models of Segregation* made very clear, that fighting racial residential segregation might require more than forbidding housing discrimination, since segregation might be unintentionally self-organized, without any housing discrimination, as defined by the Civil Rights Act, being involved. Again, that made an article by Schelling an extremely readable and insightful contribution to public discourse—this time to the hotly debated topic of segregation. The title of Schelling's article directly invoked that context. A family of models was offered to better understand and to fight more efficiently a grave social problem. By contrast, Sakoda's title will often have caused associations to parlor games—nothing particularly serious.

As we have seen further above in section 2 (especially 2.1 and 2.3), Schelling's segregation dynamics is driven by a migration regime that rests upon neighborhood preferences. Below a certain threshold of like neighbors, agents leave their neighborhood, above they stay. Schelling was *not* the first to explain residential segregation in these terms. The first one, who—in order to explain, to predict, and to warn against an imminent racial schism along with the spreading of slums—*explicitly* referred to *thresholds*, *tip points*, *processes of tipping*, or a *tipping mechanism*, was Morton Grodzins, Sakoda's former JERS co-researcher (cf. chapter 3.3). In 1945, the year of the conflict with D. S. Thomas about the publication of his dissertation, Grodzins had left Berkeley, and went to Chicago, where he directed a research project on state–local relations.[199] After a short period as part-time, he got a full-time position at the University of Chicago. In 1951 he had become the director of the Chicago University Press; in 1953, he was appointed Dean of the Division of the Social Sciences. Throughout his short life, Grodzins had been concerned about civil rights, social problems, and peace.[200] In the middle of the 1950s, he became more and more concerned about the social problems in metropolitan areas, especially the housing problem. In his article *Metropolitan Segregation*, published in October 1957 in *Scientific American*, a journal with a very broad readership, Grodzins explains the at that time comparatively *new* phenomenon of an ongoing racial segregation in the North of the U.S. by a "process

> cannot think of any serious change in my political or moral attitudes during the past fifty years [Klein, 2013, 580].

For a critical evaluation of Schelling's role and position in the debate about climate change see [Oreskes and Conway, 2010, 174ff.].

[199] For details of Grodzins' life and career cf. *In Memoriam Morton Grodzins* [Pritchett, 1964]

[200] Following his concern for civil rights that already guided his dissertation, in 1956 Grodzins published a book titled *The Loyal and the Disloyal: Social Boundaries of Patriotism and Treason* [Grodzins, 1956]. The book argues that in contrast to totalitarian ideas, a democratic society should approve of the citizens' typical *manifold* of loyalties and ask only for a very limited national loyalty. Grodzins sharply criticizes the loyalty-security investigations of the McCarthy area. In the late 1950s he became an important figure in the movement that organized the so called *Pugwash Conferences on Science and World Affairs*. The main objective of the yearly conferences was the elimination of all weapons of mass destruction. The Pugwash Conferences were initiated by Joseph Rotblat and Bertrand Russell in 1957. In 1997 the Pugwash Conference won the Nobel Peace Price jointly with Joseph Rotblat. Aged 46, and at that time Vice-President of the American Political Science Association, Grodzins died 1964 after more than a decade of recurrent health crises. For an obituary cf. [Rabinowitch, 1964].

of tipping," induced by a massive migration of Blacks from the South to the North [Grodzins, 1957].[201] In 1958 Grodzins published his small book, *The Metropolitan Area as a Racial Problem*, which was a more encompassing version of his article in *Scientific American*. Chapter III is titled *The "tipping" mechanism*. There he writes:

> The process by which whites of the central cities leave areas of Negro in-migration can be understood as one in the social-psychology of "tipping a neighborhood". The variations are numerous, but the theme is universal. Some white residents will not accept Negroes as neighbors under any conditions. But others, sometimes willingly as a badge of liberality, sometimes with trepidation, will not move if a relatively small number of Negroes move into the same neighborhood, the same block, or the same apartment building. Once the proportion of non-whites exceeds the limits of the neighborhood's tolerance for interracial living (this is the tip point), the whites move out [Grodzins, 1958, 6].[202]

As his explicit references and quotations show, Schelling knew Grodzins' approach.[203] He picked up the central component, but also noticed an ambiguity in Grodzins' (and others') use of the tip point concept.[204] In a *first* version it refers to the proportion of blacks that an individual white i tolerates in his or her neighborhood. If that proportion reaches an upper limit, then i becomes discontent, and decides to move out. In this reading, a tip point is an *individual* threshold value. Let's refer to it as *tip point*$_1$. Obviously, the neighborhood preferences in Schelling's model are of this type. But in the quotation above, Grodzins seems to have in mind *another* type of tip point as well—and that is a tipping point on an *aggregate, collective or macro level*, a kind of "point of no return" where, in a process, things become *irreversible*. It is the idea is that there may be a critical proportion of blacks that induces a domino effect, which ends up with all whites moving out. Let's refer to such an aggregate, collective or macro level threshold as tip point$_2$.[205] The family of models that Schelling presents in his *JMS*

[201] The traditional *rural* segregation patterns in the South were quite different.

[202] Outside science, the tipping-terminology was obviously used long before Grodzins coined the term *tipping mechanism* for his explanatory and predictive purposes. Grodzins reports that

> real estate operators, seeking the higher revenues that come with Negro overcrowding, talk freely among themselves about "tipping a building" or "tipping a neighborhood". (Sometimes this can be done by selling a single house to a "block busting" family.) [Grodzins, 1958, 7].

[203] Grodzins is cited in both the original RAND memorandum from 1969, and in *Dynamic Models of Segregation*; cf.[Schelling, 1969a, 73], [Schelling, 1971a, 181]. In [Schelling, 1972c] one finds a longer discussion of Grodzins' views. Cited is always Grodzins' article *Metropolitan Segregation* in Scientific American from 1957 or a "pamphlet" with the same title, that, according to Schelling, was published by the University of Chicago Press in the same year. However, I could not verify that publication; probably there is an error in Schelling's references. Grodzins is *not* cited at all in [Schelling, 1969b], [Schelling, 1971b], and [Schelling, 1974] (a reprint of [Schelling, 1971b], and in [Schelling, 1978]. Grodzins' book *The Metropolitan Area as a Racial Problem* [Grodzins, 1958] is *never* cited by Schelling. It may be that this small book is the "pamphlet" to which Schelling referred.

[204] For the following compare [Schelling, 1972c, 158ff.]

[205] Grodzins seems to *unequivocally* refer to a tip point$_2$, when he writes:

article has members that try to deal with this second type of macro level tipping point. Schelling elaborates this type of threshold models in the second half of *Dynamic Models of Segregation* [1971a, 167–86]. Therein Schelling turns to "another model". The decisive difference between the models in the first part and the models in the second part, is their neighborhood concept. Schelling titles the first part *The spatial proximity model* [ibid. 149]. Despite of the singular form in the title, *two* models of this type are presented—a one-dimensional and a two-dimensional model. *Both* have self-centered, overlapping neighborhoods.[206] In the one-dimensional model the neighborhood consists of the two cells to the left, and the two cells to right of one's own actual position. In the two-dimensional case it is a 3×3 neighborhood around one's own position. The second part of Schelling's *JMS* article is titled *Bounded-neighborhood model* [ibid. 167]. No longer we have self-centered and overlapping neighborhoods. Instead, the neighborhood now is fixed, the same for all, everybody is either in or out. A block, an apartment house, any well defined and well recognizable area, or a club (as a non-spatial instance) are examples. Schelling assumes certain frequency distributions as to tip points$_1$ of whites *and* of blacks. The tip points$_1$ are defined in terms of color *ratios*. People within the neighborhood move out, if and only if the color ratio has reached their tip point$_1$; people outside move in if, and only if, the color composition meets their preferred ratio. Schelling studies the resulting dynamics with a focus on stable and unstable population compositions of tip points$_1$. I will refer to this model as *the population dynamics model*. The dynamics is completely *deterministic*; the analysis is an *analytical* exercise—no dimes and pennies, no simulations are involved. Then, in a final part *Tipping* [ibid. 181ff.], Schelling cites Grodzins, and argues that the bounded-neighborhood concept suggests itself for an analysis of tipping phenomena [cf. ibid.]. Schelling notices in passing an ambiguity in the tipping point concept, and discusses—if only briefly—difficulties to define and identify aggregate level tip points$_2$. Finally, he declares the process to be "too complex to be treated comprehensively here" [ibid. 186].

It is a 1972 follow-up article, titled *A Process of Residential Segregation: Neighbourhood Tipping* [Schelling, 1972b], in which Schelling presents an in depth analysis of Grodzins' tipping mechanism, the ambiguities in public and scientific use of the tip point concept, makes it more or less precise, and explicitly introduces (though in slightly different words) a distinction of individual level tip points$_1$ and aggregate level tip points$_2$.[207] In the section *A model of the Process* Schelling elaborates the most simple version of his tipping model [1972b, 159ff.]. In the following I'll refer to that model as *Schelling's tipping model*.

A lot of confusion is caused by the fact that Schelling distinguishes between "the spatial proximity model" (singular!) and a "bounded neighborhood model" (singular!),

> In some Eastern cities it is possible to maintain low cost housing projects on an interracial basis as long as non-whites do not exceed roughly 20 per cent of the total residents. Once this point is reached whites will not remain in, or move into, the project [Grodzins, 1957, 7].

[206] Probably that is what Schelling tried to underline by the singular form in the title of the first part.
[207] Schelling refers to my tip point$_2$ as an "aggregate or neighborhood tipping point" [Schelling, 1972b, 166]. For more see the quotations below.

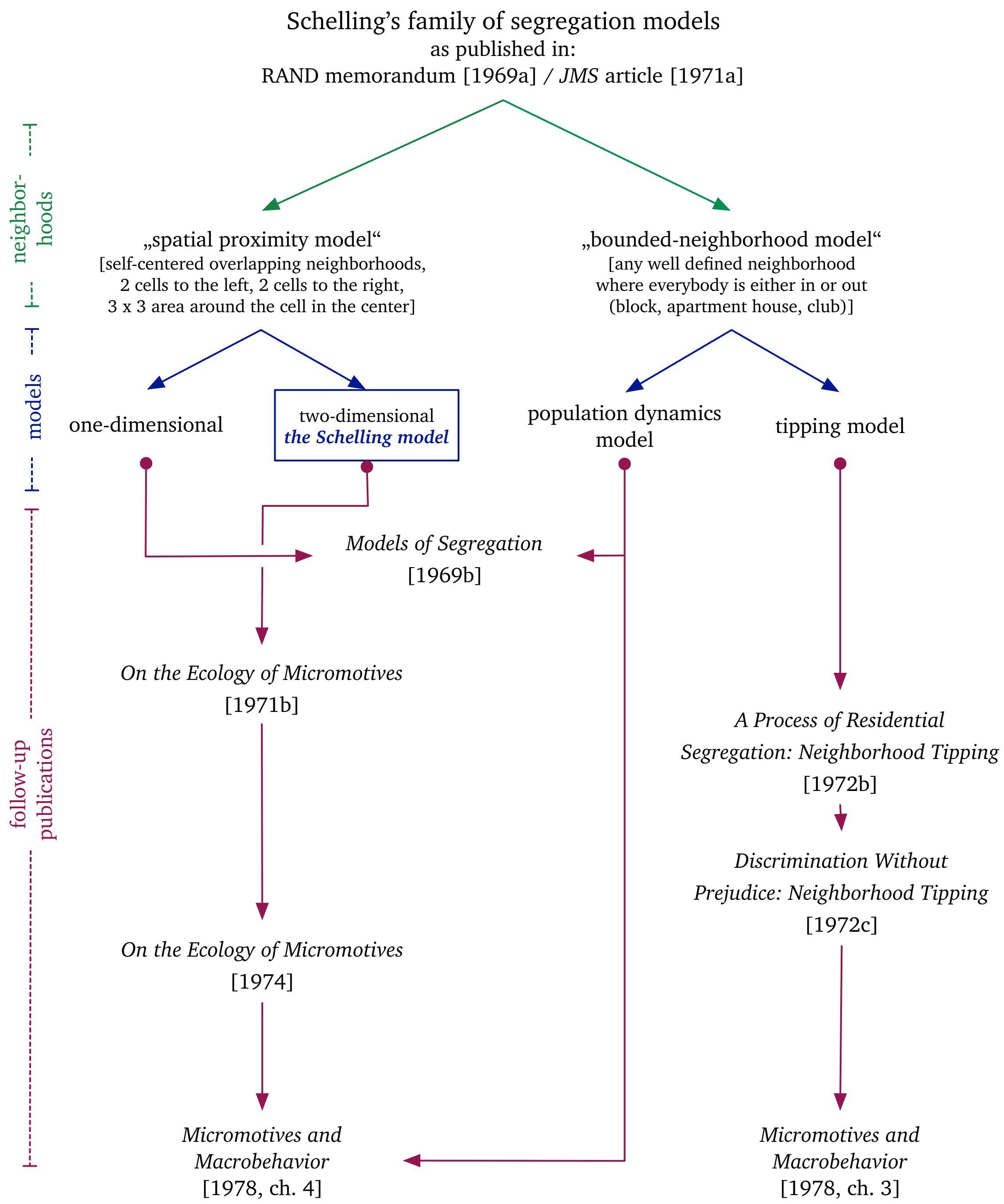

Figure 28: Schelling's family of models in the RAND memorandum [Schelling, 1969a] and the *JMS* article [Schelling, 1971a]. (The preface of the memorandum is not included in the *JMS* article. Some new last sentences are added in the *JMS* article that were not in the memorandum; cf. footnote 211).

