The Desert Fan Palm--Evidence Supports Relict Status

A rebuttal to a vastly undersubstantiated and scientifically flawed article by Curator of Palm Springs Desert Museum- J Cornett entitled: -The Desert Fan Palm-Not a Relict.

NOTE:

This article was written because certain statements made by the rebutted article appear indirectly and partially responsible for the approaching demise of large portions of certain native groves of Washingtonia filifera.

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Washingtonia filifera (sp.) also known as the Desert fan palm, a native plant in certain deserts of the Southwestern United States, has been the subject of a number of articles all attempting to determine whether or not it is a "relict" species there. In 1950 Daniel Axelrod wrote that this palm's apparent scarcity (in the wild); the discoveries of fossils of Washingtonia in the Mojave Desert and the fact that it is scattered widely over its' present range in apparently isolated oases, together supported the idea that the palm was indeed "relict." In the following years many subsequent papers and studies of this plant built upon or supported this idea in varying degrees.

However, in 1989 James Cornett authored an article in the Mojave Desert quaternary research symposium which announced alternatively that the Desert Fan Palm was a recently evolved invasive plant species and not relict at all. Cornett was explicit on this point in his title which reads: "The Desert Fan Palm, Not a Relict," strongly suggesting the matter was closed.

It was suggested by Cox (and others - 1976) that to be relict a species should follow these guidelines and be one: " which in the past was widely distributed, was subsequently affected by climatic changes and survives now only in small numbers of isolated areas possessing favorable conditions." Cornett's disagreement with Axelrod's "relict" theory was that there was "no direct evidence supporting the assertion that Washingtonia Filifera (was/is) a relict species."[sic] Furthermore, he stated that all of the evidence actually suggested that his own theory that the palm was a recently evolved and invasive plant species was correct.

It is the strong and well backed up contention of this *article* , however that upon closer examination all of Cornett's so "called" evidence cited, neither validates his theories nor his ill formed contentions that the original theory of Axelrod was wrong. In this discussion I will address each of Cornett's points of "contention" [sic] or "so called" evidence and show why it is still far more likely that W. filifera is in fact, relict. Upon close examination there is indeed absolutely no direct evidence supporting Cornett's late and scientifically flawed theory that Washingtonia Filifera is an invasive species and not relict. In fact, all of the aspects are far more indicative of a relict status than of an invasive one.

1 -The Climate evidence:

One of Cornett's first statements which should be addressed is that the Mojave Desert is "a region too cold for the genus today." Although at first, in context this statement appears to indicate a reference to the work of Axelrod, other published materials by Cornett [1] indicate that he in fact, ascribes to this provably incorrect idea. [2]

In direct contradiction to this, Palms may in fact be found in nearly every town and even ghost town throughout the Mojave. Furthermore it is an established fact that the ***winter lows*** experienced by this area today have remained virtually the same for the last 18,000 years and even longer. Peter J. Mehringer may have made the most definitive arguments regarding the Mojave's climate. In 1963 as part of the Tule Springs archaeological expedition, his research shows that ***winter cold*** for the entire Mojave has remained constant for at least 18,000 years. Aridity and precipitation however, have changed dramatically:

"Paleoclimatic evidence for southeastern Nevada demonstrates no significant changes between 7,000 B.P. and the present, on the basis of pollen spectra. . . . Fossil pollen spectra and Neotoma midden analyses, combined with dendrochronology (Fritts 1971), provide a consistent history of paleo-ecological conditions of the area for the past 18,000 years. . . .Using the above-mentioned studies for the period of 18,000 to 8,000 B. P., paleo-climatic conditions can be characterized as follows: There was a slow transition from cooler temperatures and increased precipitation to conditions similar to those of the present. The increase in temperature is considered to be between 5 degrees and 10 degrees, ***with winter conditions remaining as they were*** , and summer temperatures increased. This was accompanied by a 40% decline in precipitation at elevations below 2100 meters." NV State Museum Archeological papers # 19 1984 -pp 36-38

(Also see: Southwest Museum Papers #8 April 1933, SW Museum Los Angeles-by M. R. Harrington pages 167-171 for discussion supporting and elaborating on other aspects of all of the above).

It appears from the forgoing (includes Harrington's work) that the only thing that has really changed has been degree of aridity and availability of water in the entire region. This same trend applies to the Colorado Desert and Coastal and Inland California with increases (not decreases) in summer temps only. Although the available fossils are extremely ancient compared to this recent 18,000 year period, there is no real evidence that the area was intolerably colder for palms before that time. It appears that the demise of widespread palms may have been an extremely ancient occurrence but that conditions were at least more favorable as far as moisture is concerned in many parts of the Mojave and beyond as recently as 18,000 years ago. (S.B. Parrish a California Botanist, in 1907 commenting on a specific latitude stated "this is an area of pines not of Palms" leaving the impression that the two plant types were

mutually exclusive to habitat . This however is a broad and not entirely accurate statement since not all palms are strictly tropical and not all pines are strictly temperate. Some pines for instance have a more tropical affinity and are ***easily*** killed by frost, while there are many species of Palms which are found ***only*** in the more temperate subtropics and take frost quite remarkably well.)

