The Undaunted Psychologist

ADVENTURES IN RESEARCH

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This book was set in Palatino by The Clarinda Company.
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They All Look Alike to Me

The data are always right, but they are not always the right data.

Lots of people think they are good at remembering faces. Names may be a problem, but faces are memorable. Of course, you may find yourself at a social gathering staring at someone you are just sure you’ve met before, but you have no recollection of when or where. But most difficult is when you stare blankly into the face of someone who has just said “Huddsfipple (your name)—I wondered when we’d meet again!”—and you have absolutely no memory for this person. But if you think these situations could be upsetting, let me tell you about an acquaintance of mine who was consulting for a well-known international agency. When she arrived in the exotic foreign location she was met by a member of the agency staff who escorted her to a hotel. She checked in, and he left her to get settled, have a nap after traveling, etc. When she went to the coffee shop she spotted him in the lobby, and waved hello. He didn’t respond, and she was surprised. She took a stroll near the hotel, and again saw and greeted him. Again he did not respond. As she walked on, puzzled at his behavior, she looked up only to see him again! But this time he was dressed very differently. It was then it dawned on her that as a European in an Asian city for the first time, she was a victim of one of the oldest clichés in the book: “They all look alike to me!”

She was an experienced traveler, and was able to have a good laugh at herself for not having figured it out sooner. This is only one of the many forms of interpersonal perception and behavior in which we respond to people in terms of their group identification rather than their individuality. It is interesting, not only in its own right, but also because it represents a larger group of phenomena that are perhaps more central to intergroup identity and conflict: stereotyping and other forms of ethnocentrism. It is intriguing because it is not easily explained. A small group of researchers has been working at it for more than 20 years with some interesting findings, but no definitive explanation to show for it. This chapter is a brief description of portions of my work on this problem and how it has developed over the last two decades.
HOW DID IT ALL START?

Inquiry is driven both by theory and by the need for practical understanding. Research can take very different shapes depending on what drives it.

During the spring of 1968 a colleague and I were part of a community-action group in the Midwest. We were evaluating the examinations used to test applicants’ qualifications for positions in such agencies as the fire and police departments. The town had a substantial number of black citizens, but the fire and police departments did not. We were trying to assist in the qualification of blacks on the fire and police examinations by writing training materials, and by attempting to identify any blatantly discriminatory aspects of the examinations.

We had done this for the fire department examination, and turned our attention to the police exam. The police department was cooperative, and set us up in a conference room. When we opened the examination to the second question we had a surprise. There, spread across two pages, were the photographs of eight men. Next to each photograph was a variety of facts about it. Eight minutes were given to the applicant to study this information. When we turned to the next page, there were photos of four of the eight men, and questions about the information previously given about them. All the faces displayed were white.

On the face of it (sorry) there was something discriminatory about this test when given to blacks. We thought it would be very likely that black applicants would have more difficulty recognizing white faces than black faces. We had each heard our share of racist remarks about blacks, and prominent among these was the comment that they all look alike. But that was pretty easy to understand. In the late 1960s, before school integration was widely implemented, before discrimination in housing and real-estate sales was diminished, and before black faces began to appear more widely in advertising and other media, most whites interacted with very few blacks at all, and rarely on anything like an equal basis. So there would be little personal experience as a basis for learning about them as individuals rather than as more or less equivalent examples of a category. It seemed likely that blacks’ perception of whites would work the same way, and that they would have jokes about how all those whites look alike. Anyway, it seemed very likely that this particular test item would be more difficult for black than for white applicants, and therefore was an instance of racial discrimination.

As we left the police station I volunteered to go to the library to find the relevant literature documenting this problem. We agreed we would consider what to do once the nature of the evidence was known. At the library I was absolutely unable to find any literature bearing directly on this problem. I decided it must be my own incompetence in the library; so
I sent my graduate assistant to find the relevant studies. He found none. Then together we convinced a couple of undergraduates to try. They reported only one marginally relevant study, but it was one more than we had gotten. We set out to design research that would give us some information on the problem. The basic question was, of course, whether there really was differential recognition for own- vs. other-race faces.