subsequently presents for each of the two models *two* very different variants, but does not assign names to them. The spatial proximity model exists as a one- and as a two-dimensional version. The bounded-neighborhood model comes in two variants as well: A first version analyses a dynamics in which the tip points$_1$ of blacks *and* whites matter. I called that model *the population dynamics model*. In the second bounded neighborhood model only the tip points$_1$ of *one* color matter, either white or black. That model is especially useful to analyze the problem of tip points$_2$. Probably the most coherent reading of Schelling is to consider the distinction of "the spatial proximity model" and a "bounded neighborhood model" as a distinction of two types of *neighborhoods*. For both types of neighborhoods two *models* are presented. *Figure* 28 follows this reconstruction strategy. That, then, allows to give an overview on *all* but one of Schelling's publications on segregation and the specific models on which they particularly focus. (Not included in figure 28 is Schelling's book contribution *Segregation on a Continuous Variable* [1977]. While all models of segregation in Schelling's *JMS* article regard segregation along a binary variable like color, sex etc., Schelling develops in [1977] models of segregation in which people respond to "continuous" variables like age, income, IQ, height etc. Neighborhoods of the type discussed above do not play a role. Therefore [Schelling, 1977] is not included in figure 28.)

In Schelling's tipping model, a white individual i moves out if, and only if, $x_w \leq x_w^*(i)$, where x_w is the percentage of whites in the given neighborhood and $x_w^*(i)$ is i's tip point$_1$ in terms of percentages of *whites*. Individual i stays if, and only if, $x_w > x_w^*(i)$. Let x_b be the percentage of blacks in the neighborhood. By assumption all individuals in the neighborhood are either white or black, i.e. $x_w + x_b = 100$. That allows to rephrase equivalently the rules for leaving and staying in terms of percentages of *blacks*. We define, now in terms of blacks, the tip point$_1$ that corresponds to $x_w^*(i)$ as $x_b^*(i) := 100 - x_w^*(i)$. Then the following equivalences hold:

$$\begin{aligned} i \text{ leaves} &\Leftrightarrow x_w \leq x_w^*(i) \Leftrightarrow x_b \geq x_b^*(i) \\ i \text{ stays} &\Leftrightarrow x_w > x_w^*(i) \Leftrightarrow x_b < x_b^*(i) \end{aligned} \tag{13}$$

I will refer to the model defined by (13) as Schelling's *tipping model*.[208] (In the following, if the reference to an individual i is irrelevant, I will refer to the two tip points$_1$ simply as x_w^* or x_b^*.) Obviously, we can describe the tipping model both in terms of thresholds of whites and in terms of thresholds of blacks. Let's refer to the first version as Schelling's x_w^*-tipping model, and to the second as Schelling's x_b^*-tipping model. As Schelling, we start with an analysis in terms of whites, i.e. with the x_w^*-version.

[208] The equivalences in 13 are the *heart* of Schelling's tipping model. For a complete and explicit definition of the model we have to add a neighborhood, sets of white and black agents, a frequency distribution of the individual tip points$_1$ (either in terms of whites or in terms of blacks), and a migration regime that specifies how leaving agents are substituted by newcomers.

In principle, we might consider the two formulations of the tipping model—one in terms of blacks, the other one in terms of whites— as *two* models that are *behaviorally equivalent* in the same sense as we have seen it above for two variants of Sakoda's and two corresponding versions of Schelling's model. However, here the behavioral equivalence is based upon trivial logical equivalences, while in the Schelling/Sakoda case above it takes some deductive effort to detect the behavioral equivalence of the models (cf. section 2.3).

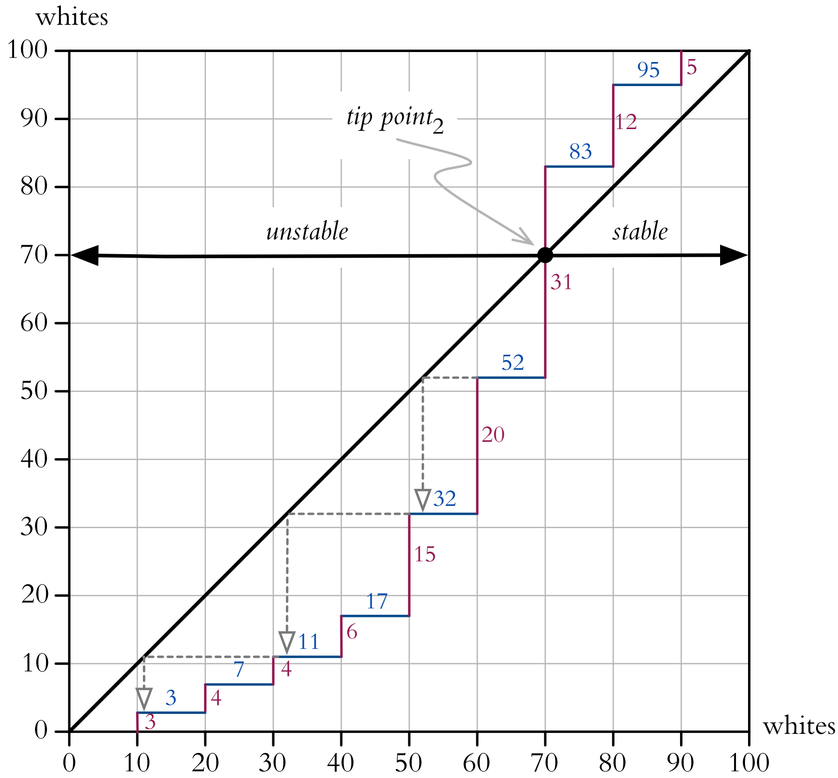

Figure 29: Schelling's tipping model in terms of whites (the x^*_w-version). *Red*: Frequencies of individual x^*_w-tip points$_1$. *Blue*: Cumulated frequencies. The intersection of the 45°-line with the cumulated frequencies is a tip point$_2$ on an aggregate level.

Figure 29 illustrates some important relations. The blue–red graph is the discrete cumulated frequency distribution (CFD) of x^*_w–tip points$_1$ of a hypothetical white population. All numbers can be read as *either* percentages *or* absolute numbers of a neighborhood with 100 whites. Thus, the x- and the y-axis show percentages or numbers of whites. The red vertical lines in the CFD and the assigned red numbers are the frequencies of whites with $x^*_w = x$. For instance, in the hypothetical white population we have 15 members (or 15% respectively) with $x^*_w = 50$ (or 50% respectively). In the following I will speak in terms of *absolute* numbers; but note that we could always switch to a percentage jargon. To make sample calculations easy, we assume an underlying frequency distribution (FD) in which x^*_w increases in steps of 10. The *blue horizontal* lines and the blue numbers assigned to them, are cumulated frequencies of whites with $x^*_w \leq x$. In our example 32 whites have an individual tip point $x^*_w \leq 50$ whites. From left to right, x^*_w–tip points$_1$ require higher and higher numbers of whites to avoid out-migration of whites. The other way around: From right to left, the thresholds become more and more tolerant of blacks.

The diagonal 45°-line shows the points for which $x = y$. Now note the following *logical consequences*: Whenever for a certain x-value (a certain x^*_w) the blue y-value (the cumulated frequencies up to x^*_w) falls *below* the 45° line, then for the most tolerant y whites their number is too small to be content in a neighborhood with only y whites.

(Example: For $x = 50$ the frequencies of all $x_w^* \leq 50$ sum up to 32. Thus, all the most tolerant 32 whites stay as long as there are *more* than 50 whites. But they are only 32.) If, instead, the y-value of a certain x-value is *above* the 45° line, then for *that* y whites their number *exceeds* the number of whites that is necessary to keep them in the neighborhood. (Example: For $x = 80$ the frequencies of all $x_w^* \leq 80$ sum up to 95. For them to stay, having more than 80 whites in the neighborhood would be sufficient.) That has further consequences that make the *intersection* of the cumulated curve and the 45° line a significant point on the *macro* level. In Schelling's words:

> Whether or not we want to call it an aggregate or neighborhood tipping point, the point at which the cumulative curve crosses the diagonal is unique and significant in the dynamics of white response. It is a watershed–a point of black entry prior to which white residency is a self-sustaining condition and beyond which white departure is a self-sustaining process. To the right of that cross-over point, the white population is stable; to the left it is unstable [Schelling, 1972b, 166].
>
> Let me call the point of crossover the neighborhood tipping point. (It may not be the tipping point that people had in mind but it may be the one they should have had in mind.) [Schelling, 1972b, 167]

Schelling's neighborhood tipping point is our *tip point*$_2$. Given the FD and the resulting CDF, numbers of whites to the left of tip point$_2$ in *figure* 29 are *unstable*; numbers to the right are *stable*. To see that, we start an analysis under the assumption that x_b blacks enter the neighborhood, while *simultaneously* the same number of least tolerant whites leave. The effect of that assumption is, first, that the size of the neighborhood is kept constant. Second, the relevant range of FD and CFD is shortened to $[0, (100 - x_b)]$. Additionally, that introduces another reading of the x-axis as numbers or percentages of whites in a population of whites *and* blacks.

Let's go to the point $x = 60$, i.e. a point to the left of tip point$_2$. The corresponding neighborhood composition is 60 whites and 40 blacks. In our example the cumulated frequencies of whites with $x_w^* \leq 60$ sum up to 52. They are the most tolerant 52 whites in the neighborhood. Not all of them are content with only 52 whites: For 20 of the 52 whites it holds that $x_w^* = 60$. For them to stay would require *more* than 60 whites. Therefore they would leave. Following the grey dashed arrow, we find the number of whites that are content with only 52 whites in the neighborhood. They are the whites for which $x_w^* \leq 50$ holds. The dissatisfied whites leave. By assumption, blacks substitute the leaving whites (and the total size of the neighborhood is kept constant).[209] But once the 20 whites that requested more than 60 whites, have left the neighborhood, and, additional 20 blacks have substituted them, the number of whites with $x_w^* \leq 50$ is 32. Again that is not sufficient to keep them all in the neighborhood. The 15 whites that request more than 50, and the 6 whites that request more than 40 whites would leave. That reduces the number of remaining whites to the 11 whites

[209] That is what Schelling assumes for the most simple version of his tipping model. But he considers other possibilities: Time delays, or an in-migration of blacks and whites "in proportions that reflect what is already going on in the neighborhood" [Schelling, 1972b, 161].

for which $x_w^* \leq 30$. All of them would be content if they were more than 30. But they are only 11. Following the grey dashed arrow, we see that 3 of the 11 could live with only more than 10 whites in the neighborhood. As a consequence, the other 8 whites would "turn black". 3 whites stay, but for them to be content would require more than 10 whites. Therefore, the very last 3 whites leave as well, and the neighborhood becomes completely black.

Neighborhood compositions with numbers (or percentages) of whites concurrent to x-values to the right of tip point$_2$ are in a certain sense *stable*: We go, for example, to the point $x = 90$. The point corresponds to a neighborhood composition of 90 whites and 10 blacks. By assumption, the black newcomers substitute the same number of least tolerant whites. In the CFD we get to the 90 most tolerant whites already at $x_w^* \leq 80$. Since their number *exceeds* 80, that is sufficient for all of them to stay. In general, to the right of tip point$_2$ every remaining white is content with the neighborhood composition—and stays in the *mixed* neighborhood.