To reiterate: Since favorable winter climate has existed in prevailing Mojave locations, for as much as 18,000 years, widespread fossil evidence of fan palms (genus speculative) suggests that ancient inland oceans or periods of declining availability of moisture -not cold- wiped out these ancient taxa. Knowing the remarkable consistency winter lows have exhibited for this recent 18,000 year period, it would be a mistake to infer that the climate in the Mojave is somehow now too cold for Palms since Palms are now extant wherever water has been developed and wherever humans have brought seed throughout lower elevations in the Mojave and even beyond. If one considers that these same conditions must have also existed for most areas in the Colorado Desert to the south (where no one could legitimately argue it has ***ever*** been too cold for this plant), then it becomes somewhat unreasonable to say the Mojave is now or ever has been a region "too cold for palms."

Additionally, in direct contrast to any assertion that the region is too cold for palms, it has been observed by this author (over a period of nearly twenty years) that at least seven distinctly separate palm genera and their species [3]

have been thriving throughout the farthest reaches of the Mojave desert. Although these plantings are mostly all adventive W. filifera with origins in the early 1900's, it is the fact that these plantings may be found near the most extreme boundaries of the Mojave in all directions which I am highlighting. In Table 1 (following)some of these locations are listed. Many must be old since they were large when the author first noted them between 1977 and 1982. From this table, one familiar with the locations can see that they are indeed spread throughout the entire Mojave desert. Communications with people at random throughout the different areas indicate that palms have been in many of the locations for more than sixty years*, in some cases even one hundred+ years** and in others perhaps anciently.***

Table 1. - Localities in the Mojave and northward where palms have been observed.

Baker CA* Barstow CA* Boron CA Boulder City NV*

Bunkerville NV* Echo Bay NV Furnace Creek CA* Glendale NV*

Grapevine Spring CA* Henderson NV* Indian Springs NV Jean NV

Katherine's Landing AZ Kelso CA* Kingman AZ Lancaster CA

Las Vegas NV* Littlefield AZ* Logandale NV** Moapa NV***

Beaver Dam AZ* Mojave CA Overton NV*** Pahrump NV

Newberry Springs CA* North Edwards CA Twenty-nine Palms CA*** Victorville CA

Palmdale CA Pear Blossom CA Rosamond CA Searchlight NV

Shoshone CA* Stateline NV Tecopa CA** (China Ranch) Temple Bar AZ

St. George UT

Notes on Table 1:

It should be noted that Grapevine Springs is near the extreme Northwest boundary of the Mojave. Mojave (the town) lies as far toward the Western boundary as one can go. St. George meanwhile extends beyond the Northeastern Borders of the Mojave. Some may class Kingman AZ with the Sonoran desert although it is actually colder than St. George Utah, Either way Kingman could represent the extreme eastern boundary. Twenty-nine Palms is part of the extreme Southern boundary. Interestingly, Twenty-nine Palms is apparently one of the colder areas of the Mojave desert and this may be due to its' geographical configuration at the bases of higher elevations. Tecopa and Shoshone (far to the north), appear to experience warmer winters than Twenty-nine Palms. Twentynine palms is nearby an ***ancient*** grove.

Furthermore areas in Southern Nevada around Moapa and Overton which are about 100 to 300 meters lower in elevation than Las Vegas (and which climate appears to have been repeatedly misrepresented) apparently experience some of the mildest winter weather of the entire Mojave and indeed have more comparable temperatures to much of the Sonoran Desert to the South.[4]Perhaps it should be included in the Colorado desert or listed as a similar 'sister' desert with drainage also to the Colorado river rather than to the Great Basin.

NOTE: the following section and other portions of this report will concentrate on the Moapa, Overton Nevada area groves and climate since this is one of the most northern areas in question and since it is an area which is incorrectly termed a "new" location for Palms. The author also happens to be intimately familiar with the area for over 17 years.

A s a rule, Overton winter temperatures are from 2 to 8 degrees warmer (Fahrenheit) than Las Vegas on a given day. Furthermore, although it may seem odd Logandale [5] (only a few miles from Overton) happens to be in a micro- climatic "cold sink." Unfortunately, temperature data has been collected there for many years even though my research and other climate data shows that it misrepresents the entire area by as much as 12 degrees Fahrenheit. According to observations of freeze data for the Lake Mead area (spanning more than ten years) Logandale experienced an average of three times the number of days below freezing as did Moapa, Overton, Rogers Springs and Stuart's Point. On summer evenings this marked cold sink effect is clearly obvious even to the casual observer, by simply driving through Logandale with the car windows down. ***(See Table 2)***

Comparisons of average frost free days in Moapa area of NV to areas in Sonoran deserts:

Table 2- *'Locale' Comparisons of Average frost free days per year over a ten-year period from 1972-1982*

(in descending order)

Yuma 350 days

Overton 349 days

Logandale 310 days

(Below is a twenty-year average for the listed Sonoran communities for an interesting comparison)

Phoenix 302 days

Tucson 242 days

Table 2-A Average minimum temperatures for the same areas

NOTE: all temps are given in Fahrenheit following the standard method used in collecting the data.

