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Voting Power Measurement: A Story of Misreinvention

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ABSTRACT

In this account of the history of voting-power measurement, we confine ourselves to the concept of a priori voting power. We show how the concept was re-invented several times and how the circumstances in which it was reinvented led to conceptual confusion as to the true meaning of what is being measured. In particular, power-as-influence was conflated with value in the sense of transferable utility cooperative game theory (power as share in constant total payoff). Influence was treated, improperly, as though it were transferable utility, and hence an additive and distributive quantity. We provide examples of the resulting misunderstanding and mis-directed criticism.

Voting Power:
A Story of Misreinvention

1 Introduction

The purpose of this paper is to highlight some of the historical milestones in the evolution of the theory of one of the fundamental concepts in social choice: *a priori voting power*. We concentrate on foundational conceptual issues – the nature of a priori voting power and how it ought to be quantified – rather than on technical matters. Our historical survey will run to the end of 2002,[1] thereby updating, correcting some details and expanding on the historical comments we made in our book [24]. However, due to considerations of space we decided to omit here some topics – mainly the (so-called) voting-power paradoxes – that were covered extensively in that book.

As the subtitle of this paper suggests, the development of the theory of voting power was bedevilled by two phenomena. First, the same ideas were reinvented several times over, because researchers were often unaware of relevant work published earlier. Second, the meaning and implications of some of the basic concepts were widely misunderstood. As a result, during much of its 57-years history as an academic discipline, the evolution of the theory was tortuous, with several fits and starts.

1.1 Early history

As far as we know, the first person to be concerned with the measurement of voting power was Luther Martin, a Maryland delegate to the 1787 Constitutional Convention held in Philadelphia. Martin was worried that the voting power of the large states in the US House of Representatives would be disproportionately too large compared to that of the small states, assuming that the representatives of each of the 13 states would always vote as a bloc. In a pamphlet published the following year he not only exposed the fallacy of equating voting power with weight (in this case size of a voting bloc), but made an attempt – albeit unsystematic and somewhat crude – to measure voting power. See Riker [50].

Martin's approach is broadly based on the notion of what we have called 'I-power' (see Section 3 below). But the measure implicit in his argument apparently relates the voting power of a voter a just to the number of minimal

[1] References [32] and [41], which were published later, were available to us in 2002 in mimeograph form.

winning coalitions to which a belongs; and therefore seems to us closest to Holler's index (discussed below in Section 7) rather than to that of Banzhaf (Section 4) as suggested by Riker, or to that of Deegan and Packel (Section 7) as suggested by Straffin (see [50, p. 294, fn. 2]).

We concur with Riker that it is significant that this first contribution was made not by a mathematician but by a politician, showing that 'power indices are not merely mathematicians' fancies but obvious categories of thought for practical politicians.'[2]

1.2 Political-historical context

The case of Martin illustrates a general point: the importance of the political historical context as a cause of the waxing and waning of interest in the measurement of voting power. It is not the only factor – among others there are the internal logic of the subject itself, as well as developments in closely related fields – but an important one.

As Riker notes, Martin's interest in the problem of voting power was motivated by the the worry that all Representatives of each state would vote as a bloc, and that large states would combine against small ones. He thus initiated the study of weighted voting. In the event, his worries proved unfounded: 'party, region, and economic interest have been much more compelling points of difference than mere geography. So Martin's model turned out to be politically irrelevant and was forgotten.'[3]

L S Penrose, in turn, was evidently motivated by the establishment of the UN and the hopes – widespread at the time – that it would evolve into a 'world government'. The dashing of these hopes with the onset of the Cold War may partly explain why his work was not taken up by others and was forgotten.

We know of no external stimulus for the revival of the subject by Shapley and Shubik. It was most probably a natural outgrowth of Shapley's work [52] on cooperative game theory. But Banzhaf – a lawyer – became interested in the subject because of its evident relevance to the principle of 'one person – one vote', arising from the interpretation of the Fourteenth Amendment to

[2] [50, p. 294]. Although Martin's intuition that a voter's a priori chance of winning in a vote is not generally proportional to the voter's weight was correct, it should be pointed out that in the particular case which was his concern he had in fact no grounds for complaint. It so happens that if the delegates of each of the 13 states in the US House of Representatives would have indeed always voted as a bloc, then in the resulting weighted voting game, $[33; 10, 8, 8, 6, 6, 5, 5, 5, 4, 3, 3, 1, 1]$, the Penrose–Banzhaf voting power of each of these state blocs would have been almost exactly proportional to its weight.

[3] [50, p. 294].

the US Constitution, which was then an important topic of public and legal discourse. In particular, the US Supreme Court's rulings in *Baker v Carr* (1962, regarding state legislative districts) and *Wesberry v Sanders* (1964, regarding Federal congressional districts) mandated that the worth of each person's vote should, as much as possible, be equal to that of any other person's vote. Banzhaf decided to comment critically on the possibility of implementing the Supreme Court's rulings by means of a system of weighted voting.

However, subsequently the US Supreme Court, in the cases of *Whitcomb v Chavis* (1971) and particularly in the case of *Morris et al v Board of Estimate of City of New York et al* (1978), rejected the idea of weighted voting and mandated that legislative districts be of equal size. This led to a decline in interest in the measurement of voting power in the US. (For details about these matters see our book [24, Ch. 4].)

In Europe, however, interest was rekindled; this was no doubt at least partly due to the subject's direct relevance to the weighted voting rule used in the European Union's most important decision-making body, the Council of Ministers. In particular, the controversy surrounding the re-weighting at the time of the Union's last enlargement (1995) impelled academics (if not politicians) to study the problem in a scientific way. This scientific activity has received fresh impetus from the prospective further enlargement of the EU: the subject of voting power has become once more a political 'hot potato' during the period leading to the 2001 Nice Treaty, in which a new system of weighted voting has been adopted for an (enlarged) EU.

2 L S Penrose

As far as we know, the first properly scientific discussion of voting power is the 1946 paper [45] by L S Penrose,[4] in which he proposes a probabilistic measure of absolute voting power. Although this measure is defined in the context of a special class of decision rules – 'decisions made by [ordinary] majority vote'[5] – it is clearly much more general, and can be applied to a

[4]Lionel Sharples Penrose (1898–1972) studied mathematics at St John's College, Cambridge; awarded degree of Moral Sciences Tripos (Cambridge, 1921); did a medical course at St Thomas's Hospital, London, and was awarded the degree of MD, (1930). His main research was in genetics as well as psychiatry. Together with his son, the famous mathematician Roger Penrose, he invented the Penrose triangle and the Penrose stairs, impossible objects popularized by the artist M C Escher.