Obviously, tip point$_2$ is indeed a kind of a watershed, a neighborhood tipping point. For white/black neighborhood compositions to the left of tip point$_2$, white departure is self-sustaining; to the right of tip point$_2$ mixed neighborhoods are stable.[210]

As already noticed above, the equivalences in (13) show that—trivially—we can formulate Schelling's tipping point model as well in terms of blacks, i.e. as a x_b^*-version. Figure 30 shows the x_b^*-version that is equivalent to the x_w^*-version in figure 29. Both are based based upon the same FD and CFD—only the language of description differs. As a consequence, in figure 30 the x-axis represents numbers of blacks. Therefore, the location of the least tolerant whites is now reverted: they are to the left of the x-axis. To the right, whites become more and more tolerant of blacks. Correspondingly, all the frequencies of the FD in figure 29, read from right to left, appear in figure 30 from left to right. It is no surprise, that in the two equivalent descriptions of the model, tip point$_2$ is the same: a neighborhood composition with 70 whites and 30 blacks.

Schelling's tipping model defined precisely and worked out in detail what Grodzins in the late 1950s, more vaguely, intuitively, and informally, had in mind when he described and explained the ongoing racial segregation in metropolitan areas in terms of what he (as the very first) coined "the tipping mechanism," based upon limits of tolerance for interracial living [Grodzins, 1958, 6]. The precise definition of the tipping model was a very thoughtful, reflective, and creative achievement to understand segregation. With his tipping model, Schelling added another member to his family of models that all contributed to better understand the ongoing and threatening racial segregation in American cities. But Schelling also saw that racial neighborhood tipping was just one of many possible applications of his tipping model. The last section of *A Process of Residential Segregation: Neighbourhood Tipping* is titled *The Generalized Tipping Phenomenon* [Schelling, 1972b, 182]. The section starts with the sentence:

> Not only is tipping not confined to residence, it is not confined to race. There can be tipping by age, sex, language, income, and social class [ibid.

[210] Our example is simple—it has just *one* tip point$_2$. But, depending upon the underlying FD, there may be *more* intersections with the 45° line. Then the resulting dynamics of in- and out-migration is more complicated.

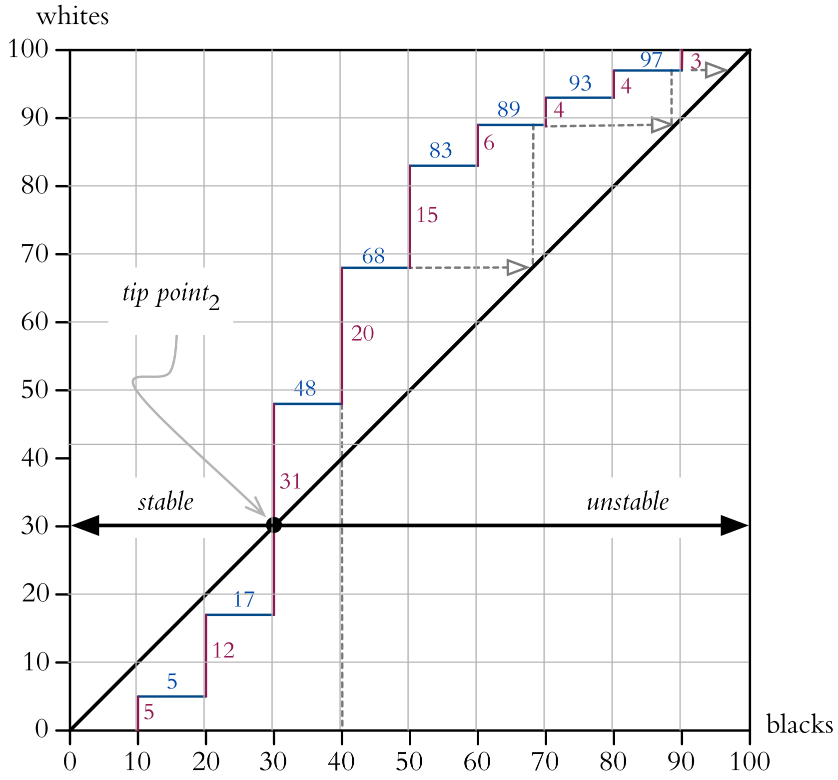

Figure 30: Schelling's tipping model in terms of blacks. The figure shows the x_b^*-version of the x_w^*-version in *figure* 29. *Red*: Frequencies of individual x_b^*-tip points$_1$. *Blue*: Cumulated frequency distribution. The intersection of the 45° line with the cumulated frequencies is a x_b^*-tip point$_2$. As in *figure* 29 the neighborhood tipping point is a neighborhood composition with 30 blacks and 70 whites.

182].[211]

A more detailed and comprehensive generalization of his tipping model can be found in chapter 3 of Schelling's *Micromotives and Macrobehavior*, titled *Thermostats, Lemons, and other Families of Models* [see Schelling, 1978, 91–110]. There he puts the tipping model into the broader context of what he calls *critical mass phenomena*:

> What is common to all of these examples is the way people's behavior depends on *how many* are behaving a particular way, or how much they behave that way–how many attend the seminar how frequently, how many play volleyball how frequently; how many smoke, or double-park; how many applaud and how loudly; how many leave the dying neighborhood and how many leave the school. The generic term for behaviors of this sort is *critical mass* [Schelling, 1978, 94].
>
> The tipping model is a special case—a broad class of special cases—of critical mass phenomena. Its characteristics are usually that people have very different cross-over points; that the behavior involves place of residence or work or recreation or, in general, *being* someplace rather than *doing* something [ibid. 101f.].

Then a "diagrammatics of critical mass" like the one in figures 29 and 30 follows—and it is applied to threshold distributions for both being someplace *and* doing something: the formal structure that underlays Schelling's tipping model covers it all. To see that requires a kind of Gestalt Switch that it is easy to elicit: Suppose a group of whites with the strange habit to paint themselves black if, and only if, a certain number of whites are already painted black. Given the FD of individual thresholds for painting oneself black (x_b^*-tip points$_1$), we can directly apply the formal structure of Schelling's tipping model to find out, whether or not a dynamics of painting oneself black, once it is started, would stop again, or finally end up with everybody painted black. For that dynamics the shape of the CFD, the existence, and the exact position of an aggregate level x_b^*-tip point$_2$ would be decisive. Obviously, we can easily transfer the formal structure of Schelling's tipping model to situations in which people have to make *binary behavioral choices*, that depend upon *how many* behave in a certain way.[212] Whether being

[211] There are only few differences between Schelling's RAND memorandum from 1969 and his *JMS* article from 1971. But he added to the *JMS* article *some new very last sentences*. They clearly demonstrate that Schelling had already in 1971 a generalized view on tipping processes. I quote the last sentences:

> The process, if it occurs, is too complex to be treated comprehensively here. But evidently analysis of "tipping" phenomena wherever it occurs—in neighborhoods, jobs, restaurants, universities or voting blocs—and whether it involves blacks and whites, men and women, French-speaking and English-speaking, officers and enlisted men, young and old, faculty and students, or any other dichotomy, requires explicit attention to the dynamic relationship between individual behavior and collective results. Even to recognize it when it occurs requires knowing what it would look like in relation to the differential motives or decision rules of individuals [Schelling, 1971a, 186].

[212] In binary behavioral choices, the behavioral options are mutually exclusive, and one of the two options has to be chosen.

someplace or doing something, the formal structure of Schelling's tipping model can be applied to all sorts of real world processes. The dynamics of racial segregation induced by black newcomers is one type of application; the invasion of a new behavior is another.[213]

Schelling's tipping model, along with all his other analytical work on a racial dynamics in bounded neighborhoods, never received the attention that—using a concept of self-centered, overlapping neighborhoods—the models in the first part of Schelling's *Dynamical Models of Segregation* attracted. Schelling was well aware of the very different reception and recognition of the different members of his family of segregation models. And, obviously, he was puzzled by that fact. In 2006, that is thirty-five years after his *JMS* article had appeared, he wrote:

> I published, along with the "checkerboard" model, a purely analytical model that I called the "bounded neighborhood" model. . . . I thought the results I got from that model were as interesting as those from the checkerboard, but nobody else appeared to think so. I also explored the nature of a collective "tipping point" in a chapter in Tony Pascal's book, published about a year later, with a purely analytical model. It got little attention [Schelling, 2006, 1642].[214]

There were only few exceptions.[215] But, essentially, Schelling became famous for *the Schelling model*—*not* for the bounded neighborhood models, *not* for his tipping model.

6.3 Excursion: Schelling—a Victim of the Matthew Effect?

Given, that *the Schelling model*, is, in a certain sense, an instance of Sakoda's model (cf. section 2.3), it is a bit an irony, if not a revenge, of history, that *not* Schelling, but *someone else*, Mark Granovetter (born 1943), at that time a still young, but already a

[213] Here I follow and use the view of models as proposed by Gibbard and Varian:

> A model, we shall say, is a story with a specified structure: to explain this catch phrase is to explain what a model is. The structure is given by the logical and mathematical form of a set of postulates, the assumptions of the model. The structure forms an uninterpreted system, in much the way the postulates of a pure geometry are now commonly regarded as doing. . . . In economists' use of models, there is always an element of interpretation: the model always tells a story. If we think of the structure as containing uninterpreted predicates, quantifiers, and the like, we can think of the story as telling what kind of extension each predicate has and what kind of domain each quantifier has. . . . Sometimes it will be found that two models, with two different stories, have the same structure [Gibbard and Varian, 1978, 666f.].

[214] For the chapter in "Tony Pascal's book" see [Schelling, 1972b]. Right after this quotation Schelling continues with the sentences that we used as an epigraph for this study.

[215] For early articles on and empirical finding relevant for the bounded neighborhood model see [Clark, 1991]. For a recent analysis of Schelling's bounded neighborhood model in general see [Afshar Dodson, 2014]. For the tipping model, empirical evidence, and literature see [Goering, 1978], [Easterly, 2009], and [Zhang, 2011]. Zhang combines Schelling's checkerboard model and Schelling's tipping model to "a unified Schelling model" [cf. 2011, 173ff.].

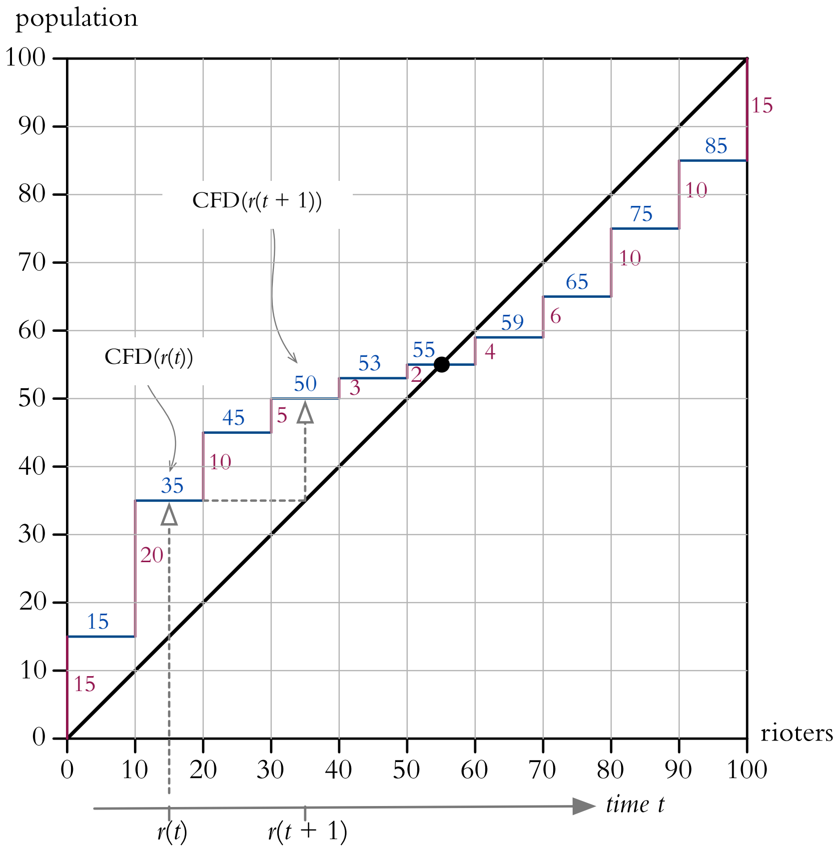

Figure 31: Granovetter's threshold model [cf. Fig. 1 Granovetter, 1978, 1426]. *Red*: Frequencies of thresholds for joining a riot. *Blue*: Cumulated frequencies. The red/blue graph is the CFD. The intersection of the 45° line with the CFD is an equilibrium point.

well recognized sociologist, became even more famous with Schelling's tipping model. The formal structure of the model that Granovetter presents in *Threshold Models of Collective Behavior* [Granovetter, 1978], *is* the formal structure of Schelling's tipping model—though, in the sense explained above, in terms of blacks.[216]

From a logical point of view, the x_w^*- and the x_b^*-version of Schelling's tipping model are *equivalent*. However, in terms of intuitiveness for an understanding of *invasion* processes, probably the two versions are *not* equally good. It may well be be the case that—psychologically—it is more natural to represent the effects of an increasing number of invaders directly from left to right on the x-axis as in figure 30 (i.e. in terms of blacks), rather than by reducing, from right to left, the initially given, predominant group as in figure 29 (i.e. in terms of whites). Anyhow, the formal structure of Granovetter's threshold model is the x_b^*-structure of Schelling's tipping model—invasion of a new collective behavior corresponds to the "leaving of whites, described in terms of blacks" and, therefore, works from left to right on the x-axis.