Before going on I ought to comment on the concept of “race” and how it ought to be understood in this area of research. There seems to be no good and consistent way to refer to all the various “races.” To refer to a white person as “Caucasian” appears to give credence to a theory of race that has Europeans deriving from a population living at one time in the area of the Caucasus mountains. Identifying modern individuals with historical geographical populations appears to assume a kind of stability of population and within-group marriage/mating that may characterize some areas of the world well, but others not at all. Whether or not English, Poles, and Italians interbreed in Europe, people with these origins do so with great frequency in the USA, as do people of more diverse geographical origins. Similarly with “blacks.” Whites in the USA tend to call anyone with discernibly “African” features “black,” but in other parts of the world many more categories are used. So the old racial names just don’t seem to work, especially in complex multiethnic societies. The color names also will not work, at least not consistently. While “black” and “white” are in popular usage, “yellow” will simply not do for Asians, and “black” just does not differentiate sufficiently between subgroups. “Hispanic” describes language and not even much of culture, not differentiating between Central American aboriginals and European Chileans. National “extraction” also provides difficulties. What do you call an American whose father was German/Italian, and whose mother was English/Chinese, especially if his appearance is somewhat Asian? For purposes of research on facial recognition, a typology of facial appearance might be useful. Maybe the real issue is what “traditional” group a person’s appearance fits into. But then a large number of people may be unclassifiable. This problem has not been solved in a satisfying way. We have to acknowledge it, and get on with the inquiry about facial recognition—even if we have to communicate by using some not so terribly appropriate terminology. But in doing so, we have to be sure that when we translate our research findings into inferences about the real world we do not forget the convenient fictions we accepted in order to get on with it!

**NAILING IT DOWN: THE FIRST STUDY**

There were important things to do. First was to obtain a suitable sample of faces to use in the research. Second was to recruit subjects, and third
was to design an appropriate memory task. Fourth, although I didn’t
know it at the time, was a problem of the index of recognition that we
would finally use. Research of this nature usually can’t be done by indi-
viduals working alone. Undergraduate students often play important
roles in the planning and implementation of research projects. On this
project we had support for undergraduate participation in the research
from the National Science Foundation, which paid the students’ project
expenses and provided them stipends.

We obtained photos from a variety of sources in Illinois and at
Howard University, in Washington, D.C., where Jerry Kravitz, a friend
from graduate-school days, was on the faculty. Kravitz had more experi-
ence with studying memory than I did, and was a good person to consult
about the structure and design of the memory task for the research. My
initial inclination was to stay very close to the form of the police exami-
nation that started us on this line of research. That would make it a
“paired-associates” task, where faces and information about the person
(face) would be associated. The test could be to produce the facts associ-
ated with each face, recognize which facts were associated with a partic-
ular face, or vice-versa.

But there would be a problem with interpretation. When the data
were in, it would not be clear whether the findings had to do with recog-
nition of facial images, with the association of verbal information with
the stored representation of the images, or both. Kravitz counseled that,
for sake of clarity, a simple recognition task should be used. This seemed
like good advice, and so the first experiment used a simple recognition
task. We first showed subjects 20 faces (10 white, 10 black) mixed semi-
randomly (no more than 3 black or 3 white faces could appear in
sequence). Then we showed them these 20 faces mixed in with 60 more
(30 white, 30 black, again mixed semirandomly). Each time a face was
displayed we asked the subjects to answer “yes” or “no” to “Was it one
of those shown in the first set?” (of 20).

We conducted this experiment both at Illinois and at Howard. When
we were through we wrote up a basic report on the study and circulated
it among a small group of researchers who we thought might be
interested, and who might offer us their critical comments. We pre-
ferred to get the first round of comments from friends rather than from
a journal editor’s anonymous reviewers! The responses to our paper
brought a very interesting surprise—one that has been influential for
me in many ways. Harry Hake, an experimental psychologist of consid-
erable reputation, returned the paper with a number of helpful com-
ments. And at the end of the paper he penciled a note asking whether I
knew about “signal-detection theory.” He also offered the opinion that if
I published the study as it was, the human-engineering types would eat
me alive.
I knew nothing about signal-detection theory, and frankly the phrase “human-engineering types” scared the hell out of me. These guys know a lot of technical stuff, and I didn’t see that in my future. At the same time the prospects for continuing ignorance didn’t seem so great either, so I got on the phone to Harry. He offered to come to my office and discuss it with me over a bag lunch.

Before going on, I want to emphasize the social aspects of professional life. I have learned a lot of interesting and important things from friends and colleagues over lunch and at social occasions of various kinds. Discussing ideas in a social setting is central to professional success. Do it early, and do it often.