[5]Initially he assumes one vote per voter; but then goes on to admit bloc votes of several voters acting together. So the decision rules he considers are, in effect, weighted voting

very broad class of rules:

> In general, the power of the individual vote [*sic*] can be measured by the amount by which his chance of being on the winning side exceeds one half. The power, thus defined, is the same as half the likelihood of a situation in which an individual can be decisive – that is to say, a situation in which the remaining voters are equally divided upon the issue at stake.[6]

Here Penrose posits what he calls 'random voting' on the part of the other voters, who are assumed to be 'indifferent'. Although he does not define this explicitly, it is clear that he is using the a priori Bernoullian model, in which voters act independently of one another, each voting 'yes' or 'no' with equal probability of $\frac{1}{2}$; so that if n is the number of voters then all 2^n possible divisions of the set of voters into 'yes' and 'no' voters are equally probable.[7]

In the first sentence of the passage just quoted, Penrose *defines* the voting power of individual a to be $r_a - \frac{1}{2}$, where r_a is the probability of a 'being on the winning side' – that is, of the event that the outcome of the division goes the way a votes. In the second sentence he states a *theorem* which says, in effect, that the voting power as defined by him is equal to half of what later became known as the *absolute* or *unnormalized* 'Banzhaf measure of voting power'. (However, the final clause – 'that is to say...' – holds only for the ordinary majority rule.) He states the theorem without proof, as something that is almost self-evident. (Indeed, it is quite easy to prove.) We write this as

$$r_a - \frac{1}{2} = \frac{\psi_a}{2}$$

and refer to it as 'Penrose's identity'.

Next, he presents some numerical results concerning the power of a ' "resolute" bloc' of voters 'who always vote together', while the remaining voters are 'an "indifferent" random voting group'.

He then turns to the main theme of the paper: a two-tier voting system, such as 'a federal assembly of nations' – a reference to the newly established United Nations – in which a set of constituencies of different sizes elect one representative each to a decision-making 'assembly of spokesmen'. He argues that an equitable distribution of voting power in the assembly is the *square-root rule*, according to which

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rules with quota set at half, or very slightly over half, of the total weight.

[6] [45, p. 53].

[7] In what follows, whenever we speak of probability *simpliciter*, we mean a priori probability in this sense.

> ... the voting power of each nation in a world assembly should be proportional to the square root of the number of people on each nation's voting list.[8]

In his 1952 little book [46] – a mere 73 pages – Penrose ranges over a great variety of topics, among which is that of voting power. Compared to the 1946 paper, there are several significant innovations, of which we shall mention three.

First, in the book's Preface he offers an interesting justification of the aprioristic assumption of 'indifference' or 'random voting'.[9]

Second, he modifies the definition of voting power, making it now *double* what it was in the 1946 paper, that is, $2(r_a - \frac{1}{2}) = 2r_a - 1$:

> In general, with a large crowd voting at random on one of two alternatives, the power, P, of a bloc of n_R voters may be measured by doubling the proportion of decisions in excess of 50 per cent. which accord with its wishes.[10]

This amended definition is equivalent precisely to that of the so-called absolute Banzhaf measure ψ.[11]

Third, in the Appendix he outlines a proof of an approximation formula for the voting powers of voters under a weighted voting rule (with quota set at half the total weight), provided the number of voters is large and the weight of every voter is small compared to the total weight. Implicit in this formula is an important theorem about the limit behaviour of weighted voting rules: as the number of voters increases (while existing voters retain their old weights) then – subject to certain conditions – the ratio between the

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[8][45, p. 57]. Although he does not say so explicitly, it is clear from the context that he is assuming representatives always vote according to the majority opinion in their respective constituencies.

[9]'In discussing the question of voting I have introduced the assumption of indifference which does not correspond closely with the facts as usually understood. However, since the future decisions of an elected representative cannot be easily predicted, voting is much more at random than it appears to be to the voter. Thus, the general theorems which can be established by using the assumption may be more often valid than might at first be supposed.'

[10][46, p. 7]. The assumption that the other voters are 'a large crowd' [of individuals with one vote each] is not needed for the definition itself but for the approximation formula for P, based on the Central Limit Theorem of probability theory, which Penrose states immediately following this definition.

[11]Penrose does not offer any explanation for the shift in his definition. In fact, his 1946 paper is not mentioned in the 1952 booklet, although it is listed in its Bibliography.

voting powers of any two voters approaches the ratio between their weights.[12]

Thus Penrose did not only lay the foundations for a mathematical theory of voting power, but got quite far in developing the theory. His work should have been seminal; but the seed fell on stony ground. His ideas were largely forgotten and many of them were subsequently re-invented by, and credited to, others. Until the late 1990s his name is hardly mentioned in the mainstream literature on voting power.[13]

We believe – and propose to show in this paper – that the long period of amnesia had a detrimental effect on the development of the theory for almost half a century: it was not only delayed, but distorted and confused.

3 Shapley and Shubik

Shapley's 1953 paper [52] defines and characterizes a 'value' for cooperative n-person games. Although Shapley himself does not claim this explicitly, he seems to imply that this Shapley value (as it came to be known) is a predictor of a player's expected relative share in a fixed prize equal to the worth of the grand coalition. At any rate, this is how the Shapley value is widely interpreted by game theorists (see, e.g., Myerson's textbook [44, p. 436]).

Shapley and Shubik (S&S) propose in their 1954 paper [54] an index of a priori voting power which is a direct application of Shapley value to so-called *simple [cooperative] games.*

Since Penrose's work was unknown to most readers of S&S, the latter were credited with inventing the theory of measurement of voting power. Because S&S presented the theory as a branch of cooperative game theory (CGT), it was generally accepted as such. In particular, voting power was equated with a voter's expected relative share in some 'prize', the *total payoff.* What is that total payoff? Shapley tells us very clearly:

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[12] The proof outlined by Penrose is faulty: the conditions he assumes are not sufficient. On the other hand, the result does hold for a large variety of cases, many of which were not envisaged by him. This topic is currently being intensively researched; see [41].