[216] A follow up and more technical analysis of what he calls *threshold models*, is given in [Granovetter and Soong, 1983].

Granovetter's paradigm process is joining a riot. He imagines a potential riot situation; participation is assumed to depend on individual, heterogeneous riot thresholds. Figure 31 gives an example [cf. Fig. 1 Granovetter, 1978, 1426]. In that example (the numbers are *my* example numbers) 100 people (or, again, percentages of people) are milling around. 15 of them are instigators with a riot threshold of 0. They riot anyway. Again, the vertical red lines and the assigned numbers are the frequencies of thresholds; the blue horizontal lines and their assigned numbers are cumulated frequencies. The complete red/blue graph is the CFD of the underlying FD. The 15 instigators are more than enough to let another 20 people with a riot threshold of 10 join the riot etc. If we assume a discrete time t, and given $r(t)$ is the number of rioters at time t, we can determine $r(t+1)$ by simple graphical methods as indicated by the dashed arrows. For all riot thresholds x to the left of the intersection of the CFD with the 45° line, the CFD *exceeds* the thresholds. As a consequence, the number of rioters increases. At the intersection point—Granovetter refers to that point as an *equilibrium*—the process stops: 55 have a riot threshold $x_r^* \leq 50$, but only 59 have a threshold $x_r^* \leq 60$. Thus, the 4 that would join if, and only if, $x_r \geq 60$ will *not* join the riot—and the process is stopped.

Riots are just a colorful illustration. Granovetter has a longer catalog of other potential applications of threshold models in binary-choice-situations. His catalog includes diffusion of innovations, rumors and diseases, strikes, voting, educational attainment, leaving social occasions, migration, and some findings about conformity or bystander effects in experimental social psychology [see Granovetter, 1978, 1423f.].

When, in 1978, Granovetter published his *Threshold Models of Collective Behavior*, he was well aware of how much he owed to Schelling's tipping point model—*and he stated that frankly*. In the second footnote of his article he writes:[217]

> I have adapted the idea of behavioral thresholds from Schelling's models of residential segregation (1971a, 1971b, 1972), where thresholds are for leaving one's neighborhood, as a function of how many of one's own color also do so. The present paper has Schelling's aim of predicting equilibrium outcomes from distributions of thresholds but generalizes some features of the analysis and carries it in somewhat different directions [1978, 1421 fn.]. [218]

That was a fair and descent description of the situation. (And Granovetter might even have added that his *slightly more formal* description of the basic structure, is easier to understand than Schelling's, which, sometimes, confuses the reader about the actual meaning and re-interpretation of the axes.) His underlying basic formal structure is the same. However, Granovetter's applications, interpretations, and accompanying

[217] The first footnote regards acknowledgments of co-workers, commentators, audiences, and funding [see Granovetter, 1978, 1420].

[218] The references in Granovetter's footnote regard in *our* list of references [Schelling, 1971a,b, 1972b]. As described above, [Schelling, 1972b] is Schelling's in depth analysis of Grodzins' tipping mechanism; here Schelling's tipping model is explicitly defined. Additionally (and again very appropriate), Granovetter points in this footnote to similar models in the literature on epidemics, innovation, and information diffusion.

“stories” focus more on doing something, behavior, rather than being someplace, or being a member of whatsoever.

As many other of his articles thereafter, and some of his articles before, Granovetter’s *Threshold Models of Collective Behavior* [1978] became a sociological classic. By now (December 2016), with more than 4,000 citations since 1978, the article is no. 5 in the list of Granovetter’s most cited works. All his other even more successful articles regard his pioneering applications of network theory in economic sociology. His most cited article is *The Strength of Weak Ties* [1973] (more than 40,000 citations), followed by *Economic Action and Social Structure: The Problem of Embeddedness* [1985] (more than 32,000 citations), *Getting a job: A study of contacts and careers* [1974] (more than 6,900 citation), and *The Strength of Weak Ties: A Network Theory Revisited* [1983] (more than 5,800 citations). Together the five articles account for about three quarters of the total of more than 120,000 Granovetter citations that Google Scholar finds to date (December 2016). The figures make Granovetter one of the most cited sociologist ever.[219]

year	Schelling	Granovetter	$\sum_{Gra} / \sum_{Sch}$
1975	6		
1980	27	5	0.19
1985	38	28	0.74
1990	52	76	1.46
1995	71	174	2.45
2000	92	382	4.15
2005	141	788	5.59
2010	218	1,750	8.03
2015	293	3,670	12.52

Table 5: Citation comparison of Schelling’s *A Process of Residential Segregation: Neighbourhood Tipping* [1972b] and Granovetter’s *Threshold Models of Collective Behavior* [1978]. The numbers in the Schelling and Granovetter column are the cumulated citations in time steps of five years (as found by Google Scholar on December 17, 2016). The right column shows the ratio of Granovetter’s citations to Schelling’s citations.

As seen above, the basic structure of Granovetter’s threshold model [1978] *is* that of Schelling’s tipping model, published six years earlier in [1972b]. Therefore, one could expect that—at least normally—scholars, who publish on problems of threshold based dynamics and, to give appropriate credit, cite Granovetter, cite Schelling’s [1972b] as well. But, as table 5 makes very clear, that is *not* the case: Up to 1985 Schelling [1972b] was cited more often than Granovetter [1978]. But then the situation changed dramatically. By 2000, Granovetter’s article had more than four times

[219] However, Granovetter is *not* the most cited sociologist: In December 2016, Google Scholar finds more than half a million citations for Pierre Bourdieu. The data vary a lot across disciplines, but citations figures seem to be extremely skewed everywhere: very few articles and/or authors get almost all of the citations. The median number of citations per article is probably a lower one-digit number. A significant proportion of articles is never cited at all. In the humanities the non-citation rate seems to be a high two-digit percentage.

more citations. By 2015, the ratio of Granovetter's to Schelling's citations is 12.52; the differential citation success differs by an order of magnitude. Citations are an important currency of scientific recognition for significant achievements. Obviously, it was *not* Schelling who got the lion's share of recognition for his tipping model; as a matter of fact, over the years, Schelling's share is in decline.

The balance is only a little better for Schelling, if we take into account as well his highly cited *Micromotives and Macrobehavior* [1978]. By the end of 2016, the book received more than 6,000 citations. That is about 50% more than the slightly more than 4,000 citations that Google Scholar finds for Granovetter's *Threshold Models of Collective Behavior*. But a qualitative analysis of a sample of the citations of *Micromotives and Macrobehavior* suggests that *almost none* of the citations refers to the chapter *Thermostats, Lemons, and other Families of Models* [1978, 91–110], where Schelling describes again his tipping model and puts it into the broader context of *critical mass phenomena*. The citations predominantly refer to chapter four (*Sorting and Mixing: Race and Sex*), the chapter that deals with *the Schelling model*, chapter seven (*Hockey Helmets, Daylight Saving, and Other Binary Choices*[220]), a chapter that deals with *n*-person prisoner's dilemmas, or the citations regard micro-macro relations more in general. In sum: In terms of recognition by citations, the recognition that Schelling would have deserved for his tipping model, went predominantly to Granovetter—and that despite the fact that Granovetter had fairly and squarely acknowledged how much he owed to Schelling.

Outside science in a narrower sense, things unfolded even worse for Schelling. In 2000, Malcolm Gladwell published *The Tipping Point. How Little Things Can Make a Big Difference* [2000]. The book is a brilliant piece of science communication and became a bestseller. Basically the book is about effective social interventions, the identification of optimal leverage points, where—as the subtitle suggests—a little bit more or a little bit less makes a big difference. Gladwell reports all sorts of cases and case studies, e.g. the revival of hush puppies, epidemics, rumors, the rise and fall of New York city crime, optimal firm size, or a reasonable design of smoking and drug policies. Not all cases involve tipping points in the sense we discussed above, but a lot do. What Gladwell, in a very readable style, communicates to the general public is scientifically grounded, highly valuable information for anybody who wants to participate in public discourse as an informed citizen.

However, under the perspective of giving credit and recognition to the inventors of the basic ideas, concepts, or models, the book is not really acceptable (to say the least): Grodzins is nowhere mentioned. Schelling is not mentioned in the text. Endnotes refer to whole pages. One of them is a long endnote to page 12. There Gladwell writes that "the tipping point model has been described in several classic works" [2000, 261]. The following list contains [Schelling, 1971a] and [Schelling, 1978]. Schelling's most relevant article [Schelling, 1972b] is not listed. For some reason, the book's index does not cover its endnotes. As a consequence, "Schelling" does not exist as an entry.[221] Gladwell does *not* claim to have invented the tipping point phrase, concept or mechanism,

[220] The chapter is a shortened version of [Schelling, 1973].

[221] Granovetter is listed with his two relevant articles [1978] and [1983]. The fifth (and last) listed article is [Crane, 1991]—though with a wrong year and a wrong volume.

but any normal reader—and that is a reader, who does not know the classic works—can't but think that he has. For the broader public, interested in science and evidence based strategies and policies, and therefore reading Gladwell's book, the tipping point phrase, idea, concept, mechanism, or model is now, in the first place, associated to Gladwell—not to its inventors Grodzins and Schelling. In a vulgar-platonistic fashion one might say: To whom it is ascribed—a good idea doesn't care. However, this type of science communication undermines the scientific reward system.

But how to explain that *inside* science a model that initially was Schelling's tipping model, became known as Granovetter's threshold model? In the case of the differential recognition of Schelling's and Sakoda's model it seems clear that Schelling had the benefit of the Matthew effect. But if so, why not with regard to Granovetter as well? Schelling's tipping model was briefly described at the end of his *JMS* article and more in detail characterized and analyzed one year later in [Schelling, 1972b], i.e. six years before Granovetter's *Threshold Models of Collective Behavior* appeared. But this time advantage did not help in the "struggle for scientific recognition". Why?

Several factors seem to be relevant. *First*, in 1978, Granovetter was no longer an unknown. He had already published *The Strength of Weak Ties* [Granovetter, 1973], and *Getting a Job: A Study of Contacts and Careers* [Granovetter, 1974]. Both publications pioneered network theory in sociology by presenting an extremely fruitful application of it to an understanding of contact networks in the labor market. That pushed a new type of economic sociology and, at the same time, made Granovetter a leading figure in that field. In 1978, that process had already started.[222] Different from Sakoda, Granovetter was not a technical pioneer, developing tools that might be used to do social science. Granovetter used innovative theoretical tools to better understand a process that everybody considered important: getting a job. But, taking all that together, what does it mean with regard to the differential eminence of Schelling and Granovetter—the decisive difference for Merton's Matthew effect to set in? I do not know! *Second*, there is a difference between Schelling's *Discrimination Without Prejudice: Neighborhood Tipping* and Granovetter's *Threshold Models of Collective Behavior*. Granovetter's article is better. As already insinuated above, Granovetter's description of the model is a bit more formal, much more precise, and much easier to understand. Schelling's description, on the other hand, tends to confuse the reader about the actual meaning and re-interpretation of the x and the y-axes.[223] *Third*, Schelling's checkerboard model and the attention that it increasingly attracted, may simply have distracted attention from the other members of Schelling's family of models of segregation—for

[222] However, the start was not without complications: *The Strength of Weak Ties* was initially submitted in 1969 to the *American Sociological Review* under the title *Alienation Reconsidered: The Strength of Weak Ties*. It was rejected. A revised and retitled version with no reference to alienation was accepted and published in the *American Journal of Sociology*. The letter of rejection can be found at `https://scatter.files.wordpress.com/2014/10/granovetter-rejection.pdf`. The letter is *not* a clear cut demonstration of a reviewer's incompetence—as Granovetter himself seems to admit; see `https://scatter.wordpress.com/2014/10/13/granovetter-rejection/`.