Logandale 25.6 F

Overton 32.3 F

Yuma 36. F

NOTES ON TABLE 2- and 2-A

While the average number of frost-free days during the indicated period in Overton is comparable to Yuma AZ for the same time period, the Average minimum temperature is lower by about 4 degrees. This suggests that while sub-freezing temps are equally as infrequent in Overton as in Yuma they tend to be lower. (6)

Also once again Logandale is a great exception and consistently shows far lower freezing temperatures than any of the surrounding areas. Just for example a comparison of the three coldest days for the ten-year period referenced in the table:

Jan 1973 Logandale's lowest low was 17 deg F

Jan 1974 Logandale's lowest low was 15 deg F and in

Jan 1979 Logandale's lowest low was 15 deg F

For the exact SAME periods 11 miles away for Overton

Jan 1973 Overton's lowest low was 25 deg F (a full 8 degrees higher)

Jan 1974 Overton's lowest low was 22 deg F (a full 7 degrees higher) and in

Jan 1979 Overton's lowest low was 27 deg F.(a full 12 degrees higher)

Observations:

In 10 years the author noted that for three separate data collection points: Overton, Echo station, and Stuart's Point, there was NO YEAR in which more than one month experienced temperatures lower than 27 degrees F. and only four days in ten years experienced temperatures below 25 degrees F. The coldest temperature on record for all three stations in ten years was 22 degrees F in Jan 1974 (however only 2 days were recorded for the entire year as being 25 degrees F. or below). For the entire year of 1974 these three locations had only a total of 14 days which show temperatures 32 or below and the lowest of those remaining 12 days (excluding the two days of 25 deg or lower) was only 31 degrees.

The author's observations indicate that subfreezing temps also probably tend to be longer in duration in Overton than in Yuma (which is as would be expected). Still the data supports the idea that the climate around this very Northerly part of the Mojave is indeed far more tolerable to the Desert Fan Palm than has repeatedly been suggested. This misrepresentation and possibly some form of preformed bias has contributed the fact that this palm has been repeatedly omitted in local flora surveys . Although this has been partly due to a climatic bias there appear to be other types of bias afoot as well. Note that a similar climatic bias misplaced the type locality of this plant for 107 years! (7) *(See also:* (8) *)*

2- The Fossil Evidence:

The next contention of Cornett is that "Recent evidence suggests that Washingtonia filifera has never been more widely distributed than it is at present. There does not, in fact, appear to be any fossil evidence indicating that this species was once widespread in the Mojave Desert." [sic] Here no references are offered save a (pers. comm.) with Axelrod noting Axelrod had retracted many of his earlier assertions that the fossils in question could be assigned to the genus

Washingtonia. Axelrod adds that there is at least one exception to this: that of a fossil collected near Wickiup, AZ. This particular specimen was not located by Cornett at the museum where it resided and therefore "could not be examined". [sic] Furthermore he goes on to say that an examination of additional Axelrod Palm fossils "failed to reveal any specimens that displayed spines on the petioles, an important characteristic of this genus, and therefor none could be classified as Washingtonia". [sic]

Firstly, and most importantly, NO palm genus (except for Phoenix) may be positively identified by fossil leaf remains alone. Even with Phoenix there remains the possibility that an extinct taxa existed which possessed Phoenix type leaves leaving some question there as well. This one fact is precisely why

Axelrod retracted his early statements in which he had assigned the leaves to Washingtonia. I am certain he never stopped believing they were Washingtonia, rather he simply knew it was morphologically impossible and scientifically improper. To continue in this vein of reasoning appears to not only unfairly place emphasis on an inappropriate facet of this argument but it also could be very misleading to overstate the importance of a point which in reality neither validates nor discounts anyone's theories!

Since this issue keeps resurfacing however, this author would like to go over its' points. However to be fair to Axelrod it should first be pointed out that simply because Cornett himself could not personally examine the fossil at the museum of Paleontology at Berkeley, it can not necessarily follow that it would be possible to so broadly infer that "there is no direct evidence supporting the assertion that Washingtonia filifera is a relict species". Using methods evidenced by his own writing it could be said that Cornett's statement in itself may be an unjustifiable "assertion". (The following examination of the 'missing spines question' will bear this out). It should be noted that according to Cornett himself, Axelrod is said to have believed that this particular fossil could be classified as Washingtonia. It seems unreasonable that Axelrod should be disallowed to make such an observation while Cornett on the other hand, allows himself the opportunity of so preemptive an observation. After all, Axelrod was able to actually view the fossils while Cornett was not. Certainly Axelrod would be more than qualified to make this legitimate observation based on his extensive knowledge and the actual viewing of the fossils.

The foregoing would especially be true if based only on an observation that the fossils simply exhibit no spines. Indeed, spineless petioles on Washingtonia specimens are ubiquitous beyond a certain age and thereby unable to afford any objective observer the satisfaction of a meaningful or definitive exclusion of this genus as being a represented fossilized taxon.