[13] The exception that proves the rule is Morriss [43, p. 160], which gives him full credit, but does not belong to the mainstream. Morriss (private communication) recalls that he found out about Penrose's work from Fielding and Liebeck [30, p. 249], which also does not belong to the mainstream, where Penrose is given credit for the square-root rule, but not for his measure of voting power. This attribution of the square-root rule is cited also in Grofman and Scarrow [33, p. 171], which does belong to the mainstream.

> ... the acquisition of power is the payoff.[14]

The view of voting-power theory as a branch of CGT was credible to mathematicians and others, partly in view of the fact that the binary decision rules, which are the most basic structures to which any such theory must apply, are indeed the same – structurally speaking – as the simple games in the sense of CGT.[15]

This CGT view of voting power was further reinforced by S&S's emphatic claim – italicized by them – that

> *any scheme for imputing power among the members of a committee system either yields the power index defined above or leads to logical inconsistency.*[16]

This claim is in fact false;[17] but why should readers – particularly non-mathematicians – suspect this, given the great authority of S&S?

The intuitive, pre-formal notion underlying the S-S index was quite different from that underlying Penrose's measure. To facilitate our discussion, let us step out of the historical sequence and introduce here a useful (if somewhat inelegant) terminology proposed in 1998 by Felsenthal et al. [29, p. 101].

By *I-power* we mean voting power conceived of as a voter's potential *influence* over the outcome of divisions of the decision-making body: whether proposed bills are adopted or blocked. Penrose's approach was clearly based on this notion, and his measure of voting power is a proposed formalization and quantification of a priori I-power.

By *P-power* we mean voting power conceived of as a voter's expected relative share in a fixed *prize* available to the winning coalition under a decision rule, seen in the guise of a simple cooperative game. The S&S approach was evidently based on this notion, and their index is a proposed quantification of a priori P-power.

As Penrose's work was unknown or forgotten, the notion of P-power dominated. I-power was largely either totally ignored or – worse – conflated with P-power.

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[14] [53, p. 59].

[15] The family of all such games was described precisely and classified by Shapley [53].

[16] [54, p. 789].

[17] As evidence for their claim S&S refer in a footnote to the uniqueness proof in [52]. But this is – at best – misleading: the proof in [52] applies to the class of *all* cooperative games, but not to that of *simple* games, because it is not closed under the algebraic operation of game addition. In 1975 Dubey [20] gave a correct unique characterization of the S-S index; but one of the conditions postulated by him – Dubey's axiom – is by no means compelling for a measure of voting power.

In hindsight it seems that even without knowledge of Penrose's work it ought to have been immediately realized, or at least suspected, that whether or not the S&S approach is valid, it cannot possibly be the only way for formalizing voting power. The tell-tale difficulties or anomalies of an exclusive P-power notion include the following, in increasing order of importance.

First, so-called 'improper' simple games make no sense in CGT (except as components of composite games that are proper) but can serve as decision rules for certain – admittedly fairly special – purposes.

Second, the CGT approach to the measurement of voting power does not allow for abstention as a rational option. But in some important decision rules (e.g. that of a permanent member of the UN Security Council) abstention plays a special role, not reducible to voting 'yes' or 'no', and is often resorted to.

Third, the CGT approach, which presupposes bargaining and binding agreements prior to the actual play, makes no sense for secret voting. But voting power surely must be meaningful in such cases.

Fourth, the S-S index is inherently a purely relative measure: it can only quantify the relative share of a given voter in the sum total of the power (or whatever quantity it measures) of all voters under the given decision rule. It says nothing about the absolute power of a voter – the concept itself is pretty meaningless in the S&S approach. For example, intuitively it seems clear that a member in a committee of n members ($n > 2$) that uses the unanimity rule has a smaller – or at any rate a different – amount of absolute power than a voter in a committee of the same size that uses the ordinary majority rule. But the S-S index assigns to the voter the same value, $\frac{1}{n}$, in both cases.

Fifth, in CGT the total payoff – whether or not it is 'the acquisition of power', as Shapley asserts for voting games – is a private good divided exclusively among the winners. But this makes no sense in many, perhaps most, decision-making situations, where the outcome of a division is not the acquisition of power or any other private good by the winners, but a public good (or public bad) that may benefit (or harm) all the voters and others, possibly in an open-ended way (i.e., not a constant sum).[18]

It was this last point that was eventually picked up by Coleman, as we shall see in Section 6.

[18] This is clearly the case in the UN Security Council, which is one of the two examples cited by Shapley in [53, p. 59] where allegedly 'the acquisition of power is the payoff'.

This is quite apart from the fact that Shapley misrepresents the decision rule of the UNSC as a [binary] simple game. In reality, it cannot be so represented because abstention by a permanent member has a different effect from both a 'yes' and a 'no' vote. On this, see Subsection 10.1 below.

It is worth mentioning here that the S-S index can also be arrived at via a completely different route and obtained as a measure of I-power (rather than of P-power, as originally obtained by Shapley and Shubik). This was shown in 1977 by Straffin [55] (see also [56]), who derived this index under his 'homogeneity assumption'. However, as shown in [29], when obtained in this way the S-S index, albeit a measure of I-power, is not a truly a priori measure, as it assumes in effect that the voters act like identical clones (see also [24, pp. 206ff.]).

4 Banzhaf

The contribution of John F Banzhaf III [2, 3, 4] and his impact on the issue of weighted voting in the US is discussed at length in our [24, Ch. 4], to which the reader is referred. But some facts must be mentioned briefly here.

Banzhaf certainly re-invented some of Penrose's most important ideas. His general approach is indeed quite similar. However, his definition of voting power, and his justification of it, went through a gradual evolution.

In his first paper (1965) Banzhaf does not speak in probabilistic terms but purely in terms of the 'number of voting combinations' (i.e., frequency of divisions) in which a given voter can affect the outcome. In fact, in [2, p. 326] he pointedly disclaims any probabilistic assumptions:

> No assumptions are made as to the relative likelihood of any combination... .

However, without such an assumption he is unable to justify assigning equal weight to all combinations.