[223] For evidence, the reader should do some self-experimentation by reading the decisive pages [Schelling, 1972b, 162–65] on the one side, and [Granovetter, 1978, 1424–27] on the other. As a personal remark I want to add that of all the articles of Schelling that I read, it is one, and only one, article, namely [Schelling, 1972b] with which I got into this type of trouble.

instance via some herding behavior with respect to attention, perhaps contingently driven by an attention related threshold dynamics as discussed above.

Was Schelling, with regard to his tipping model, a victim of the Matthew effect? Did other mechanisms work against him? A final answer can only be given based upon much more research on the history of the reception, resonance, and impact of what Schelling called the tipping model and now is known as Granovetter's threshold model.[224] As to me, I can't give an answer here.

7 How to Become an Unknown Pioneer? The Recipe and Some Concluding Remarks

Sixty years ago, in another of his seminal articles, namely *Priorities in Scientific Discovery: A Chapter in the Sociology of Science*, Merton analyzed in detail the role and function of priority in science [Merton, 1957]. His starting point is the great frequency of disputes over priorities in the history of science (e.g. Newton versus Hooke, Hooke versus Huygens, Cavendish versus Watt versus Lavoisier, St. Simon versus Comte—basically an endless list of major and minor debates [ibid. 635–37 et passim]). Merton argues that all these struggles with their typical vehemence, passion, and hot temper, can't be explained as owing to a selection of especially contentious men or especially egotistic personalities by the scientific recruitment system. Such propensities play a role, but often these controversies involve persons that are normally very modest. Often the struggle is fought not by the discoverers and inventors themselves rather than by their friends and followers [ibid. 638]. Therefore, a plausible explanation has to go deeper.

A scientist has best fulfilled his or her role when he or she "made genuinely original contributions to the common stock of knowledge" [ibid. 639]. By the publication of a new piece of knowledge a scientist produces a public good in exchange for a certain right: "the recognition by others of the scientist's distinctive part in having brought the result into being" [ibid. 640].[225] Priority matters seriously. The right to recognition of his or her achievement is the right of no person other than the scientist that was the very first—the winner takes all, and that premium for originality drives the advancement of science.[226] Merton cites Francois Arago, the permanent secretary of the French Academy of Sciences, who, in the 19th century, in the controversy between Cavendish and Watt argued that describing discoveries as having been made " 'about the same time' proves nothing; questions as to priority may depend on weeks, on days,

[224] The Schelling-Granovetter case presents itself for a bibliometric analysis that goes into the details of the citation history. The same is true for the Schelling-Sakoda case in general, but especially with regard to the significant increase of Sakoda citations in recent times.

[225] In later years Merton described that structure as "the seeming paradox that in science, private property is established by having its substance freely given to others who might want to make use of it" [1988, 606].

[226] For an analysis of this and alternative reward schemes cf. [Strevens, 2003].

on hours, on minutes" [cited after Merton, 1957, 658].[227] To the *right* to be recognized as the discoverer of an original new piece in the common stock of knowledge, corresponds a *duty* to recognize the achievement as *the discoverer's achievement* on the side of all others. This normative structure is an essential part of what constitutes the institution of science.[228] Violations of these norms are considered as an attack on the fundamentals of science as an institution—and that, according to Merton, causes the furor in so many controversies over priority.

Recognition comes in various and graded forms; Merton's enumeration and ranking is long:

> Largest in scale and shortest in supply is the towering recognition symbolized by eponyms for an entire epoch in science, as when we speak of the Newtonian, Darwinian, Freudian, Einsteinian, or Keynesian eras. A considerable plane below, though still close to the summit of recognition in our time, is the Nobel Prize. Other forms and echelons of eponymy, the practice of affixing the names of scientists to all or part of what they have contributed, comprise thousands of eponymous laws, theories, theorems, hypotheses, and constants, as when we speak of Gauss's theorems, Planck's constant, the Heisenberg uncertainty principle, a Pareto distribution, a Gini coefficient, or a Lazarsfeld latent structure. Other forms of peer recognition distributed to far larger numbers take further graded forms: election to honorific scientific societies, medals and awards of various kinds, named chairs in institutions of learning and research, and, moving to what is surely the most widespread and altogether basic form of scholarly recognition, that which comes with having one's work used and explicitly acknowledged by one's peers [1988, 619f.].

Ideally there is a harmony between priority and recognition:

> When the institution of science works efficiently, and like other social institutions, it does not always do so, recognition and esteem accrue to those who have best fulfilled their roles, to those who have made genuinely original contributions to the common stock of knowledge [Merton, 1957, 639].

As Merton knew, and our study of the Schelling/Sakoda case shows, the institution of science may not work properly. As to priority, Sakoda developed a first checkerboard model of social interaction at least two decades before Schelling independently developed his two-dimensional segregation model. In a certain sense Schelling's model is an instance of Sakoda's more flexible and more general 71-model. There was an early

[227] Merton does *not* approve Arago's view. He explicitly notes that, when criteria of priority are as finely discriminated as Arago and many others with him think, "then priority has lost all functional significance" [Merton, 1957, 658f.]. In his *Conclusion* Merton warns that the institution of science can get out of control "as the emphasis upon originality and its recognition is stepped up" [ibid. 659]. In the last column of the article, written with regard to misbehavior of individual scientists, we read the remarkable diagnosis: "The culture of science is, in this measure, pathogenic" [ibid. 659].

[228] For Merton's more general view cf. his early work *The Normative Structure of Science* [1942].

recognition of Sakoda's model (and his other work) in a small group of computational social scientists. But that recognition faded away while Schelling scored on all levels of Merton's ranking of forms of scientific recognition. Eponymy is "the most enduring and perhaps most prestigious kind of recognition" and it is "limited to the relatively few" [Merton, 1957, 644]. Schelling's two-dimensional segregation model made it, and became *the Schelling model*, which additionally generated and contributed to a very high number of citations (Merton's "basic form of scholarly recognition" [1988, 620]). At the same time, there was another member in Schelling's family of segregation models for which his originality *and* priority seem to be clear, namely Schelling's tipping model. But the lion's share of recognition for that model accrues to Granovetter—it became "Granovetter's threshold model," by the genitive not (yet?) a case of eponymy, but highly cited, and normally *not* accompanied by a co-citation of Schelling. Thus, an ultra-short and catchy summary of our case study could be: As to "their" models, Granovetter got the recognition for a model that essentially is Schelling's model; Schelling got the recognition for a model that is essentially Sakoda's model; and Sakoda was forgotten.

Obviously something went wrong. But did someone *do* anything wrong? Sakoda's 49-model was described in an appendix of an unpublished dissertation that was deposited in the Library of Berkeley University.[229] When Schelling started to work on models of segregation by the end of the 1960s, and given the search technologies at that time, he could not know about its existence. More precisely: Without already knowing about its existence, Schelling had no chance to find Sakoda's dissertation by a careful literature search. Therefore, in his RAND Memorandum—Schelling's earliest paper that presents and discusses what now is *the Schelling model*—Schelling simply could not cite Sakoda. At the same time, it is an indicator of Schelling's fairness, that right at the beginning of the chapter on neighborhood tipping, Schelling explicitly refers to Grodzins for having discussed the phenomenon already years ago [1969a, 73]. When Sakoda published in 1971 his *The Checkerboard Model of Social Interaction* in the very first issue of *JMS*, Schelling's RAND memorandum was already there for two years. But it can hardly be said that it was published in a serious sense. Obviously some copies, produced by a duplicating machine, circulated outside the usual academic publication channels—more a piece of grey literature. Similar to Schelling's situation with regard to Sakoda's dissertation, without already knowing about its existence, Sakoda had no chance to find Schelling's memorandum.[230] When Schelling's article *Dynamic models of segregation* appeared in the second issue of *JMS*, Sakoda's article *The checkerboard model of social interaction* was already published. But in all likelihood, Schelling's manuscript went into print (or, at least, into the first steps of the printing process)

[229] It is a serious warning against not publishing one's work, when Merton writes:

> Only when scientists have published their work and made it generally accessible, preferably in the public print of articles, monographs, and books that enter the archives, does it become legitimately established as more or less securely theirs [1988, 620].

[230] Schelling's article *Models of Segregation* [1969b], published in the American Economic Review is irrelevant in this context. In that publication Schelling discusses the one-dimensional spatial model and one variant of his bounded neighborhood model. Neither the two-dimensional spatial model, nor Schelling's tipping model is discussed. The RAND memorandum is not mentioned at all.

months before Sakoda's article really appeared as an available copy that was cataloged and could be read in an university library.

But what's about Bernhardt Lieberman (1927–2006), the founder and chief editor of *JMS* at that time. As Schelling reports, his *JMS* article was an invited contribution [cf. Aydinonat, 2005, 4]. In all likelihood the same holds for Sakoda's article. Lieberman must have held in his hands both manuscripts long before they went into print. Didn't he see the similarities? Why didn't he contact Sakoda and Schelling? Why no request to mutually refer to each other and to settle the priority issue? Lieberman died in 2006 following a car accident, and we can't ask him any more. But probably all these questions suffer from a kind of hindsight bias: What to date looks like very natural questions is distorted by later developments that misguide the perception of the early beginnings. In Lieberman's perspective, Schelling's *JMS* article was *not* the article that presented the Schelling model. It was an article that presented a whole bunch of more or less precisely described "mathematical" models. They all addressed a hot political issue of that time—segregation. For Lieberman, probably, Sakoda's article was about checkerboard societies, inhabited by two groups with specific attitudes to members of their own and to members of the other group. The interactional dynamics could be calculated by means of a computer that, though still expensive, in principle was affordable for a university. In such a perspective there is only little overlap in the two articles—too little to require an editor's intervention. Thus, measured by the usual standards for recognition by citation, none of the main actors—neither Sakoda, nor Schelling, nor Lieberman as the chief editor of *JMS*—did anything wrong in 1971.

Seven years later, in a part of chapter four of his *Micromotives and Macrobehavior*, Schelling again presents his two-dimensional segregation model [1978, 147–155]. Schelling does *not* refer to Sakoda's *JMS* article anywhere. Given his own RAND memorandum [1969a], and Sakoda's still unpublished dissertation, there was no obligation to refer to Sakoda under the priority rule. However, a reference to Sakoda would have been a reader friendly action that, at the same time, might have had a major effect on the future reception and recognition of Sakoda's checkerboard model. Neither does Schelling cite or refer to Grodzins in chapter three of *Micromotives and Macrobehavior* (*Thermostats, Lemons, and other Families of Models*), where he—very briefly—describes his own tipping model and puts it into the broader context of *critical mass phenomena* [1978, 91–110].[231] But both, not mentioning Sakoda, not mentioning Grodzins, is not an indicator of an unfair self-centered citation policy on Schelling's side—Schelling does not even hint to his own most relevant article on his tipping model, namely [Schelling, 1972b]. Probably that was all due to what Schelling considered a reader friendly design of the book: concentration on simple ideas and models, illuminating examples, minimizing of footnotes, no discussion of the literature. In terms of citations of the book, that policy paid off. In terms of recognition for his own tipping model, Schelling made probably a major mistake by not mentioning his earlier work. With respect to Granovetter's *Threshold Models of Collective Behavior* [1978], it was Schelling himself who obscured his priority. Now the more focused, more extended, and more comprehensive description of what essentially was Schelling's tip-

[231] There is only the summary remark that among earlier writers on tipping "the model was not explicit" [1978, 101]. And indeed, Grodzins had no explicit model of the tipping process.

ping model, was given by Granovetter, who himself explicitly recognized Schelling's earlier work. Arguably Schelling did not do Sakoda a favor that he could have done, and he made a mistake with respect to his own interest. But again, by 1978, none of the main actors did anything wrong in terms of violations of obligatory rules for the recognition of others' scientific achievements.