In fact Cornett himself published a paper entitled: "Spineless petioles in Washingtonia Filifera (Arecaceae) -(Madrono, vol. 33 No.1, Jan 1986). In this paper it is clear that he has observed first-hand that on W. filifera palms over 8 meters in height the spines are nearly always absent on the distal ends while individuals exceeding 14 meters in height almost without fail exhibit no spines at all. My own observations indicate that for completely spineless petioles to occur, trees may be even shorter (and apparently also younger than Mr. Cornett more conservatively allows.) (The Palms at KOFA AZ purported in other writings to be possibly "the only relict population"* are good examples of this phenomena. Furthermore it is still possible there may be some genetic differences in the KOFA population, which bears implications likely obvious to the reader.) *(Quote from page 21 "Desert Palm Oasis" -Cornett, J. W.-1989) *(See endnotes for KOFA comparisons.)*

The reality is that: spineless petioles are an equally important characteristic of Washingtonia. The feature may be mostly related to age or other factors (as in the case of KOFA palms). To make a statement implying that one is more a characteristic than the other appears arbitrarily selective and unfounded. It would be a little like making the statement that baby smooth skin is an important characteristic of humans, while of course, so is weathered skin as one ages. Perhaps if all the "observers" were 20 meters tall they would walk among the palm groves noting "spineless petioles is an important characteristic of Washingtonia" -then as an 'aside' point out that "it appears that younger palms have spines on the petioles".

Even more interesting is a written acknowledgment that fossils found in the Mojave may have been improperly assigned to the genus Sabal based solely on this "spineless" feature. [9] Cornett states that: "...fossils of the Pliocene age, assigned to the genus Sabal, occur over much of the southern half of California including what is now the Mojave desert." and that"...These fossils may be misidentified...As I have shown,...the petioles of Washingtonia may lack spines and therefore could be assigned erroneously to the genus Sabal."[sic] [10] *(It should be pointed out that in fact, all three genera (includes Brahea) may exhibit spineless petioles, not only Sabal.)*

One should realize that entire fossils of such things are rare or non-existent. What we have instead are generally shortened portions of the leaves or petiole segments. It is my observation as well as Cornett's (see preceding reference) that the portions of the petiole nearest the distal end display fewest spines even in younger, hence, 'spinier' specimens. This increases the obvious likelihood that if only fossils with visible leaf forms were considered, they will likely exhibit few, or no spines ***even if they were from spine-bearing specimens.***

Another observation by this author includes common and apparently gradual dis-integration of petiole edges resulting from age (whether the petioles continue to hang on the tree as part of the petticoat or fall to the ground). This disintegration has been observed to cause the edges of the petiole to become soft and fibrous facilitating easy removal or loss of spines. The author has often observed old petioles on the ground which appear to be semi-devoid of spines simply due to disintegration of the edge. This perhaps could also be considered when debating whether spines should be visible on fossil specimens.

Brahea armata (the closest other genus geographically), also possesses spiny petioles. It appears that this is why the fossils were assigned to the genus Sabal even though the nearest occurrence of Sabal ***(adventive or not),*** is in Cameron Texas or in north central Mexico near the ancestral homes of the Pima and Papago. If all the fossils are Sabals then it would seem odd that there remain no Sabals extant in the areas in question even though they are as hardy or even more so than W. filifera. These arguments regarding spines, are basically moot however, due to other factors. One of the most important of which perhaps, lies not with the spines at all but in other less variable leaf characteristics.

The Leaf Form Clue:

On page nine of "Desert Palm Oasis" by James Cornett (1989 J. W. Cornett and the Palm Springs Desert Museum) one can see a photograph of a fossilized Palm leaf. I will use this example fossil since it is clear that this one has been a subject of study in this matter. This appears to be one of several fossils assigned (simply due to lack of spines) to the genus Sabal. However, this author has difficulty assigning this leaf to Sabal. Specifically, the specimen seems more likely the fossil of a palmate or fan rather than costapalmate leaf. If one looks closely, one may see that the fossil shows the abaxial view because of the rounded shape of the petiole. (See figure of the fossil in question in Photos) However if the observer follows the petiole into the leaf itself one finds what clearly appears to be the termination of the petiole into induplicate folds (in abaxial view). In lieu of a visible costa (which one might expect with Brahea or Sabal), there in fact appears to be a clear 'V' of a fold pointing back toward and meeting with the distal end of the petiole precisely as a fan shape or only briefly costapalmate form would exhibit. This probably would not occur with most Sabal species which are most often strongly costapalmate and usually exhibit a very marked costa. Additionally the fossil appears to be very relatively flat . Since most Sabals exhibit strongly recurved leaf-forms, this too appears odd. (Perhaps one should try making a faux plaster fossil (as an experiment) and attempt to get this particular portion of the leaf to press out flat without destroying or substantially splitting the leaf shape. I have tried this with Brahea armata which is recurved and strongly costapalmate as well and have had no success.) In fact even the juvenile simple leaves on Sabals are so strongly cupped that they split and curl quite dramatically before they are fully grown into their adult shapes. (See comparisons of Washingtonia with Brahea in photos.)

This fossil appears to be a flat Palmate leaf with no evidence of a continuation of the petiole or costa through the leaf (as a Brahea armata or Sabal would likely exhibit.) I would agree with Axelrod, that this could well be Washingtonia.

One more interesting note: An excellent picture of Washingtonia spines is found on page 17 of Cornett's "Desert Palm Oasis" Note how the spines point upwardly and not horizontally (as one may have casually expected) in this adaxial view. Since the fossil discussed is an abaxial view with the margins of

the petiole slightly recessed or possibly obscured to some degree is it also possible the spines are simply obscured by peripheral fossil material?

To summarize the forgoing points regarding this contention:

(a)-The fact that Washingtonias ***alone persist*** *into the present day in areas closest to locations 'Axelrod' fossils were found (IE; the Mojave desert, Wickiup etc.),*

(b)- The forgoing arguments which disagree sharply that lack of spines alone on fossils should be used to classify or assign fossil taxon, and:

(c)- The fact that Palm fossils widely distributed throughout the Mojave cannot successfully be discounted as being Washingtonia in origin.