In this paper Banzhaf does not actually propose, let alone use, the measures of voting power – absolute and relative – attributed to him. The measure he does propose is the crude Banzhaf *score* (aka 'Banzhaf *count*'); and he does so merely in passing. What he is mainly concerned with is the *ratio* of the power of one voter to that of another.[19]

> In fact, ... the appropriate measure of a legislator's power is simply the number of different situations in which he is able to determine the outcome. More explicitly, in a case in which there are N legislators, each acting independently and each capable of influencing the outcome only by means of his votes, the ratio of the power of legislator X to the power of legislator Y is the same

[19] This was to remain his chief concern throughout.

> as the ratio of the number of possible voting combinations of the entire legislature in which X can alter the outcome by changing his vote to the number of combinations in which Y can alter the outcome by changing his vote.[20]

In his second paper (1966) Banzhaf repeats essentially the same definition of his measure as in [2] (the number of combinations in which a given voter can alter the outcome) but now qualifies this measure as 'relative'.[21]

This time he uses quasi-probabilistic terms in order to justify assigning equal weight to all combinations:

> Because *a priori* all voting combinations are equally possible, any objective measure of voting power must treat them as equally significant.[22]

In the third paper (1968) there is a further shift. The language is more overtly probabilistic:

> If all voters have an equal chance to affect the outcome in a given voting situation, we say that they have equal voting power. ... Since, a priori, all voting combinations are equally likely and therefore equally significant, the number of combinations in which each voter can change the outcome by changing his vote serves as the measure of his voting power.[23]

Moreover, a few lines further down he says:

> A person's voting power, then, is measured by the fraction of the total number of possible voting combinations in which he can, by changing his vote, alter the outcome of the group's decision.

If we take the term 'fraction' literally rather than as synonymous with 'part', then this is precisely the measure ψ proposed by Penrose 16 years earlier, which would come to be known as the absolute Banzhaf measure.

The so-called relative Banzhaf (Bz) index, obtained from the crude Banzhaf score (or from ψ) by normalization, so that its values for all voters of an assembly add up to 1, was invented by Banzhaf's followers (we don't know

[20] [2, p. 331].

[21] [3, p. 1316].

[22] [3, p. 1316].

[23] [4, p. 307]. Note the terms 'chance' and 'likely'. On the following page he explains and justifies very lucidly the aprioristic nature of this assumption, and draws a distinction between a priori power 'inherent in the rule' and actual power.

who was the first to use it), most of whom (including ourselves in [22]) treated it, quite improperly, as though, like the S-S index, it quantifies a voter's share in some fixed quantity. A common practice, when comparing the positions of a given voter a under two alternative decision rules, was to infer that a was more powerful under that rule for which the Bz index assigned a a higher value. This is an error because the Bz index measures a's *relative share* in the total power of all voters, which is not fixed. A change of decision rule may therefore lead to a having more absolute power, but a smaller relative share in the total power.[24]

From the start, Banzhaf's underlying notion of voting power is as I-power: 'the ability of a legislator, by his vote, to affect the passage or defeat of a measure'.[25] 'Power in the legislative sense', he repeats, 'is the ability to affect outcomes.'[26] On these grounds, in his first paper (1965) he rejects the S-S index, as based on what we would call the notion of P-power:

> Their definition, based as it is upon mathematical game theory in which each 'player' seeks to maximize his 'expected winnings,' seems to make unnecessary and unreasonable assumptions about the legislative process ....[27]

But in his later papers, possibly under the influence of Shapley and others, he tends to minimize the differences between the two approaches. Thus in his 1966 paper [3] (in which he, in effect, proves Penrose's square root rule, but does not state it explicitly) he claims that his own measure of voting power '...is substantially in accord with a measure of voting power which has been generally accepted in the field of mathematics and political science...'.[28]

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[24]For example, in $\mathcal{U} = [3; 1, 1, 1, 0]$ the value of ψ for the first voter is $\frac{1}{4}$ and that of β is $\frac{1}{3}$. In $\mathcal{V} = [3; 1, 1, 1, 1]$ the respective values of ψ and β for the first voter are $\frac{3}{8}$ and $\frac{1}{4}$. Thus the first voter has in $\mathcal{V}$ more absolute power but less relative power than in $\mathcal{U}$.

In their invaluable 1979 study [21], Dubey and Shapley note that the common normalization of the Banzhaf score is 'not as innocent as it seems'. Instead, they propose, and study in the rest of the paper, 'another normalization [that] is in many respects more natural', namely ψ, which they call 'the swing probabilities' (p. 102). This major understatement was unheeded by most users of the Bz index.

[25][2, p. 319].

[26][2, p. 331].

[27]See [2, p. 331, fn. 32]. However, he also argues against the S-S index on the grounds that 'it ... attaches an importance to the order in which legislators appear in each minimal voting coalition rather than simply to the number of minimal voting coalitions in which each appears' (loc. cit.). This criticism of the S-S index, which has been made by several other authors, is in our opinion based on a misunderstanding; see [24, p. 200].

[28][3, p. 1317]. The references listed in the footnotes to this claim leave no doubt that

This false – or at best misleading – claim is repeated in his 1968 paper [4], where he again implies that his own measure and the S-S index are 'essentially similar'.[29]

5 Rae

Douglas W Rae's 1969 paper [47] is probabilistic, using explicitly a model similar to the Bernoullian model described in connection with Penrose, but with one difference: as he assumes that the proposal on which the assembly of voters divides is always proposed by one of its members, he excludes the division in which all voters vote 'no'.

He considers only symmetric weighted rules – those in which all voters have equal weights.

He is interested in the probability of four kinds of event regarding a generic voter, named 'Ego', and a random division of the assembly of voters on a proposal:

A: Ego supports the proposal, but it fails.

B: Ego opposes the proposal, but it is adopted.

C: Ego supports the proposal, and it is adopted.

D: Ego opposes the proposal, and it fails.

He seeks to minimize the probability of $A \cup B$. This is of course tantamount to maximizing the probability of $C \cup D$. If we ignore the slight difference between Rae's probability space and the more usual Bernoullian one, then the latter probability for voter Ego is equal to r_{Ego}, which, by Penrose's identity, equals $\frac{1}{2}(\psi_{\text{Ego}} + 1)$.[30] But Rae does not appear to be aware of Banzhaf's (let alone Penrose's) closely related work.

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the latter measure is the S-S index.