Were there "sinners" at all? Many contributed in many ways to the creation and perpetuation of some myths about the origins and inventors of a couple of models. It was an unintended effect of not reading (carefully) the original articles, copying the references of others, doing no independent search for similar models that might have existed already.[232] Of course, no one, writing on checkerboard modeling and not already knowing about the existence of *Appendix B* of Sakoda's dissertation, had a chance to find and access Sakoda's 49-model. But it was easy to find Sakoda's 71-model. Careless referencing is often due to the pressure to publish, and that under the pressure of time.[233] Surveys, introductions, handbooks are expected to give sound and comprehensive overviews. They should get it right as well in terms of priorities and assigning credit in an appropriate form. Thereby they should help to save time on the side of their readers. One should be allowed to trust them. However, sometimes and partially, that expectation turns out to be wrong.

Who started earlier with checkerboard models of social interaction: Was it Schelling, or was it Sakoda? This issue is definitely settled by our case study. But there is another *more general priority question*: who invented the very first checkerboard model at all? As to that question, taking a long view back is instructive. A certain checkerboard game, *chess*, has a centuries long history as a means to learn about societal life—*if* "learning" is meant in a *moral* rather than an explanatory or predictive sense. In the late medieval times it became common to use chess as an *allegory* for justified social stratification, the specific duties of social strata, or to teach critical lessons like that

[232] In a case study on the effects of poor referencing Anne-Wil Harzing puts together and motivates twelve guidelines for good academic references:

> 1. Reproduce the correct reference. 2. Refer to the correct publication. 3. Do not use "empty" references. 4. Use reliable sources. 5. Use generalizable sources for generalized statements. 6. Do not misrepresent the content of the reference. 7. Make clear which statement references support. 8. Check out the original—do not copy someone else's references. 9. Do not cite out-of-date references. 10. Do not be unduly impressed by top journals. 11. Do not try to reason away conflicting evidence. 12. Actively search for counter-evidence [Harzing, 2002, 128].

The list is *not* complete. An additional guideline should be: Do not use *unspecified* references. With "unspecified" I mean references that refer to a publication without stating the exact page(s) where the reader should look. In almost all cases, omitting pages number makes it difficult for the reader to profit from the references—and it makes it difficult to check references. That is an invitation to writers to insinuate support, where actually there is none, or only a very questionable one. Harzing's article has an instructive background. In 1995 she had written an article on the purportedly high expatriate failure rates. Harzing argued that in fact it was "a myth created by massive misquotations and careless copying of references". She reports that the former article was "borne out of sheer amazement and indignation that serious academics seemed to get away with something students at all levels were warned not to do" [Harzing, 2002, 127]. Since nothing improved (and the myth still persists), she wrote [2002].

[233] In a personal communication to the author, Georg Brun stressed additional factors: the pressure of editors and publishers to minimize words, pages, and in particular footnotes and lists of references. He thinks that these pressures actively discourage writers from handling questions of priority properly.

even a king “can’t do it alone”.[234] A late echo of this educational and allusive use of the game of chess can be found in the writings of Adam Smith (1723–1790). In his *Theory of Moral Sentiments* from 1759 we read:

> The man of system . . . seems to imagine that he can arrange the different members of a great society with as much ease as the hand arranges that different pieces upon a chess-board; ... but in the great chess-board of human society every single piece has a principle of motion of its own, altogether different from that which the legislator might chose to impress upon it. If those two principles coincide and act in the same direction, the game of human society will go on easily and harmoniously, and is very likely to be happy and successful. If they are opposite or different, the game will go on miserably, and the society must be at all times in the highest degree of disorder [Smith, 1759, 342f.].

Obviously for Smith playing around with a chess board and chess figures is a *seductive and deceptive* analogy for the design and control of social life. His point is *negative*, namely, human society does not work that way. The figures in the real “game of society” are autonomous agents, *not* just figures that can be arbitrarily positioned and directed—rather they will move *at their own principle of motion*. Smith’s *men of system* are legislators, regulators, social planers, social engineers, social designers of all sorts, that are completely wrong if they think that their problem is like configuring pieces on a chess-board. Via the chess-board analogy, Smith gives them a warning. Its negligence is a major cause for why so many (often well intended) policies fail.

However, moral instruction is different from scientific understanding of complex interactional processes generated by moves of agents that mutually react on (or even anticipate) their actions. And Smith’s use of a chess-board metaphor is obviously *not* a checkerboard model as Sakoda presented it 190 years later.[235]

But were there other checkerboard models, developed to better understand the *dynamics of social interactions* (whatsoever the type of social interactions), and that were earlier than Sakoda’s 49-model? *Very broadly* conceived, one might consider Torsten Hägerstrand’s (1916–2004) spatial models of innovation diffusion as a kind of checkerboard modeling: A geographical area is represented as a rectangular grid with a cell size of, for instance, 5km × 5km, inhabited by certain (empirically calibrated) numbers of agents. Innovation spreads by contacts. But contacts with close-by neighbors are more frequent than contacts with more distant ones. Hägerstrand developed

[234] A famous example in that tradition are the sermons of Jacobus de Cessolis, a Dominican monk in the 13th and 14th. century, and his book *Libellus de moribus hominum et officiis nobilium ac popularium super ludo scachorum* (Book of the customs of men and the duties of nobles or the Book of Chess). An easily available copy is [de Cessolis, 2008; originally ca. 1275].

[235] However, Smith’s warning together with Sakoda’s 71-model inspire and constitute a serious challenge that one might call *Smith’s checkerboard configuration challenge*: Under what conditions is it possible to design a socially optimal configuration of agents (e.g. “optimal” in utilitarian terms) on a checkerboard, that remains stable when agents, after they were configured by a planner, are free to move on the checkerboard based upon their attitude/utility matrix? The same question can, of course, be asked for Schelling’s model, here considered as an instance of Sakoda’s more general model.

and analyzed that model in his Swedish dissertation from 1953. Hägerstand's *Innovation Diffusion as a Spatial Process* [1967a] is the late (but not too late) translated publication of his dissertation.[236] Even if we accept (contrary to the secretary of the French academy of science in the 19th century) that inventions or discoveries may have been made "at about the same time," on the normal time scale for doing science, 1948–49 and 1953 is *not* at about the same time. Thus, compared to Hägerstrand's dissertation, Sakoda's priority as a checkerboard modeler is out of question.[237] It becomes more difficult, if we do not focus on *social* interactions *only*, and consider as well checkerboard based interactions of *physical* particles, parts, and states. An answer to the extended question requires a discussion of what now is called *cellular automata* and their early history.[238] The answer will be given in another article.

To sum up: I did a lot of search for checkerboard models of social interaction earlier than 1948–49 and did not find any.[239] True, non-existence claims are notoriously difficult to prove. However, in all likelihood, *Sakoda was the first social scientist* who developed a checkerboard model for a better understanding of social interaction processes. Sakoda's model was an *agent-based model* in the usual sense: single agents, individuals, actors (whatsoever the wording) are the smallest units, the atoms, of the model; a multitude of such smallest units interact in the model; each of the agents is equipped with a more or less complex decision module, set of rules, procedures (whatsoever the wording) for how to act in an actual situation. Already Sakoda's 49-model is, in a certain sense, a *simulation*. It is a sense that Schultz and Sullivan identified in a comprehensive early survey on simulations as "a convergence in meanings", namely simulation "as *the use of a process to model a process*" [Schultz and Sullivan, 1972, 4]. In that understanding simulations need not be computer based. To be a simulation, it is sufficient, that certain pieces (checkers, dimes, pennies, aspirins etc.) that represent agents, are manually moved on a checkerboard. As a matter of fact, Sakoda's original

[236] Cf. as well Hägerstrand's article [1965]. A very careful historical and systematic analysis of Hägerstrand's models of innovation diffusion is [Morrill et al., 1988]. Hägerstrand reflected very early on the perspectives for the use of computers in geography. In his view—besides descriptive mapping, and quantitative analysis—the most promising perspective was running "process models" to find out what is "the joint outcome over time and space when the units of behaviour start to interact and the behaviour of one unit influences the behaviour of others in a chain of events" [Hägerstrand, 1967b, 17f.]. Years before Schelling's segregation models, Hägerstrand's approach was used by Richard L. Morrill to model the spread of a ghetto in Seattle as "a spatial diffusion process in which Negro migrants gradually penetrate the surrounding white area" [Morrill, 1965, 348]. The article was later reprinted as [Morrill, 1972].

[237] One might doubt whether Hägerstrand's model is a checkerboard model at all. In Hägerstrand's model the cells represent a *physical* space, that can be inhabited by any number of individuals. In Sakoda's case the interpretation of the checkerboard as physical space is possible, but it is not the most natural interpretation. More natural is an interpretation as a *social* space: cells are possible network positions. As such they can be occupied by one and only one agent. In that view the checkerboard structure is a simplifying (but severe) restriction on logically possible neighborhood relations of a network.

[238] For a preliminary discussion cf. [Hegselmann, 1996a], [Hegselmann, 1996b], [Hegselmann and Flache, 1998], [Hegselmann, 2010. Reprint of Hegselmann 1996b].

[239] Cf. for helpful early overviews on early social simulations, the state of the art at that time, and the hopes and perspectives back then: [Guetzkow, 1962]; [Beshers, 1965]; [Dutton and Starbuck, 1971] and therein the comprehensive bibliography [Starbuck and Dutton, 1971] with more than 1,900 categorized titles; [Guetzkow et al., 1972]; [Bailey, 1978] with the bibliography [Anderson, 1978].

49-model was not computerized. Only later did it become a *computational* agent-based simulation model. As a checkerboard model of social interactions, Sakoda's 49-model seems to be the very first such model. As an agent-based social simulation model that, probably, holds as well. As a computational agent-based simulation model, Sakoda's 71-model was at least one of the earliest such models.

In short, Sakoda was *the* pioneering checkerboard modeler of social interactions. Additionally, he pioneered computational data analysis in the social sciences in general. He was the pioneering developer of a programming language designed for the special purposes of social scientists. It was a pioneer feat to establish and direct at Brown University the Socology Computer Laboratory (1962–1975), and later the Social Science Data Center (1975–1981). There were times in which Sakoda was well recognized in a certain, though at that time small, segment of the scientific community. In the 1960s, his model was the central reference model in some early books on computational social science. In 1995 Sakoda had an entry in Lee's book *Computer Pioneers*—a kind of hall of fame for computer and computing pioneers [1995, 599]. With the advent of affordable personal computers a lot of new, for the most part very young, self-educated, self-professed, and self-made computational social scientist, simply started their computational projects. The new generation was scattered over institutions and places, more a kind of grass-roots movement, rather than an orderly, "cooption" based extension of the small old community of computational social scientists. Among the new generation, Sakoda and his model could not even pass into oblivion—almost none of them will ever have heard of him. Sakoda became the pioneering computational social scientist that computational social scientists did not know.

If asked for a recipe for how to become an unknown pioneer, we might recommend the following:

1. Be brilliant, early, and exclusive: Have a good idea that almost all others can't pick up because they do not have the technical equipment or do not command it.

2. Be modest: Do not promote your model—even if obviously your time has come. Do not care about publishing, no strategic considerations, focus on the technical side, no publication campaign!

3. Trust the Matthew-Effect: There will be well reputed others that will reinvent or pick up your idea—and then it will spread.