...argue ***both against*** the contentions or evidence which the referenced article has offered us, and ***for*** the idea that W. Filifera may indeed be relict after all.

Once more it should be stressed that the previous assignments of taxa to fossil Palm leaves based on presence or absence of spines (at least with respect to W. filifera petioles) has appeared:

-somewhat arbitrary: (since leaves may be either spiny or spineless)

- inconclusive: (since it is clear that Washingtonia has most definitely not been ruled out.)

-And premature: (since it would appear other more reliable and less variable leaf characteristics were not completely accounted for.)

The 'spine' debate appears to be too subject to wide variation even in 'same species' to serve as valid criteria in such determinations. On the other hand, less variable factors such as leaf type, (which remain consistent at leaf maturity from plant to plant within the species) are not so subjective and may perhaps be more reliable.

However the main point after having said all is:

..."that among all palms only the genus Phoenix, ...can be definitely identified to genus from leaf remains." (Genera Palmarum page 53- Uhl & Dransfield)

Furthermore, other characteristics such as presence of adaxial hastula, palmate venation etc may be used to identify a fossil as a palm but not to assign genus. (Ibid) The presence or absence of spines would not appear to be valid criteria. Since it seems clear that *no fossil may be assigned to any genus from leaf remains (except Phoenix) then the entire fossil premise could be flawed from the start.*

One should be aware however, that even though assigning genus is not possible, it is sometimes possible to describe leaf type such as costapalmate, palmate, or pinnate. Furthermore since indication of leaf type may eliminate the possibility that a fossil belongs to a certain extant genera or species, it becomes clear that we may effectively eliminate some extant species in certain genera without assigning a fixed taxon to the fossil. In light of this, the author feels strongly that the absence of costa on the fossil referred to in the above dialogue as well as the flatness of leaf shape substantially precludes the possibility that the fossil is either B. armata or any extant species in the genus Sabal. This leaves several possibilities: that it is an extinct fan taxon, that it is another fan taxon extant today only in locations far from where the fossils were found, (much as Phoenix fossils have been found in Texas), that it is a precursor or genetic variant of W. filifera *or that it is one of the thousands of W. filifera leaves which simply exhibit no spines.*

3- Declining Number of Individuals (and Shrinking Geographic Range)

The third contention is that since a "relict species should be expected to have a declining number of individuals" [sic] ...(then how is it that:) "...at the present time, there are more wild desert fan palms than there were when the first census was taken." ..."(and) the distribution of W. filifera is presently expanding particularly to the north."[sic] (Words in parentheses mine.)

There are five points I would like to make regarding the above:

(a) *- First of all, it may be that the 'relict criteria' has been interpreted far too broadly suggesting that to qualify there needs to be a contemporary decline in numbers of individuals. However it seems to this author that a given population should be allowed to increase or decrease over given periods of time and not detract from the simple fact that anciently the taxon was more widely spread and subsequently reduced to scattered pockets of favorable conditions due to long term changes in environment or habitat. The entire concept that an increase in Palm numbers over the last 100 years somehow precludes it from fitting the model of a relict (a distinction which apparently came about before the last 18,000 years or even far more anciently), seems overstated.*

(b) *Secondly, it is implied that the "first census" taken of wild palms was complete and that it included all the groves which his late counts now include. This is definitely not the case however. These so called 'official counts' failed entirely to include groves which are known to have been extant when those counts were made. (Such as Castle creek Palms as well as grapevine Springs and at Moapa Warm Springs.) These "left out palms" leave two false impressions:*

1) -that Palms have 'spread' Northward in the last 70 years, And

2) -that there are vastly spreading numbers of wild palms in general.

In reality little has changed at most of these locations and further reading will show that inother cases numbers have more than likely declined. (Though this is due to human intervention.) The counts themselves will be shown to be vastly askew as well.

(c)- Inadequate credit appears to be given to extrinsic factors in the"spread" of the palm into perceived"new range" in turn effecting an inaccurate impression that somehow the palm is"spreading" due to other more subtle and intrinsic factors such as"global warming" and some sort of evolutionary range expansion. The palm it seems however, has apparently been as far north in the wild as it is currently for as long as white men have been counting them. As thorough as Henderson and his first counts were, it appears he did not know about Palms inMoapa for instance. These were certainly extant and already old when he began counting. Butl don't think Henderson ever claimed to know every single palm location, (although he tried.) It doesn't seem intellectually proper to this author for one to be able to use these counts as if to say: "if Henderson didn't count them they apparently didn't exist..." (this seems to this author, an unfair use of known facts.) Henderson for instance, may not have known about some of the wild palms and even Cornett and others may not have known, But that fact shouldn't equate with the idea that they didn't exist.

Cornett himself must be aware for instance, that Palms were extant at Castle Creek (the type locality) and at Warm Springs Nevada, (one of the most northerly locations,) as well as perhaps even Grapevine springs long before researchers began counting most Palms. These"uncounted" palms are missing from all these referred to 'previous counts'. I say this since his own research at the 3 locations termed as "new" shows that he is aware all these palms existed when the earlier counts were made. In my humble opinion, It seems inaccurate that one should use the fact that none of these groves had ever been part of a published "count" to infer that these palms should now considered "new". Cornett must be aware by now that these palms are not "new". (Note: In fact this author has published a detailed report detailing highly probably evidence that in some cases these so called "new" palms apparently predate white mans' arrival. see http://home.inreach.com/willmike/wf-hr-foreword.htm for full access to these reports.)