[29] See [4, p. 312, fn. 28], where he also reports that Riker, Shapley and Mann had predicted that 'the two techniques yield substantially similar results for large numbers of voting units'. This prediction is correct under certain conditions; but it is known to be false for so-called oceanic weighted decision rules, in which there are a few 'heavy' voters and a large number of very nearly insignificant ones (see e.g. [57, Example 2, p. 1134]).

[30] Dubey and Shapley [21, p. 124f] prove Penrose's identity, and remark that '[i]t was not noticed for several years that this "Rae index" is nothing but the Banzhaf index in disguise.' In fact, as we saw in Section 2, Penrose had been aware of this identity 33 years earlier, 23 years before the publication of Rae's paper.

6 Coleman

James S Coleman's 1971 paper [15][31] takes up the fifth objection (mentioned above, p. 8) to the S-S index – based as it is on what we have called the notion of P-power – being the sole measure of voting power.

He starts by recalling the CGT origin of the S-S index as a special case of the Shapley value:

> What is important is that the [S-S] measure was first obtained as a measure of the value a player can associate with a given game, and that it is based on the important identity between value and power: the value a player associates with a game, the value that he can expect to get from it, is precisely what he can expect to realize from the game, which in turn is his power to affect the outcome of the game.
>
> The origin of the Shapley–Shubik measure of power is important because it gives some sense of the motivation behind the measure, and its intended original purpose. The measure was then adapted as a measure of power in a collectivity by setting the overall value of the game as 1, and determining that a coalition received the value of the game (normalized to unity as indicated) if the coalition was sufficient, according to the rules of the collectivity, to obtain passage of an action by the collectivity.

He then adds:

> This general orientation is somewhat different . . . from the usual problem of power in a collectivity, for the usual problem is not one in which there is a division of the spoils among the winners, but rather the problem of controlling the action of the collectivity. The action is ordinarily one that carries its own consequences or distribution of utilities, and these cannot be varied at will, i.e. cannot be split up among those who constitute the winning coalition. Instead, the typical question is the determination of whether or not a given course of action will be taken or not [*sic*], that is, the passage of a bill, a resolution, or a measure committing the collectivity to an action.[32]

Later in this paper Coleman defines three key concepts. First, *the power of a collectivity to act*; second, *the power of a member to prevent action*; and third, *the power of a member to initiate action*.

[31] Reprinted in [17].

[32] [15, p. 271f].

He phrases his definitions not in probabilistic terms but in terms of relative frequencies. However in the last section he comes clean about the probabilistic model 'implicitly assumed' by him – 'equal probabilities of positive and negative votes by each member, and independence of votes among members', which is precisely the Bernoullian model implicitly assumed by Penrose – and explains the difference between the notion, based on this model, of 'formal power, as given by a constitution' and actual 'behavioral' power.[33]

Rephrased in probabilistic terms, the power of a collectivity to act can be defined as the probability A of an act being passed; the power of member a to prevent action as the conditional probability γ_a of a being critical, given that the act is passed; and the power of a to initiate action as the conditional probability ${\gamma^*}_a$ of a being critical, given that the act is blocked.

These quantities are of course closely connected with the Penrose–Banzhaf (P-B) measure ψ, but contain important additional information.[34]

However, by then the P-power notion of voting power, advocated by S&S, had become dominant. Coleman's critique of this notion and his advocacy of a very different notion – which we have called 'I-power' – was largely ignored.

This attitude was no doubt encouraged by the mistaken view that Coleman's technical contributions of 1971 merely replicated Banzhaf's work, of which he was apparently unaware.[35] As we pointed out in Section 4, p. 11, the dominance of the P-power notion led to the widespread error of regarding voting power as an inherently relative quantity. This in turn encouraged the practice of normalizing any proposed measure so that the values for all voters in a given assembly would add up to 1. Thus, for example, the relative Bz index – rather than the P-B measure – came to be regarded as the principal alternative to the S-S index. Now, when it was pointed out that normalization of both of Coleman's measures (which we have denoted by γ and γ^*) yields the Bz index,[36] that seems to show that they were all essentially one and the same thing. What was not understood is that normalization of the Coleman measures – as well as of the P-B measure – involves great loss of information.

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[33] [15, p. 297].

[34] $A := \omega/2^n$, where n is the number of voters and ω is the number of winning coalitions. The values of ψ_x for all voters x are not sufficient to determine A. But from ψ_a and A we can obtain $\gamma_a (= \psi_a/2A)$ and ${\gamma^*}_a (= \psi_a/2(1-A))$. Conversely, from γ_a (or ${\gamma^*}_a$) and A we can retrieve ψ_a. Also, ψ_a can be obtained from γ_a and ${\gamma^*}_a$ jointly, as their harmonic mean: $\psi_a = [({\gamma_a}^{-1} + {{\gamma^*}_a}^{-1})/2]^{-1}$.

[35] This was somewhat strange in view of Banzhaf's high public profile in matters concerning social choice. Even more strangely, Banzhaf's work is not mentioned in Coleman's 1973 contribution [16] to the subject.

[36] See [12].

A puzzling point about Coleman's important 1971 contribution (in addition to his being apparently unaware of Banzhaf's work) is that he failed to combine his two conditional measures into a single unconditional one. A simple application of elementary probability theory – one that naturally suggests itself – yields as the (unconditional) power of voter a to affect action:

$$\gamma_a A + {\gamma^*}_a(1 - A),$$

which, using two of the identities in footnote 34, reduces to ψ_a.

7 Minor indices

The P-B measure and the S-S index are by far the most important measures of a priori voting power, in the sense of being the most robust mathematical formalization of the two alternative pre-formal notions. They are also the most widely used (although in the case of the former it is its normalized form, the Bz index, which is still most often used).

In this section we discuss three other, far less important, indices that have been proposed. Two of them are mentioned briefly, for the record. But the invention of the remaining one provides an insight into the state of the subject at the time.

The Deegan–Packel (D-P) index, proposed in 1978, [18], is squarely and explicitly based on the notion of P-power, as can be seen immediately from the very title of the 1982 version of their paper [19]: 'To the (minimal winning) victors go the (equally divided) spoils...'. Thus the D-P index shares the same pre-formal notion with the S-S index. But, unlike the latter, it relies for its justification on a specific bargaining model.[37] Namely, D-P assume that only minimal winning coalitions are formed, that they do so with equal probability, and that if such a coalition is formed it divides the (fixed) spoils of victory equally among its members.