As the recipe already suggests, I tend to think, it was *not unavoidable* that Sakoda became an unknown pioneer. What, if at the beginning of the 1980s Sakoda had written and published a booklet on his model, but now in a combination with instructions for how to program his model on a PC or an Apple Macintosh.[240] Imagine a booklet that would have given detailed advise for how to display a checkerboard on a screen and

[240] From David Lister's obituary we know that Sakoda was a Mac user. He writes:

> James's early experience with computers continued after he retired. He was firmly wedded to the Mac and quickly took up the challenge of diagramming models on the computer. He experimented with Coral Draw and Coral Paint which enabled him to compile

James M. Sakoda			Thomas C. Schelling
born in Lancaster, California	1916		
		1921	born in Oakland, California
with his parents to Japan	1933?		
return to the U.S., California	1939		
A.B. Psychology, Berkeley detention camp$_{–1945}$ JERS research assistant$_{–1947}$	1942		
		1944	A.B. Economics, Berkeley
		1945	U.S. Bureau of the Budget$_{–1946}$
checkerboard idea while fellow at Harvard	1948	1948	Marshall Plan (Copenhagen, Paris)$_{–1950}$
PhD thesis *Minidoka*, Berkeley assist. prof. psychology, Brooklyn College$_{–1952}$	1949		
		1951	Ph.D. Economics, Harvard White House and Executive Office of the President$_{–1953}$
assist. professor, psychology, Connecticut$_{–1962}$	1952		
		1953	associate professor, economics, Yale$_{–1958}$
computational turn, start working with FORTRAN			
		1958	senior staff, RAND$_{–1959}$ professor, economics, Harvard$_{–1990}$
NIH computer advisory board$_{–1965}$	1960	1960	*Strategy of Conflict* (paperback 1963)
			intensive activities as a government adviser on military questions
professor, dept. of sociology and anthropology, Brown University$_{–1981}$ director Sociology Computer Laboratory$_{–1975}$	1962		
		1969	professor, John F. Kennedy School of Government, Harvard$_{–1990}$ *RAND memorandum*
until 1971: about 20 articles on data processing, tests, learning experiments, fertility models, use of computers, DYSTAL Manual$_{1.0\ /2.0}$			*until 1971: more than 60 publications (including three books) on conflict, strategy, bargaining, arms race and arms control*
The checkerboard model of social interaction	1971	1971	*Dynamical Models of Segregation*
after 1971: about 10 articles on data processing, statistical indices, DYSTAL, FORTRAN; one article on CHEBO			*after 1971: about 120 articles on racial segregation and integration, arms control, addiction, self-command, health policy, global warming, energy*
director Social Science Data Center$_{–1981}$	1975		
		1978	*Micromotives and Macrobehavior*
retirement	1981		
2 last articles on JERS and the checkerboard model	1989		
		1990	professor, economics, Maryland$_{–2016}$
		1991	president, American Economic Association
death, afterlife as a paperfolder	2005	2005	Nobel Prize
		2016	death, afterlife as an eminent 20th century scientist

Figure 32: Timeline Sakoda / Schelling

how to get actors moving on the checkerboard.[241] Flowcharts could have shown procedures for neighborhood evaluations and migration decisions. Programming solutions in one of the general programming languages that became common in the 1980s, and that could be used on a PC or an Apple Macintosh like BASIC, (Turbo-)PASCAL, or C, could have been added. Sakoda would have published his booklet somewhere where it was visible for the new generation of computational social scientist. To make the booklet better known he could have written a short and inviting article in one of the computer magazines that started to appear in those days and that grew rapidly in terms of circulation figures (as, for instance *BYTE*). A handful of such articles would have been even better.[242]

If Sakoda had written such a booklet, had he started such a promotion campaign, then the history of computational social science could well have taken an *alternative* path: Sakoda could easily have become the pioneering checkerboard modeller who had invented *the Sakoda model*—an eponymy that could rapidly have evolved in the alternative 1980s. Of course, that is *contrafactual history*, it is historical speculation—as we *have to do it* in reflecting about alleged historical necessities, in identifying decisive causes of historical events, or in thinking about the relative strength of certain factors in historical developments. It works as well in history of science. In our case, as it seems, the alternative path requires only very little change of the actual path. Sakoda had to write a booklet on checkerboard modeling of social interactions, and how to do it on a MacIntosh or a PC. Additionally, the booklet should have been embedded in some campaigning in widespread computer journals. It is hard to imagine that Sakoda did not notice that with the advent of personal computers in the 1980s new times, basically his time, had come. He must have noticed that suddenly many more students, scholars, and other interested people could take up his checkerboard model of social

> his bi-monthly news sheet, "MacOrigami". This formed the greater part of his contributions to *FOLD*, but he also produced other separate copies for distribution to other paperfolders [Lister, 2005].

FOLD is an origami magazine.

[241] One has to keep in mind that—different from today—it was still a major programming task to generate, display, and control on a screen a checkerboard world with moving agents. A comfortable graphical user interface to choose group sizes and attitudes was practically out or reach. It would have required many hundreds and probably more than thousand lines of code. The setting of parameters was usually done within an initializing procedure, i.e. as a part of the program *code*. For a new parameter constellation the program had to be compiled again. It is a primitive solution, but in principle a checkerboard dynamics can be visualized on a screen as an alphanumerical output: crosses, zeros etc. represent agents, space is an empty cell; like a line printer, line by line the situation on the checkerboard is displayed on the screen; after a move of an agent, again line by line, the new situation is displayed. But a really good visualization of a checkerboard dynamics requires something else: display of a grid, and an ongoing "movie" in which filled circles, squares etc. of different colors or patterns move from one cell to another. What one would consider the core of the scientific problem, namely the dynamics driven by attraction, repulsion, or neutrality, would regard only a one digit percentage of the lines of code. Almost all of the program code would deal with problems of memory management and elementary graphical operations like drawing a line, a circle, or filling certain areas—and all that often by addressing the single pixels of a 640 × 200 (IBM CGA) or 720 × 350 (Hercules) pixel screen.

[242] At the beginning of the 1980s, courses on computational statistics became a compulsory component in the curriculum of students of the social sciences. This development would have supported the contrafactual publication campaign.

interaction (which always had been a major research program rather than simply "a model"). It is hard to think that Sakoda did not see that writing such a booklet, a kind of research invitation for the new generation with their new, visualization friendly computational devices, might make the difference.

But in reality, on the actual path of history, Sakoda did not write *the* booklet, and he did not start any promotion campaign for what might have become the *Sakoda model*. Why? He may have thought that via new compilers FORTRAN could become useable on the new computing devices. But Lehman's book *Computer Simulation and Modeling: An Introduction*, published in 1977, contained already a complete FORTRAN program of Sakoda's checkerboard model [1977, 254–304]. Additionally, Sakoda may have thought, when FORTRAN becomes useable on the new devices (PC, MacIntosh, and some others), that would help to spread DYSTAL, what then could help to analyze his model carefully. In 1979, on the occasion of the release of FORTRAN 77, Sakoda published an article with the programmatic title *DYSTAL 2: A General Purpose Extension of FORTRAN*. He starts saying that "FORTRAN is alive and is likely to stay that way in the foreseeable future" [1979, 77]. At the end he offers for sale his DYSTAL 2 manual and a tape with the source programs. Three years later, in 1982, the optimism is gone. In his very last article on DYSTAL, Sakoda writes about DYSTAL as a project of the past. At that time DYSTAL's use of FORTRAN as a host language had become a trap: The strength of DYSTAL were routines urgently needed by social scientists (e.g. string and list operations, statistical operations on lists like calculating means or standard deviations; cf. section 4.1). Technically, they were non-numerical procedures and recursive functions that did not exist or were not allowed in FORTRAN. But these extended features were depending upon dynamic storage allocation, which itself was reliant on the *equivalencing of different data types*.[243] But now the FORTRAN standards committee was hoping to eliminate equivalencing from core FORTRAN. Consequently, the host language concept would not work any longer. Sakoda considered that development as a serious mistake [cf. 1982b, 830]. It degraded DYSTAL to an example for what developers of FORTRAN *in the future* should try to achieve *within* the FORTRAN language. Looking backward and forward, Sakoda writes:

> My approach was that of an amateur, unaware of the niceties of computer language design, doing what appeared to be necessary to achieve features which FORTRAN did not normally provide. Much of this would not even be of historical significance, since DYSTAL was not widely used. But some of it is pertinent to the present-day effort to provide a more general-purpose language via FORTRAN [Sakoda, 1982b, 827].

In our context, it is important that Sakoda had an exclusive, life-long relationship with FORTRAN. In the 1950s he had begun to work on statistical programs in FORTRAN. In 1982 he writes that "since then it has been the only language in which I have programmed" [1982b, 827]. If Sakoda really thought that to make his checkerboard model known, programmable, and analyzable for a new generation of computational social scientists, with Lehman's description and FORTRAN code, all that could be done

[243] Cf. Sakoda's technical description in the early *DYSTAL Manual* [1964, 17ff.].

was already done, then that would have been a serious misjudgment. *First*, it was still true what Sakoda wrote of about FORTRAN in 1965: it is a language that "does not lend itself well to many of the kinds of programs the social scientist desires" [1965c, 31].[244] As to DYSTAL, a programmer had to have a good command of FORTRAN to really utilize the extended features of DYSTAL—and that, probably, was the decisive disadvantage of DYSTAL.[245] *Second*, the most important point about the new computation devices was their display, the screen. That allowed for the first time a comfortable visualization of the dynamics on the checkerboard. But nevertheless, instructive visualization was still a tricky programming issue. Sample solutions for how to do that would have been extremely helpful. Lehman's description of a FORTRAN implementation of Sakoda's model didn't provide any help with such a visualization. In short, the booklet, that was to be written on the alternative path of history, had to be written using one of the *new* higher programming languages, that were easier to learn, better structured, had already integrated some of the DYSTAL features,[246] and—most importantly—made it for the first time possible to program impressing visualizations.[247] As an experienced programmer, Sakoda would have been fast in learning one of the new programming languages. However, even for Sakoda, who had retired from Brown University in 1981, it would have meant a serious investment of time and effort to learn the new programming language, and then to write the booklet. May be he considered that as a bit too much, and perhaps a bit too late. May be, after his retirement, he saw himself primarily as an Origami artist with gardening as his second "obsession".[248] May be it was a question of personality as well. Based upon his encounters with Sakoda during a some weeks long workshop on computers in the social sciences in the middle of the 1970s, Ronald E. Anderson describes Sakoda's as

> quiet but very pleasant; brilliant but modest. For fun, he would make

[244] Lehman writes about FORTRAN that it is "in many ways a poor choice for writing simulations, but it is probably the closest thing that we have (or are likely to get for some time) to a universally available computer language" [1977, 19].

[245] Right at the beginning of his first manual on writing programs in DYSTAL, Sakoda states frankly that "it assumes a basic knowledge of FORTRAN programming" [1964, i]. Then, some pages later in the introduction, he turns it positive: "DYSTAL employs a syntax which not only provides a powerful language, but also is not difficult for a FORTRAN programmer to learn" [ibid. 1]. That was true. But the problem was the first step—learning FORTRAN.

[246] For instance, dynamical storage allocation in TURBO PASCAL.

[247] Probably the best candidate for that purpose would have been TURBO PASCAL (TP). In the critical times, i.e. in the middle of the 1980s, TP became a quasi standard for programming courses. It was widespread among self-educating programming novices. TP was available for several operating systems (MS-DOS, CP/M 86, CP/M) as they were used by IBM, Apple and others. Version 1 was released in 1983 and retailed for $49.99. "Turbo" was meant as a hint to the high speed of the compiler. TP's Integrated Development Environment (IDE) was revolutionary. No longer writing of program code, compiling, and linking of other components had to be done by switching between different applications. TP very soon got graphics tools that allowed for movie-like dynamical visualizations. But the use of these tools had to be learnt. Sample solutions for typical problems of checkerboard modeling and visualization would easily have saved a checkerboard novice hundreds of hours of learning it the hard way. See also footnote 241 above.

[248] By reading Sakoda's letters in his exchange with Hansen one can get that impression. Art Hansen generously sent me a copy of the correspondence between Sakoda and him. The correspondence starts in preparation of the interview in February, 1988. By reading Sakoda's letters I got a much better understanding of Sakoda's personality. See also footnote 147.

> origami figures with paper while sitting in discussions ... He did not try to promote his own work [personal communication, August 2012]. [249]

Basically, I do not know why Sakoda did not write a booklet on how to implement his checkerboard model on the new, visualization friendly computing devices using one of the new, comparatively easy to learn, and visualization friendly programming languages. At least the late Sakoda, was interested (again?) in his model, his old and still unpublished dissertation, and his contribution to the JERS study that, via the dissertation, had led him to invent the very first checkerboard model of social interaction. Sakoda's two very last scientific publications were both contributions to Yuji Ichioka's collection *Views from Within: The Japanese American Evacuation and Resettlement Study* [1989a]. In his first contribution to the book, Sakoda describes his reminiscences as a participant observer [1989b]. Sakoda's second contribution (the last in the book) is *The "Residue": The unresettled Minidokans, 1943-1945* [1989a]. This last article is the first occasion where Sakoda published at least a summary of his dissertation as it was deposited—at that time 40 years ago—in the library of the University of California at Berkeley in 1949. As in the dissertation, there is again an appendix that describes his checkerboard model, now with improved graphical figures. But, somehow, that was a publication in a remote study on another old study on some evacuation and resettlement that, as the book title suggests, had involved Japanese Americans—long ago and far away. In terms of making his checkerboard model known among computational social scientists, it was not much better than no publication at all. Probably none of the new (or old) computational social scientists will ever have come across Sakoda's first summary article on his dissertation and the early checkerboard model therein. As a matter of fact, Sakoda remained the unknown pioneer.