(d)- There is also a failure to note that some of the literature cited which is supposedly used to account for all known wild palm locations, never represent that they are attempting to account for any plants outside of those flora found within the ranges covered by the scopes of those particular publications. This seems an important omission.

For instance Munz's - "A California Flora" is cited to substantiate that the Turtle mountains were the most northerly previously known wild location for this palm. (Here the implication appears to be that the palm had subsequently spread to more northerly locations) However, it would not seem A California Flora is obliged to take Nevada's flora into account. Elsewhere Cornett indicates that Moapa's palms must have been well known before 1959 when the botanical in question published that Turtle mountains were the most northerly location. (Desert Plants Volume 8, Number 4 on pages: 169-171 in an article entitled: "The occurrence of the Desert Fan Palm, W. filifera, in Southern NV.") Clearly again, they weren't known to Munz. (Turtle mountains are incidentally about 165 km south of Moapa NV and have a colder winter clime. This fact too complicates the idea that the palm is "pushing" it's way northward into colder climes. In this case 'south' would constitute colder. . . not warmer.)

(e) - In other articles Cornett published in 1985 such as "Reading the Fan Palms" - by Natural History magazine (page 72,) similar statements such as: (... "periods of above average rainfall are responsible for impressive increases"...) in (palm populations near Palm springs) over the last 50 years," indicate again, that more obvious contributing factors affecting the palms' 'spread' may have either been substantially minimized or understated. The most obvious and significant of these factors (the loss of Native American harvesters) are discussed a little later.

Discussion about conflicting counts

First of all, I'll quickly mention the counts of the naturalists. When R. Henderson made his famous "counts" in "On Desert Trails" he would have failed to count the palms in Castle Creek AZ as 'wild' palms since they were not established as "native" until 1976. [11] This is conspicuously ***not*** noted although by the time of the referenced ***1987*** count it would seem odd that one reporting this for a reference paper could be unaware of this published data. For now however I wish to concentrate on conflicting counts.

This author contends strongly with the accuracy of "counts" published by Cornett which it is believed may be shown by photographic record to be in error by a margin of as much as 400%. (see photos and notes at end) (The original photographs in the author's possession actually have the dates printed by Kodak on the backs of the photographic materials.) (See photos) For example, in January 1986 Cornett counted the palms at Blue Point spring NV. pictured in photo number one and photo number two. This count was published in Desert Plants Volume 8, Number 4 on pages: 169-171 in an article entitled: "The occurrence of the Desert Fan Palm, W. filifera, in Southern NV." In the tables which he provided and in the body of the report, the count and corresponding observations state:

"Although no seedling palms were found, 3 immature individuals, in addition to the 4 planted immature palms, occur near the spring."

(It should be noted here that in addition to living only a kilometer distant from the above spring himself, the author's family continues to live within a kilometer of Blue Point springs.)

First of all the statement that "no seedling palms were found" is incongruous with this author's observations and actual photographic evidence. Six years earlier this author noted as many as fifty or sixty Washingtonia eophylls (growing throughout a half kilometer stretch among grass) along Blue Point springs. Burros, Jackrabbits and even Lizards have been observed eating the tender seedlings and may have been responsible for the yearly demise of a vast majority of these. Until about 15 years ago, cattle had traditionally ranged the area's of Blue Point and Roger's springs which further kept these palms in check since the early 1900's. [12] Further-more, other palms down-stream not even a half a kilometer away were apparently completely missed in this 1986 accounting . In fact some small seedling palms have been closely observed over the last 17 years and the majority of them have grown extremely slowly. One particular seedling which was measured in 1979 at only 50 centimeters has grown only one meter as measured in January of 1996 (total 1.5 meters).

Additionally, the 1986 tables state that 7 immature palms were found as well as 1 mature one. This implies that at least four of the palms (palms which my photos indicate were at least 8 meters tall and bearing spadices in 1979) - are surprisingly termed as "immature". (I would also point out that along the stream banks among the grass, eophylls have been visible every year for the past 17 years.

Since the authors' photographs of the 5 main palms at Blue Point indicate that little or no growth occurred over the last 17 years (they have remained at 25-27 ft. tall or 8.2 meters), their growth to current height is thought to have taken place over substantial time. No argument is offered here regarding the obvious adventive status of the palms currently found at Blue Point, however the author contends that the following statement: "...the presence of just one mature individual at...Blue Point springs..." [sic] is incorrect and that the stated counts appear amiss as well.

Additionally, the account given in "Desert Plants" tentatively dates the palms to a 1950's fish farm operation. But less speculative eye-witness accounts I have gathered indicate the Palms were extant and already full grown at least 16 years earlier than that date in 1936. (see Washingtonia filifera, its History in Nevada revisited. by the author.) Judging from their slow growth over the last 20 or so years (as documented by the photographs) this author would

concur with those latter accounts since it's rather impossible to imagine these trees could only have been planted about 45 years ago, that they subsequently accomplished all their current growth in the first 25 years, and then 20 years ago suddenly ceased to grow.