The assumption that only minimal winning coalitions are formed is defended, following Riker [49], on the grounds that if a winning coalition contains a non-critical member, one whose removal would leave behind a coalition that can still win, s/he would be ejected by the remaining members who would not wish to share the spoils with a freeloader. This argument –

[37] It is widely believed that the Shapley value, and hence the S-S index, also relies for its justification on a specific bargaining model, namely the model in which the grand coalition is formed in random order, and each player, on admission, demands and is promised the marginal amount that s/he contributes to the worth of the coalition. This belief is mistaken; see [24, Comments 6.2.8, 6.3.9].

whether or not it is convincing – is plainly based on the notion of P-power. No argument that is even remotely convincing is offered for the other two assumptions (equal probability for all minimal winning coalitions and equal division of spoils).

However, since this bargaining model – like all known bargaining models for cooperative games, even simple ones, with more than two players – is neither realistic nor intuitively convincing for at least some of the games in question, the D-P index suffers from rather grotesque pathologies that make it quite unacceptable as a credible measure of voting power.[38]

Another index was invented by R J Johnston [37], also in 1978. The comedy of errors that gave birth to it is described in some detail in our [24, Section 6.4], so we can be brief here.

In 1977, Johnston had used the Bz index in several papers, starting with [36], dealing with the distribution of power in the decision-making bodies of the EC. He was severely criticized on this score by M Laver [39], who was unfamiliar with the Bz index, let alone its underlying justification, and assumed Johnston had invented it all by himself.[39] A believer in the prevailing P-power ideology, Laver misinterpreted the Bernoullian probabilistic model underlying the P-B measure (and hence the Bz index derived from it) as some kind of CGT bargaining model, and hence assumed that the Bz score of a voter is a quantum of payoff. To this he objected:

> If one assumes that all winning coalitions are of equal value (both Johnston and Shapley and Shubik do this, either explicitly or implicitly) then, presumably, if two parties can each destroy a particular winning coalition, they have equal power with respect to that coalition, and share the profits. Similarly, if three parties can each destroy a winning coalition, they also divide the spoils equally, and get less than in the former case. Despite this, John-

[38] For a detailed discussion see our [24, Section 6.4 and Ch. 7 *passim*].

[39] In his 1977 paper [36], Johnston does not refer to Banzhaf – of whom he may not have been aware at that time. Instead, at the end of this paper (p. 577) he appends an Acknowledgement: 'The procedure outlined here was developed from one used by Coleman (1972) and Rae (1972).' (These are references to [15] and [48] in the 1972 edition of [40].) Laver must have missed this Acknowledgement. In his 1978 response [37] to Laver, Johnston tries to put the record straight (p. 907): 'At the outset, however, I should make it clear that the power index that I used was not devised by me. . . . All that I did was to modify slightly . . . an index apparently developed independently by Coleman (1972) and by Rae (1972) and very akin to that used by Banzhaf (1968).' (This is a reference to [4].) A few pages later, Johnston cites Banzhaf's 1965 paper [2], borrowing from it an argument against the S-S index, which Laver favoured.

> ston's index registers one point every time a party can destroy a coalition, regardless of how many others can do the same thing.[40]

Laver based his advocacy of the S-S index as against 'Johnston's index' (which was in reality the Bz index) on the mistaken grounds that the former is justified by means of a specific bargaining model, which he took quite seriously. This is a version of the common error mentioned in footnote 37. In fact, as we show in [24, Comment 6.3.9], this model is quite unrealistic.

In his response [37], Johnston rebuts Laver's critique by pointing out, in effect, that they were addressing two different notions of voting power. But the rebuttal is rather vague and fails to come to grips with the basic issue. Apparently, Johnston himself was diffident about his own position, so – in a true spirit of compromise – he went half way towards his critic. He did not adopt the S-S index and the bargaining model recommended by Laver, but instead proposed 'altering the [Bz] index slightly' ([37, p. 909]) as follows. For each winning coalition containing critical members, instead of assigning one unit of score to each of these members (as Banzhaf does), he now divides a single unit – treated as though it were a quantum of payoff – equally among all critical members of the coalition, just as Laver suggested. Finally, he normalizes the scores accumulated by the voters. This is the Johnston index. It was obtained by grafting on a perfectly good index of I-power (the Bz index) a 'slight correction' motivated by the ideology of P-power. The result is an index that lacks any coherent justification.

Finally we mention briefly an index proposed in 1982 by Holler [34] and called by him 'the public good index' (PGI). Like the Johnston index, the PGI is a hybrid. But in this case the 'slight correction' goes in the reverse direction. The starting point is the Deegan–Packel index, which as we saw is explicitly based on the notion of P-power. But it is modified as follows. For each minimal winning coalition, instead of dividing the single unit (representing a quantum of payoff) equally among all the coalition's members, each member is now awarded the entire unit. This is justified on the grounds that the payoff is a public good. But no reasonable explanation is given for admitting only minimal winning coalitions,[41] let alone for assigning them equal probability.

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[40] [39, p. 902].

[41] As we saw, in the case of the D–P index the argument for this was that the spoils won by a coalition must be divided among its members.

8 Barry

An ironic episode in the history of the measurement of voting power in the latter part of the period under consideration is the 1980 intervention of Brian Barry [5]. What makes this paper so ironic is its combination of the author's perceptive critical analysis with the confusion and misinformation that were widespread at the time.

Barry opens with a massive critique of the S-S index and its underlying P-power ideology. Although some of his criticism is misconceived – he commits the common error mentioned in footnote 37 – its main brunt is very similar to that of Coleman. In fact, much of it reads like a quotation or paraphrase of whole passages from Coleman [15], which however is not referred to and of whose very existence Barry seems to be unaware.