Harry Hake did indeed come to my office, and gave me the first lesson in what has become a major interest for me—the response-decision processes that interpose between whatever it is we know personally and the actions we take on the basis of this knowledge. We often say more, less, or different from what we know, and we do so to serve our own interests. Think what the world would be like if we always said just what we have seen, thought, or remembered! In a face-recognition experiment, if we think it is important to identify as many as possible of the faces we are asked to remember (called “hits”), we will probably be willing to say “yes” to a face that looks even remotely familiar. As a result, we’ll probably make some mistakes—saying yes to faces we actually didn’t see before (called “false alarms”). So both hits and false alarms increase because we are more willing to say yes. If we really wanted to avoid making false alarms—as we might to avoid a mistaken identification of an innocent person in a police lineup—we would want to be much more careful about saying yes, and probably say it less often. So both hits and false alarms would decrease. But our ability to tell faces we saw before from faces we didn’t see before probably would not be different. Only our willingness to say yes would be different. So if we want to know about recognition accuracy apart from people’s willingness to say yes in the experiment, we need a way to adjust the accuracy score for the false-alarm rate. Signal-detection theory does that (and much more—but that’s another story).

We reanalyzed our data using signal-detection-theory procedures, and what seemed like mildly confusing results were cleared up. The results and their interpretation seemed pretty obvious. First of all, there was a difference in recognizability between the white and black faces used in the experiment, for both the groups at Howard University and at Illinois. White faces were more often recognized on the average for both groups of subjects. In addition, there was a “statistical interaction” between the “race” of the faces and the “race” of the subjects, indicating that faces were better recognized by subjects of their same race.
Now what did we really have? We had a perfectly good experimental result that partially confirmed a conjecture (we can hardly call it an observation) from "real life." We had at least dragged this idea into the domain of scientific study, but two problems remained. First, we did not have a good explanation for the finding. Second, while many theories might be used to help understand this phenomenon, it was not going to be their testing ground.

The second of these—that the finding of a cross-race recognition differential would not likely be the testing ground of theories—is an important illustration of how "science" works, and deserves further comment. The primary focus of psychological research is testing propositions derived from theory, and modifying existing theory in accordance with the research findings. Development of new theory is a related activity. Theory-testing takes place in a context of well-defined techniques and methods. Theories understandably focus on problems that lend themselves to investigation and measurement in ways that are relevant to the theory. As a result, the questions that receive the major focus of research and theory do not necessarily represent the questions "out there" awaiting explanation. At any time, many interesting phenomena and processes exist that are the focus of no theory. Therefore, they are either not investigated at all, or they are investigated as a function of their importance in contemporary social events. If existing theory addresses these phenomena they may be "brought into theory." If not, their understanding may have to wait for the development of new theory. In the meanwhile, researchers busy with their own theory-driven research can hardly be blamed for not getting excited when someone says, "Yeah, but your theory can't explain my favorite phenomenon." While investigating a phenomenon that is interesting in its own right but not generated by existing theories one can feel slightly homeless.

Anyway, we began our investigations of the basis (cause) for differential recognition across race in what might be called the quarter-finals approach so well known in sports. We assembled the major contending explanations and started elimination rounds. We began with the most obvious potential explanations and set out to collect data that would allow us to choose among them.

The first study was based on the idea of "communication accuracy," which had been well worked out in the (then) newly flourishing field of language and cognition. Communication accuracy is the idea that those objects that can be more reliably described and identified from their verbal descriptions should be more reliably recognized visually. According to the theory, this is because verbal descriptions become part of the
memory code for objects, and later help to trigger their memory. Observers see an object in a recognition experiment, generate a linguistic label or description of the object, and the label activates associations with other objects or concepts. Later, when the observer sees the label, the previously generated associations help to elicit the object’s image. The more reliable or consistent the verbal description, the better the visual recognition. We knew from a previous study that there were differences in the verbal descriptors used by blacks and whites to refer to facial features. So we reasoned that if verbal descriptions were important in facial recognition, these differences might explain the differences in facial recognition.

Henry Lavigneur, David Weldon, and I designed experiments in which subjects received different amounts of training in describing faces. More verbal-description training ought to make their descriptions more reliable, and subjects ought to get better at recognizing faces on the basis of a verbal description. So we designed a test of communication accuracy to detect whether verbal training was having this effect. Subjects also ought to get better at recognizing faces—particularly other-race faces. So they were also tested for recognition of own- and other-race faces.