Whatever the date of the Blue Point palm's origin however, the author contends that there were a minimum of 7 or more trees at Blue Point which were fruit bearing as early as 1979. If fruit bearing has anything to do with maturity, and if you can consider a 25 to 27 foot tall specimen, "mature."then it would seem there is a discrepancy between the tables and statements as they are presented in "Desert Plants" and the photo-graphical records which were in this author's possession 7 years earlier.

Another "Count" which this author finds disconcerting is an account of only 800 mature palms given in the same article in reference to Warm Springs NV (at Moapa). This author has made regular visits to this area for at least 17 years and can attest to the fact that the palms have neither naturally diminished nor spread radically since 1986 to present. The only exception is that the Wildlife, Fish and Game dept. on the site of the original "Blodale" ranch has been cutting or selling large quantities of palms . Cornett noted this fact stating that "several hundred palms have been sold to hotel developers in Las Vegas..." Indicating that the main difference between my new 1996 count and Cornett's 1986 count would be that if anything there ***should be*** fewer large volunteer palms now than then. Yet my 1996 count (as follows) is massively and conspicuously greater than the 1986 count. Since my counts were not liberal, this is something I find difficult to understand. *Especially* if one is to take the numbers as given in these papers as objective data to use in substantiating these types of hypotheses.

The author conducted a Warm Springs recount -Jan 96 using the following guidelines:

(a)- No palm under approx. 3 meters was counted. This was in an effort to hopefully eliminate any immature palms which may have been included in Cornett's count. (Two competent assistants were enlisted to ensure accuracy.)

(b)-Only palms which were clearly volunteer or wild were counted. (No border plantings,

straight lines or ornamentally planted palms were included.)

(c)-We each counted the same areas giving us three separate counts to compare & average.

(d)- We counted all natural springs of the area (not just the "official old resort of Warm Springs" This then, included the old Baldwin Ranch where the Mormon pool is and the all surrounding springs all over the upper Warm Springs valley.

The following approximate conservative counts were taken of the Warm springs in the Upper

Moapa at Moapa NV., 1996:

310 -in the large areas just before Warm springs ranch (the old Blodale place)

0 -at the location on the river just before Warm Springs (all the palms appeared planted)

530 -in the first main plot of the Mormon ranch

420 -the Palmer place formerly the "Blodale" ranch or Warm Springs Resort

175 -the west part of the Wildlife refuge (next to Pederson ranch)

40 -Pedersons

125 -along the corner of the Mormon property

340+ -Power plant property (a major spring is located here)

254 -LDS ranch with the old Hughes estate

80+ -outside the fence near the spring @ LDS property

120+ -from hill overlooking past LDS ranch between and around the corners of property

100+ -specifically between the old Doty ranch (the current Jim Hayworth place) and around the

opposing corner from LDS entrance.

116+ -old Baldwin place (difficult to count all of these because of distance)

15+ -in pasture near seeps behind Baldwin

2625 - total

The count only ten years earlier of 800 palms in the area of Warm Springs compared to the author's count of 2625 superficially implies that there has been an astounding increase of over 324% in numbers of mature wild palms. Knowing what the general growth rate has been in the area, (such as was mentioned regarding Blue Point springs earlier,) this is an astounding difference in counts.

The reality however, is that the large palms of the area are being cut and removed far faster than they are growing or being replaced. This can be verified by everyone in the valley who has lived there for the past 10 years. There has been a decrease by literally hundreds over the last ten years in Large wild palms. Furthermore a dreadful fire in summer of '94 gave the ones who were removing the palms the excuse they needed to bulldoze literally 100 to 200 palms from the Wildlife refuge, saying they had been destroyed when in fact nearly every single palm (of course,) rejuvenated and thrived the following spring.

All the forgoing being the case, it could safely be reasoned that had this author counted the same areas in 1986 (concurrently with the 1986 "count"), a number far greater than either count should have resulted. Missing from the 1996 count, the numbers of smaller and immature palms surely figure in the

many hundreds over and above the count of 2625. Even if the 300 immature palms of the 1986 figures were added together with it's own mature count of 800,... the 1996 amount would still far exceed the 1986 count by a factor of 250%. The logical explanation seems that perhaps the 1986 count did not include the other springs through the valley with their hundreds of wild palms all around them. Perhaps there was a presumption that the only volunteer or wild palms of the area existed on the old Blodale (Palmer Warm Springs) property, the Wildlife refuge and the First part of the Mormon ranch. However even then the count would have been far too insufficient. To not count the other palms seems arbitrary and at the least certainly makes it difficult to use the actual numbers statistically.

A summary of the forgoing points and the original contention

A profound difference in the 1986 and 1996 counts has been noted which leaves far too many unanswered questions. Furthermore, photographic records indicate conclusively that at least some of the 1986 counts had to be off by as much as 400%. To this author at least, it brings not only the accuracy of the published 1986 count into question, but the usefulness of such counts as well if one needs to use this data as support for a solid statistical argument.

Even though the author radically disagrees with this 1986 Warm Springs and Blue point counts, there is no contention with Cornett's other counts. So for the following argument, those counts are accepted as stated.