Personally Sakoda was still convinced of his model. On August 10, 1988 (the second day of Hansen's interview with Sakoda, and, by coincidence, the day President Reagan signed into law the Civil Liberties Act with its apology for what the U.S. government had done to its Japanese American citizens during World War II[250]) he says in a very self-confident, and at the same time very appropriate, retrospective judgement:

> Of all the things I've done, I think the best thing I've done is the social interaction model, which solved the problem in social psychology of going from the individual level to the group level [Sakoda in Hansen, 1994, 417].

It sounds a bit like regret when Sakoda then continues to say that, from an academic point of view, the model "is worth pursuing, although that, again, I didn't do too much with. I wrote one article, and that was it. But I could have pursued that a little more" [ibid.].

When Schelling died on December 13, 2016, his death was much-noticed by the international press. For *The Washington Post* it was the death of "perhaps the most

[249] Anderson refers to an NSF funded workshop on computers in the social sciences in the summer of 1973 or 1974; it was an 4–6 weeks workshop that took place in Boulder, Colorado.

[250] Cf. footnote 36 for Sakoda's reaction.

important economist and social scientist of his generation".[251] When Sakoda had died in 2005, the only obituary was the one written by David Lister on behalf of the British Origami Society. In terms of public perception, a brilliant paperfolder had passed away [cf. Lister, 2005].

Probably that perception will change, and some scientific recognition will be reallocated. Rectification of misattributions and the corresponding misallocation of recognition, is not the least important task of history of science.[252] In fulfilling such a task, a study like this is in a certain sense *self-undermining*—at least that is the hope: if my study is right, then Sakoda was the pioneering inventor of checkerboard models of social interactions. If the story spreads and becomes accepted, then that has effects in terms of citations, references, and other forms of recognition—Sakoda, the unknown pioneer, becomes a known and recognized pioneer (again).[253] Hansen concludes his interview with the following remark to Sakoda:

> I first read your dissertation many years ago [in 1973], liked it very much, and wanted to meet you. I imagined then what you looked like, and the way in which you had responded to situations at Minidoka. . . . I think you made the principled and correct choice when you told Dorothy Thomas

[251] *The Washington Post*, December 13, 2016. Other obituaries appeared in *The New York Times* (December 13, 2016, B14), *The Economist* (December 24, 2016, p. 90), in *The Sunday Times* (January 6, 2017)—to mention a few.

[252] Co-citations seem to suggest that a mild reallocation of recognition in favor of Sakoda started already as a consequence of pointing to Sakoda in the articles [Hegselmann, 1996a], [Hegselmann, 1996b], [Hegselmann, 2010. Reprint of Hegselmann 1996b], [Hegselmann, 1998], and [Hegselmann and Flache, 1998]. But the hints to Sakoda in these articles were very general. The relation to Schelling's model was not understood. Almost all details as presented in this study were not yet known to me. In several talks that I gave on the Schelling/Sakoda case over the last years, partially in front of a large auditorium of computational social scientists, I could present more and more details and explanations. That, of course, contributed to a process that now is making Sakoda a known pioneer. One of the consequences is a growing number of Sakoda citations. In January 2017 Google Scholar finds already 205 citations of Sakoda's *JMS* article.

[253] In January 2017, Pablo Medina, Eric Goles, Roberto Zarama, and Sergio Rica published in Complexity the article *Self-Organized Societies: On the Sakoda Model of Social Interactions* [Medina et al., 2017a]. For the first time the eponym "the Sakoda model" appears in the title of an article. The article discusses and analyzes the simulation results for all 45 possible attitude combinations for two groups, given that $\{+1, 0, -1\}$ are the only possible attitude values (cf. for the 45 possible combinations already [Sakoda, 1978, 363] and p. 75 above). While in Sakoda's 71-model the whole world matters, the authors reduce for their central results the relevant interaction neighborhood to the 3×3 Moore neighborhood (as it is the case in Schelling's model). By extensive simulations the authors identify the attractors and give an overview. With regard to that, the article is a promising start for a systematic analysis of Sakoda's model. Two critical remarks: First, the authors imply that the model that was published in Sakoda's *JMS* article, is the model that Sakoda developed in his Ph.D. thesis [cf. Medina et al., 2017a, 1]. A reading of *Appendix B* of Sakoda's Ph.D. thesis where he describes his early model, does not support this assumption. The 49-model is *not* driven by attitude matrices (cf. p. 41 above). Second, the authors claim that Schelling's model "is only a special class already included in the original Sakoda model" [ibid. 1]. With regard to Sakoda's 71-model that is true. However, the authors forgot to mention that, on request of one of them (Eric Goles), I had sent to them the slides of my keynote lecture to the European Social Simulation Conference 2014 (Barcelona). The slides included the detailed demonstration of section 2.3 above, that, in a certain sense, Schelling's model is just an instance of Sakoda's model. In a *Corrigendum* to their article, Medina, Goles, Zamara, and Rica add this information in their acknowledgements; cf. [Medina et al., 2017b].

> that you would not to let go of the theory part of the dissertation just to gain her assistance in getting it published. I think it's certainly true that the empirical part of your dissertation is what makes it interesting to a wider public, but the theory part is really what relates the events at Minidoka to social science, that makes it not simply about Japanese Americans, but about people in certain types of situations. So I think that *someday* it will see some form of publication. You may not be around to see that come about, but I hope you are [Hansen, 1994, 446, emphasis added].

It may be that this "someday" is coming now.

Acknowledgements

This long study evolved over more than 20 years. I have accumulated many debts in that time. In the academic year 1994/1995, during my time as a fellow of the Netherlands Institute For Advanced Study in the Humanities and Social Sciences (NIAS), I gave a talk on cellular automata based models of social dynamics and mentioned Schelling's model. After the talk, a Dutch colleague whom I did not know, never met again, and whose name I do not recall, pointed to another checkerboard model of a certain Sakoda, published as well in the *JMS*. That was the start. I send my deepest gratitude to that unknown colleague (in the hope that he is still around, though he was an old man already then). The friendly and ready to serve librarians of NIAS procured the article. Sakoda's article refers to *The Spoilage* and to Sakoda's dissertation *Minidoka*. The NIAS librarians finally found a copy of *The Spoilage* in the Netherlands. But they could not help to access the dissertation. Even without access to the dissertation, it was very clear that Sakoda was a pioneer of checkerboard modeling—and that was stressed in the early articles [Hegselmann, 1996a], [Hegselmann, 1996b], [Hegselmann, 2010. Reprint of Hegselmann 1996b], [Hegselmann, 1998] and [Hegselmann and Flache, 1998]. In the late 1990s, I obtained a xeroxed copy of *Minidoka* by Richard J. Gaylord. I'm deeply indebted to Richard for this gift. But then, for about a decade, I could not continue the project. At the University of Bayreuth I developed, established and directed a BA- and MA-program Philosophy & Economics. Little time was left for research. I invested that time into research on opinion dynamics, especially the so called bounded confidence model (see [Hegselmann and Krause, 2002], [Hegselmann and Krause, 2006]). In 2009, I was invited as a visiting professor at the Katholieke Universiteit Leuven. Without any teaching obligations, I could fully concentrate on a programming project in social epistemology. It combined the bounded confidence opinion dynamics with an ongoing network formation among members of different epistemic groups. The program allowed two types of network formation: one in the Sakoda style (driven by attitudes), one in the Schelling style (driven by preferences). By writing that program I stepwise realized that Schelling's and Sakoda's model had the same structural components. It became very clear that Sakoda's model was much more general, much more flexible—and that Schelling's model could be translated into a Sakoda-style model. In the end, some thousands lines of DELPHI code were simply superfluous. That was learning it the hard way.

It was Igor Douven, who had invited me to the formal epistemology group of the KU Leuven. Without our discussions and without the calm and peaceful atmosphere of my flat and garden in Leuven's Grote Beginenhof I would never have learnt that lesson. For that I want to thank Igor Douven and the Belgian tax payers. Convinced of the "superiority" of Sakoda's model, I found it more than puzzling that Schelling's model had become a classic while Sakoda's model was unknown. As a consequence, I started with research on the historical details of computational social science. It helped a lot that university libraries all over the world are now throwing out their 50 or more years old books on computers, computation, computational social science etc. As a consequence, to date it is a cheap pleasure to establish one's own private library of early computational social science. Normally the postage for getting the book from an antiquarian bookshop is more expensive than the book itself. Access to old working papers, often manually duplicated, is more difficult. In 2012, Thomas Schelling sent to me his last personal copy of his *On Letting the Computer Help With the Work* (and he agreed to publish it in JASSS). One page of Schelling's copy was not completely readable. Mathias Risse helped to find another copy in the library of the John F. Kennedy School of Government. That allowed to reconstruct and repair the missing part. As librarians and archivists, Andrew Creamer and Jennifer Betts (Brown University), and Timothy H. Horning (University of Pennsylvania) helped with information on Sakoda and D. S. Thomas. The Harvard University Archives found out when exactly Sakoda had spent a term at Harvard. In personal communications, Ronald Anderson, Thomas Schelling and James Vaupel contributed historical details. I owe a lot to Arthur A. Hansen: He xeroxed and sent to me his complete exchange with Sakoda before and after the long interview in 1988. It included Sakoda's CV with a list of his publications, research projects etc. as Sakoda had sent it to Hansen in preparation of the interview. As an expert on the history of Japanese Americans during World War II, Arthur Hansen corrected many details that were wrong in my first draft of the relevant sections.

Over the years, at different times and occasions, many colleagues and friends encouraged me with their interest in my study. Some put a helpful pressure on me "to get it written". Many gave advise on special problems or pointed out open questions. Sometimes short remarks and critical looks made me re-think what I had thought or written. Here, in alphabetical order, the list of people that in this way had an impact on my study: Frederic Amblard, Robert Axtell, Michael Baurmann, Gregor Betz, Matthew Braham, Jennifer Bradham, Helmut Büttner, Jake Chandler, Rosaria Conte, Rainer Cramm, Andreas Diekmann, Igor Douven, Corinna Elsenbroich, Andreas Ernst, Hartmut Esser, Andreas Flache, Werner Güth, Stephan Hartmann, Dirk Helbing, Catherine Herfeld, Sylvie Huet, Paul Humphreys, Paul Hoyningen-Huene, Jürgen Köhler, Ulrich Krause, Wim Liebrand, Sebastian Lutz, Daniel Mayerhoffer, Johannes Marx, Margaret Morrison, Werner Raub, Thomas Schelling, Gerhard Schurz, Flaminio Squazzoni, David Stadelmann, Friedrich Stadler, Klaus Troitzsch, Oliver Will, Richard Zeckhauser. I'm most grateful to Georg Brun who has read all of the manuscript. His comments helped to clarify a lot of passages.

I gave talks that involved or directly dealt with Schelling, Sakoda, and their respective models to audiences in Amsterdam, Ascona, Bamberg, Bayreuth, Guildford, Greifswald, Kassel, Leuven, Lüneburg, Munich, Salzburg, Vienna, and Zurich. Additionally, I had the honor to present the Schelling-Sakoda case in keynote lectures to the Social Simulation Conference SSC'2014 (Barcelona) and to the International Conference of the German Society for Philosophy of Science GWP.2016 (Düsseldorf). The questions and comments that came out of all these audiences have greatly improved this study.

During my time at the University of Bayreuth, Jörg Rambau, Sascha Kurz, Torsten Eymann, and I held for a number of years a joint research seminar on the *Efficiency of Decentralized Structures*, later re-titled to *Information and Control*. At different stages in its development, the components of this study were tried out in our interdisciplinary seminar. That forced me to clarify many thoughts. I would like to thank all participants, colleagues, and PhD students, for their questions, comments, proposals—and patience. Additionally, Jörg Rambau invested many hours of his time to explain LaTeX to me. If the reader of this study finds the layout reader-friendly, then that is for the most part Jörg Rambau's merit. Matthew Braham corrected my English, convinced me to give up my excessive use of italics, and proposed the actual subtitle.

Sections 2 and 4 of this study were drafted while a senior fellow at the Alfried Krupp Wissenschaftskolleg in Greifswald during the 2012–13 academic year. Major parts of section 3 were written in spring 2016 during my time as a fellow at the Center for Advanced Studies (CAS) of the Ludwig-Maximilians-Universität München. Both institutions were a perfect environment to think things through and write them down.

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