The results were interesting and informative. Verbal-description training did substantially improve the communication accuracy of face descriptions, but it had no effect on visual recognition, for either own- or other-race faces. The absence of a verbal–visual relationship surprised us. We thought that perhaps face recognition might just be very difficult to improve through a relatively short training program. But in a second experiment a relatively short series of trials in which subjects were punished (by electric shock) for recognition errors brought own- and other-race recognition to the same (high) levels. Improvement was possible, but verbal training didn’t get it done.

It appeared that cross-race recognition has something to do with the outcomes of subjects’ experience with the facial images, but that verbal processes weren’t an important part of it. Therefore differences in verbal references to own- and other-race faces could not be used to explain differential recognition. These results told us something about where not to look for an explanation of cross-race face recognition: just what the “elimination rounds” approach does in the scientific process.

Other research programs have produced similar findings. This creates an important practical problem. If the police want to construct an image of a wanted criminal, the obvious way to do it is through the descriptions of witnesses and their verbal comments on attempted constructions. But attempts to create such images are notoriously unreliable. My conjecture is that it’s because they all depend on verbal access to the subject’s facial memory, but facial memory does not use verbal categories to any great degree. We appear to recognize faces in very short spaces of time, and we are aware of a face’s identity before we are aware of its descriptive attributes.
Other studies followed, looking for other “obvious” explanations of the recognition differences in terms of well-known and important concepts. For example, we examined the relationship of racial attitudes to face recognition. If whites and blacks possess negative attitudes toward each other, racial attitudes might explain differential recognition. But we were unable to find any relationship between intergroup attitude and face recognition, in a number of studies. Other researchers have also failed to find this relationship. Our inability to find a satisfactory explanation in existing theory has been one of the things that has kept my interest in this work at a high level for many years. In many ways, it’s a mystery story! Another reason is its practical implications.

**TAKING IT TO COURT**

We began this line of research after observing what we believed to be a racially biased test item on a police-qualification examination. From there we dug deeper into psychological theory and research until we had clearly described the differential recognition phenomenon. Later on, Bob Bothwell, Jack Brigham, and I examined a series of studies in our laboratories and elsewhere and found that differential recognition (often called the “cross-race” effect) was a stable phenomenon, applying to blacks and whites alike. We did not yet fully understand the basis of the cross-race effect, but there it was, facing us (sorry, again), as real as anything. So when I was first asked to go to court as an expert witness in a trial of a black man identified by a white man I had a sense of returning to the community from which I had derived this interesting problem that had been so good to me professionally.

Good to me professionally? I ought to explain that. How can a phenomenon like differential face recognition be good to an academic psychologist? It’s simple, really. This phenomenon is inherently interesting, and it gives me a chance to work on something challenging, something that draws from a wide range of theoretical approaches and areas of psychology. But even better, it has implications for the real-world from which it was originally derived. That’s a nice, diverse, but integrated package. Beyond that, however, I have been identified with the cross-race effect, since the paper I published with Kravitz in 1969 was the first (and hence “classic”) study on this problem. Since the phenomenon is memorable, so is my connection with it. One should not underestimate the degree to which that opens doors, helps to begin conversations, leads to invitations to attend conferences, participate in symposia, present papers, review manuscripts and grant proposals, and many other things that enrich the life of an academic psychologist. The way it all began seems like a happy accident. But my work on this problem since that
time has been very rewarding, both personally and professionally. That's how it's been good to me.

But back to court! There are some difficulties involved in testifying in court about cross-race identifications. First, it is not the role of an expert witness to make judgments about whether the identification in a particular case is or is not valid. Rather, it is the expert’s role to assist the jury in their evaluation of the evidence—including the accuracy of the particular identification involved. So the expert can testify only to findings in general. Therefore, before one testifies one ought to be satisfied that there is a basis for testimony (that the own-race/other-race differential actually exists), and that the basis generalizes over populations studied, measurement techniques, and conditions of observation.

If one is satisfied on these counts, it seemed to me, one could make a real contribution. If the haunting problem of mistaken eyewitness identification is being increased by errors of cross-race identifications, it makes an already bad situation worse. Further, if it is true, as most would suspect, that white identifications of black suspects would be far more numerous than black identifications of white suspects, a note of social injustice is also added to the problem. Surely it would be a good thing to contribute our knowledge to the courts to prevent as many errors as we reasonably and practically can.

For cross-race recognition the issues are sometimes less than straightforward. We have found that a reasonable interpretation of cross-race recognition difficulty is that people are less able to distinguish other-race faces from each other. That is to say, other-race faces appear subjectively more similar to each other than they would to an own-race observer. This has two important implications.