Unfortunately, Barry believes that his critique applies *a fortiori* to the Bz index.[42] He shares the view, prevalent at the time, that the Bz index is a CGT concept, like the S-S index, but in his view it is simply less coherent as such than the latter. (No wonder: if one insists on viewing the Bz index as measuring P-power, then it lacks persuasive justification and performs rather badly.) His argument on this point echoes that of Laver (quoted above, Section 7, p. 16f.) and stems from the same misunderstanding:

> The Shapley–Shubik index is perfectly consistent given its premises (which happen to be inapplicable) and has a certain theoretical elegance. The Banzhaf index, by contrast, is a mere gimmick, with no coherent theoretical underpinnings. It has all the drawbacks of the Shapley–Shubik index in that it too assumes that the 'power' in a committee sums to unity and can be exhaustively allocated among the members. But in addition it violates equiprobability in a very queer way by in effect assuming that each coalition should be weighted in the computation by the number of different ways in which it can be brought down by the withdrawal of a member. Thus, in a committee with weighted voting it may be that a certain coalition is vulnerable to the defection of only one particular member, while another is vulnerable to defection by any of three members. . . . Then the Banzhaf index counts the first coalition towards one member's score whereas the

[42] By the way, his definition of this index ([5, p. 186]) is imprecise, and applies in fact to Holler's PGI (see above, end of Section 7). He also claims (ibid.) that 'it is generally the case that a change in voting rules that increases an actor's power on the Shapley–Shubik index also increases it on the Banzhaf index, and vice versa.' This is false, as can be seen from [24, Examples 7.8.5, 7.9.16] and [51].

> second coalition counts separately in the scores of three members. Yet, if one were going to depart from equiprobability, it would seem more plausible to go in exactly the opposite direction and assume that, the more different ways a coalition is open to being brought down by defection, the less likely it is to form.[43]

In the third sentence of this passage Barry makes clear his opposition – which he repeats elsewhere in the paper – to the assumption (inherent in the notion of P-power) that the sum of the voting powers of all voters is a fixed quantity, independent of the decision rule. And he believes that using the S-S and Bz indices alike implies commitment to this assumption. He is quite unaware of the fact that the Bz index is merely a relativized form derived from the P-B measure, which makes no such assumption.

But the greatest irony of Barry's paper is in the measure of voting power he proposes.

In the second part of his paper, he considers more general probabilistic models than the simple aprioristic Bernoullian one, and allows different divisions of the assembly (different voting combinations) to have different probabilities. With each voter, he associates three quantities, aptly named *success*, *luck* and *decisiveness*, connected by the simple identity:

$$\text{success} \ = \ \text{luck} \ + \ \text{decisiveness}.$$

On examination of these quantities it turns out that in the a priori case, the success of voter a is our old friend r_a, luck is $\frac{1}{2}$ and decisiveness is $\frac{1}{2}\psi_a$.

So, here Barry has unwittingly re-invented the so-called Rae index and the Penrose measure in its original 1946 version (from which the Bz index he has derided is derived by normalization); and re-discovered Penrose's identity connecting these two quantities.

9 Morriss

The last important conceptual contribution to the measurement of voting power made during the 1980s was Peter Morriss's book [43], first published in 1987. Perhaps because of its title, which suggested it was a purely philosophical text, it was for too long ignored by mainstream social-choice researchers – to their detriment, because Morriss was acquainted with Penrose's work and championed it.[44]

[43][5, p. 191].

[44]It is from this book that we found out about Penrose's work in 1995; we wish we had come across it earlier.

The part of his book devoted to the measurement of voting power contains much of value.

In addition to a robust critique of the S-S index and the underlying P-power ideology, broadly similar to that of Coleman,[45] Morriss conjectures correctly some interesting results, which he was unable to prove.[46]

In addition, Morriss differs from contemporary mainstream voting-power authors in that he does not ignore decision rules that allow abstention as a *tertium quid*.[47]

10 Critical re-evaluations

Some developments in the theory of voting power that gathered pace during the 1990s pointed away from CGT and suggested that the former theory cannot be regarded as a mere branch of the latter.

Two issues were critically re-examined. First, it was pointed out that the *formal structure* of a cooperative game – let alone a so-called simple one – is inadequate for representing certain decision rules, including some important real-life ones, to which, intuitively speaking, the notion of voting power ought to apply. Second, examination of the (so-called) paradoxes of voting power and discovery of new ones led to a fresh appreciation and elaboration of the critique mounted in 1971 by Coleman [15] and to a re-evaluation of the foundations of the theory of voting power. These two issues are addressed briefly in the following subsections.

10.1 Non-binary decision rules

Until the 1990s, work on voting power was concerned almost exclusively with *binary* decision rules. To be precise, the decision rules considered were binary in a double sense: each voter must choose between exactly two inputs; and a division of the voters must have one of two outputs. Normally, the two possible inputs are a 'yes' or a 'no' vote; and the two possible outputs are adoption or rejection of the proposed act. However, the same format can obviously be used – and indeed was used – for decision rules in which an

[45] Morriss is however unfair to Coleman, whose measures he dismisses as 'identical to Banzhaf's' (p. 167). In our view, he is also too scathing about the Bz index, which he rejects as utterly useless and advocates 'banishing' it (p. 166). In our opinion, the Bz index is useful for certain purposes, provided it is handled with care and understanding.

[46] These can be found, with proofs, in [24, Cor. 3.4.10, Comment 6.2.24].

[47] Barry [5, p. 350] also mentions abstention as a significant factor.

input is a vote for one of two competing candidates; and an output is the election of one of these candidates.[48]

In reality, there are of course more general decision rules. To facilitate our exposition, let us use the following notation. We shall say that a decision rule is of type (j, k) if each voter has j possible inputs and there are k possible outputs $(j, k \geq 2)$. Thus, the (doubly) binary decision rules of the preceding paragraph are of type $(2, 2)$.

Rules of type (j, k) may be classified into two kinds: ordered and unordered. In an ordered rule, the inputs and outputs are strictly ordered in some natural way. A prime example of this kind is where the j possible inputs are levels of approval by a voter, ranging from extreme disapproval to total approval; and the k possible outputs are, similarly, levels of approval by the decision-making body. A common kind of unordered rules are election procedures, where each input may be a vote for a particular candidate, or a preference ranking of the candidates; and each output is the election of a single candidate, or a group of candidates.[49]

The most important type of ordered rule other than $(2, 2)$ is undoubtedly $(3, 2)$, where the added possible input is abstention. In fact, this type is probably more common in real life than $(2, 2)$, because the decision rules that apply in legislatures and other decision-making bodies usually treat abstention as a *tertium quid*, whose effect is different from both a 'yes' and a 'no' vote. This makes it all the more surprising that this type of rule – like all types other than $(2, 2)$ – was for so long ignored by voting-power theorists.[50]

As far as we know, the first to address a non-$(2, 2)$ rule from the viewpoint of voting power was Fishburn in his 1973 book [31, pp. 53–55]. His treatment is very brief and concerns a rather special class of ordered $(3, 3)$ rules,[51] for which he defines a generalization of the Bz index.