First, when white witnesses sift through a set of black mug-shots in the process where witnesses “nominate” possible suspects, they may be more likely to identify an innocent person who looks similar to their recollection of the offender. Since these faces will appear subjectively more similar to each other, such mistakes are easily understood. At this point in the process, then, other-race nominations may be more likely to incriminate an innocent person. Also, if the identification used by the police was a “show-up,” where the witness is offered a single individual for identification, there are circumstances that could easily produce an identification error. If the suspect was chosen on the basis of a good verbal description, one would understand if the suspect was similar in appearance to the offender, and also if the witness confused one for another.

Second, in some circumstances an other-race lineup might be “fairer” than an own-race lineup. The fairness of a lineup is related to the similarity of the lineup members to each other. In a chapter published in 1983, Trish Devine and I showed that, with the limitation that they must not be look-alikes, greater similarity generally means a fairer lineup. So if the
lineup is otherwise appropriately constructed, the increased subjective similarity due to the lineup members being of an other race than the witness would appear to make the lineup fairer than would be true for an own-race lineup. This would be a better protection for the innocent suspect, but at the same time it might work against the identification of a guilty suspect.

Attempts to actually take our work on the cross-race effect to court met with limited success, but from my point of view it had some interesting side-effects. One day in the late 1970s I had a phone call from a public defender in a northeastern state who had read of our work. He asked if I would testify in a case that would shortly go to trial. After we discussed the case, what questions he would ask me and what my responses would be, he did invite me to offer my testimony as an expert witness in the trial of a young black man accused of assault and robbery. My testimony was to center on the question of whether white witnesses would be less able to make an accurate identification of a black offender/suspect—whether an innocent black suspect would be more likely than an innocent white suspect to be wrongfully identified as the offender. My experience as an experimental psychologist active in studying this matter qualified me, at least potentially, as an expert who could assist the jury in determining the facts of the case. But first, the judge had to decide whether the jury would be allowed to hear my testimony.

To make a long story short, the judge heard my testimony in the absence of the jury (the normal procedure) and then decided that the jury would not hear it (not an unusual result). The trial judge has great discretionary latitude in such matters, but there are certain standard issues that structure the decision. One issue concerns the probative value of the testimony—whether the information an expert would contribute would actually assist the jurors in their attempt to decide what the facts of the case actually are (for example, whether or not the witness’ identification was made in error). Another is whether the information the expert would contribute comes from an established and recognized field of knowledge. And still another is whether hearing testimony from an expert would tend to influence the jurors to give excessive weight to what the expert had to say. There are plenty of factors for the judge to balance, and also lots of room for the judge’s personal evaluation of the issues to enter the decision.

I happened to see the judge later that day in the hallway, and I was very curious as to his reasons for disallowing my testimony. He came over to me and said he thought our research was very interesting. When I asked him why, then, he had not allowed my testimony, he began to give his legal reasoning (having to do with its probative value). I stopped him and said yes, I understood that, but I was curious about why he came to view my work as lacking in probative value. He observed that all my studies had used photographs of faces, projected as color slides on
a screen, and asked what that had to do with what happens to some poor
guy outside a bar at two in the morning (referring to the case in which I
had testified). I had to acknowledge that this was a good point.
Whether laboratory results generalize very far beyond the laboratory
was an unanswered question. Later on, as a direct result of this interac-
tion with the judge, Trish Devine and I addressed this problem in a
series of studies in eyewitness identification. We did a series of "staged
crime" studies to explore differences in eyewitness identification deci-
sions in laboratory and "apparently real" settings. We found some inter-
esting and unexpected things. For one, it appears that if you can manipu-
late the beliefs witnesses have about the consequences of making an
identification you can modify their willingness to make an identification
in a lineup. For example, if witnesses are led to believe that the people
running the lineup think the offender is actually present, they will be a
lot more willing to choose someone from the lineup and say he is the
offender. Of course in our system of justice the question of who is the
offender is a matter for the jury to decide. It is a bit premature to make it
the basis for witnesses' willingness to make an identification!

An unexpected finding concerned the witnesses' belief in the severi-
ty of punishment. We thought that most people would be particularly
careful about making identifications if the consequences of mistaken
identification were particularly severe. Nobody wants to cause great
injury to others, at least not by mistake! But in a study where our "exper-
imental vandal" had destroyed scientific equipment belonging to a visit-
ing lecturer in front of an audience, members of that audience were
unwilling to make identifications if the consequences for the offender
would be trivial, while many were quite willing if the consequences were
severe. They appeared to be quite upset by the "crime," and willing to
assist the police so long as he wouldn't just get his hand slapped!
Reference to these studies is in the "suggested readings," if you're inter-
ested. At the same time, research documenting a cross-race recognition
differential in natural social environments has yet to be done.

Still, there were other possible explanations that seemed interesting,
and we pursued a number of these in our laboratory. One promising
possibility, suggested by June Chance and Alvin Goldstein, was that
people may naturally look at own-race faces in a way that connects with
existing information in each person's cognitive system. That information
may have to do with judgments about the person's personality (e.g., their
honesty, how friendly they are). In contrast, other-race faces may be seen
in a more superficial, less-connected way. There was already evidence
that if subjects were directed to consider aspects of a face requiring cog-
nitive elaboration and social inferences (personality judgments, for
example), recognition would be better than if subjects were directed only
to superficial information (like whether the person is white or black;
whether the nose is big or small).
The idea was worth checking. Trish Devine and I did a study in which we manipulated whether subjects were oriented to inferential (e.g., honest or dishonest) or superficial (e.g., black or white) attributes of own- and other-race faces. We expected an orientation to complex attributes to result in a decrease in the cross-race recognition difference. It didn’t happen. Even though orienting to superficial facial information does hurt recognition, it appears people spontaneously look at all faces in an inferential way regardless of race. So, we still had to look elsewhere for an explanation. And in the meantime, with many studies being reported which searched for possible relationships between other-race face recognition and either social attitudes or personal cross-race experience, these perennial favorites still did not come through as the explanation we were looking for.

**TOWARD A THEORY OF FACE RECOGNITION AND SOCIAL EXPERIENCE**

Restricted social experience with other-race persons has always been the favorite explanation for the cross-race effect. And that makes good intuitive sense: how can you learn to tell “them” apart if you don’t know many of “them”? For this reason it is particularly surprising that since the very first study, subjects’ reports of how many other-race persons they know (and how well they know them) have been unrelated to recognition for their faces. It is possible that there are problems with the sample of subjects involved (e.g., extremely little cross-race experience, and little variation) or it could be that we simply are not asking the right questions.

Brian Mullen of Syracuse University suggests that information about small minorities (e.g., Asians, in most northeastern cities) is actually processed cognitively in different ways than information about large minorities or majorities (e.g., women or men). Mullen and his colleagues have tried to explain the cross-race effect on this basis. My own view is that we have not asked the right questions about experience. For example, people generally don’t know much about the differences among U.S. nickels unless the differences are important. Similarly with people. If it is useful to differentiate among other-race individuals we will find ways to do it. For example, as I think back to my first days in school I have a couple of vivid memories of the school playground and “recess.” Recess, as I remember, was not supervised to any great degree. There were mean kids there, older and bigger than I was. I can remember a couple of them very well, even now, nearly 50 years later.

If the mean kids wore black hats, like the villains in old cowboy
movies, we wouldn’t need to know anything more than the color of their hat to know who’s what. But when there are no other markers that indicate who has good things for you, who will tease and humiliate you, who will protect you, who will hurt you, who will be friendly and cooperative, it is important to be able to identify people as individuals so you can remember whether they are one of the good guys or not. I think that’s why we’re pretty good at recognizing members of our own groups. Likewise, when you can identify entire categories of people that are socially irrelevant, or which can be categorically avoided with little personal or social cost, then there is little need to learn how to distinguish among them as individuals. In the absence of black hats, other social-category markers will do. This is roughly what I think is the important aspect of intergroup social experience, at least as far as intergroup face recognition is concerned. To study this idea we ought to examine the ways in which people have personal significance for each other.

This is the problem on which I will focus much of my own energy in the years ahead. I am hoping that my recent paper dealing with this issue will provide a platform for further research in cross-cultural contexts where the variations of intergroup experience may allow us to get a better look at this interesting but yet unexplained phenomenon.

If you’ll pardon one final personal digression, this line of thinking about social experience relates to another of my early memories. My father collected coins, nickels in particular. He was always waiting for the one to turn up that would make us millionaires. He examined every nickel that crossed his palm. He would provide anyone who would listen with lots of details about the different types of nickels. He never found that nickel.

They all look alike to me.

SUGGESTED READINGS


—— & —— (1983). Measuring the fairness of eyewitness identification line-