The true pioneer of systematic study of voting power for non-$(2, 2)$ rules is Edward M Bolger, who started to address this topic in 1983 [6] and returned

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[48] This is done, for example, by Penrose in his 1952 booklet [46] and by Banzhaf in his 1968 paper [4].

[49] Arguably, if $j = 2$ then the inputs can automatically be regarded as naturally ordered; this is also true when $j = 3$, provided one of the three possible inputs is abstention, or indifference between the other two. The same holds for the outputs where $k = 2$; and also for $k = 3$, provided one of the three outputs is a tie between the other two.

[50] In his 1965 paper, Banzhaf raises and dismisses the issue of abstention in a brief, and in our opinion inadequate, remark in a footnote [2, fn. 34]; but this is at least better than ignoring it, as others do.

[51] These are weighted rules whose possible inputs are x and y, which may (but need not) be 'yes' and 'no', and abstention; the possible outputs are x, y and a tie, which occurs if and only if the total weight of the x votes equals that of the y votes.

to it in a series of papers, including [7, 8, 9, 10, 11]. Bolger's work is concerned with generalizing the Bz and S-S indices to unordered (j, k) rules for arbitrary $j, k \geq 2$.

In his 1993 paper [9, pp. 319–320], Bolger observes: 'It should be noted that the U.N. Security Council game is often erroneously modeled as a 2-alternative, namely "yes" or "no", game in which an issue passes if and only if it receives "yes" votes from all five permanent members and at least 4 nonpermanent members.' This is of course a misrepresentation of the actual decision rule, because an abstention by a permanent member does not block a resolution. As should be well known, the actual rule is that a resolution on substantive matters passes provided at least nine members vote for it and no permanent member votes against it.

In fact, Bolger understated this point: the UN Security Council rule had been misrepresented in the voting-power literature not merely 'often' but almost without exception. This holds also for the decision rules of the US legislature. It is not just a matter of using the wrong theoretical model for these rules; *the actual real-life rules are erroneously stated in this literature.* We treat this curious phenomenon in detail in our 2001 papers [26, 27].

We discovered this widespread misrepresentation in 1996, when we started working on ordered $(3, 2)$ rules, in which abstention is a third possible input. Our work on these rules is reported in [23, 24]. Unfortunately we were not aware at that time of Bolger's earlier work on unordered rules.

To conclude this subsection we would like to mention a recent conceptual advance: the correct definition and characterization of ordered (j, k) *weighted* rules. For the $(2, 2)$ case, this is a rather obvious matter; but for the general case it is highly non-trivial. The problem has been solved by Josep Freixas and William Zwicker [32].

10.2 The foundations re-evaluated

In our 1995 paper [22] we showed that the Bz index β suffers from the bloc paradox: it is possible for a voter to 'lose power' (as measured by β) as a result of annexing the voting rights of another voter.[52] Since it is intuitively clear that a voter annexing the voting rights of another voter cannot genuinely lose power thereby, we concluded that β should be disqualified as a reasonable index of (relative) a priori voting power. Moreover, in that paper

[52] For example, let $\mathcal{U} = [11; 6, 5, 1, 1, 1, 1, 1]$. Now suppose the first voter (with weight 6) annexes the voting rights of one of the voters with weight 1. The result is $\mathcal{V} = [11; 7, 5, 1, 1, 1, 1]$. Now, the value of β for the first voter is $\frac{11}{23}$ in $\mathcal{U}$ and $\frac{17}{36}$ in $\mathcal{V}$. Since $\frac{17}{36} < \frac{11}{23}$, it seems that the first voter has lost power as a result of the annexation. (This is [24, Example 7.8.14] and is somewhat simpler than the original example given in [22].)

we expressed the view that the S-S index – which was the only index not susceptible to any of the (genuine) voting-power paradoxes known at that time – is the only reasonable index of a priori voting power.

It took us three years to reverse our position. In a 1998 paper [29] written jointly by William Zwicker and us, the distinction between I-power and P-power – foreshadowed by Coleman [15] – was made explicitly for the first time. In that paper it is also shown that the S-S index (but not the P-B index) suffers from a severe form of the so-called bicameral paradox, which seems to be a genuine pathology. We therefore concluded that this should possibly disqualify the S-S index as a reasonable measure of P-power.

These and other foundational matters are elaborated in our book [24], published in 1998 – the first monograph wholly devoted to the theory of voting power.[53]

We were now of the opinion that the fact that β displays the bloc paradox does not disqualify it as a (relative) index of I-power because it is merely a derivative (obtained by normalization) of ψ, which is not susceptible to the bloc paradox. The false conclusion we had reached in [22] was due to the error pointed out above, in Section 4.[54] That an annexation may cause the annexer to lose *relative* I-power is an interesting non-trivial phenomenon. But there is nothing unreasonable about it: the annexer is not really weakened but actually gains *absolute* I-power.[55] However, some of the remaining voters may also gain power, so much so that the annexer's relative I-power is reduced.

As for a priori P-power, the S-S index still seems to be the most reasonable candidate for measuring it – provided the notion of P-power itself can be coherently formalized, which is somewhat doubtful.

In our 2002 paper [28] we analyzed another related counter-intuitive phenomenon: we showed that when voting power is understood as I-power, forming a voluntary bloc may be advantageous even if its voting power is smaller than the sum of the original powers of its members, and it may be disadvantageous even if its voting power is greater than that sum.[56]

[53] The I-power/P-power distinction is also further discussed and explained in subsequent papers, for example [25] and [42].

[54] See text to footnote 24.

[55] In the example of footnote 52, the value of ψ for the first voter is $\frac{33}{64}$ in $\mathcal{U}$ and increases to $\frac{17}{32}$ in $\mathcal{V}$.

[56] We too were guilty of (partly) reinventing the wheel. After that paper was published we discovered that Coleman [16] had addressed a similar problem almost 30 years earlier. However, his analysis was less comprehensive than ours.

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