In light of the following factors (in the next section) which may easily explain any possible rise in population figures of wild palms (whether counted properly or not), it seems justifiable to say that just because there may be a recent identifiable increase in the plant's numbers it is not necessarily to say the palm cannot at once be considered relict. It does not appear that the general criteria for identifying 'relict' species were meant to be taken as literal rules but rather as general guidelines. How does a sudden ***explainable*** increase in population figures preclude a species which is ***already defined*** as a relict from remaining a relict? A sudden shift in population due to new ***extrinsic*** factors does not change the fact that this species was anciently restricted in the wild to scattered small pockets. It is this author's understanding that the meaning behind the criteria of "decreasing numbers" is simply that the numbers of a suspect plant in the wild are decreased to the point of being identifiable as a "leftover" species from a former time. Otherwise we should have to count all the millions of these palms which have been planted worldwide. Clearly this is not the purpose behind such a definition.

It wouldn't seem that anyone could convincingly argue that the current count of 25,000 or even 30,000 wild Desert Fan Palms in scattered groves comprise much more than "left-over". For such a stable genetically configured plant which also possesses many very strong reproductive capabilities it would seem this is indeed a true remnant. Many palms found in greater numbers in the wild bear seed which remains viable for only a few days with only a small percentage of those surviving to juvenile stages. Washingtonia seed is almost 100 % viable in massive quantities which may be stored for very long periods of time. It also endures amazing contrasts in moisture and temperature requirements and is not picky about soil and light and is very fast growing. In any conditions other than it's current desert domain, it would have surely been widespread. Therefore, it's comparatively spare numbers in the wild cannot be taken to represent much more than a remnant of what it may have been in earlier times.

Fossils spread all over North America may actually beg this issue. If there were a scarcity of such things, perhaps this whole argument could be given a wide benefit of the doubt. But the fact remains suspect fossils may be in relative abundance. While it is true they may not be assigned to the specific taxa of Washingtonia... there appear an abundance of fan fossils and the only fan palms extant today in these areas are the Washingtonia. Although clearly that is not a solid or valid argument in itself, it is surely just as valid as suggesting that ***Sabal*** is the represented taxon when no factors other than the ***spineless feature*** are forthcoming. When one notes that spineless petioles are common on Washingtonia the *Palmate* feature (not costapalmate) and the *relative proximity* to *extant groves* should become impressively more important as a ***possible*** indication of the represented Taxon.

As for compelling reasons for any general up trend in W. filifera populations, there are a number of explanations that appear reasonable. These more compelling and convincing arguments are: *Alternative reasons for possible increases this century in wild W. filifera populations:*

#1 Native Americans ceased harvesting millions of pounds of otherwise fertile seeds. about 75 years ago. Here lies opportunity for literally billions of fertile seeds to germinate on Oases floors. Clearly if seeds are being ground into flour and eaten they cannot become palms. Even in wet tropical countries where food is more abundant natives are known to scrape up every seed on the ground. Surely in the desert where food is scarce the aboriginal peoples would have allowed little seed to go to waste. This time frame relates precisely to population increases.

#2 Carpenter Bees which are known to destroy the Palm's flowers in search of pollen, now have literally hundreds of new pollen crops throughout the entire irrigated southwestern deserts and home landscapes. This has also happened only in the last 100 years. In fact this is the first time there has been so much pollen in as much as perhaps 6,000 years.

#3 Many hikers who happen to see the palms variously adorned with thousands of seeds, do exactly what man has been doing for ages: They pick them. One simply needs to travel through the state of California to see all of the thousands of obviously adventive palms. In Palm Springs the "palm" is absolutely ubiquitous. What happens to all those fertile Palm Springs 'street- seeds' every year?

#4 One should realize that irrigation has truly made the desert a garden in places where only a century ago people probably thought there would never be adequate water. Even the most remote places in the desert now have a developed water source somewhere. All this artificial hydrological improvement allows all types of birds and animals (and humans) to travel easily without fear of dehydration to more and more remote places. This author has traveled all the deserts extensively and in every desert there are some nearby developed water sources. It doesn't require a study grant to figure out why plants of all kinds are spreading to areas where they never were before. Animals are clearly more common in previously remote areas and all of them are potential seed carriers -including man.

In Final Summary to contention #3

It doesn't seem to this author that it has been sufficiently shown that the idea of a "shrinking" number of individuals either applies to *or is a valid criteria* for proof that W. filifera is ***not*** a relict taxon.

There appear too many other circumstances which have to be taken into account which cannot so easily be dismissed or segregated from one another. These other circumstances may easily preclude a definitive *NON-relict* status determination based simply on the opinion that numbers in the wild should be "declining" when in reality any current increase may readily be explained by other contemporary developments which have nothing at all to do with the palms' real status as a possible relict . No reasonable person could surely argue this point and still maintain any degree of objective credibility. In fact for all purposes, (explainable increases in filifera populations barred), this plant in the wild remains isolated in small numbers to a relatively few scattered pockets which it appears to have retreated following the wetter less hostile climates of 8,000 to 18,000 years earlier.

4- A Relict Species Should Display Genetic Divergence Between Populations due to Isolation.

This final contention is based on Electrophoretic studies by McClenaghan and Beauchamp (1986) which "revealed a surprisingly low genetic differentiation between isolated populations of desert fan palms in Anza -Borrego Desert State Park."[sic]

First of all, although to modern humans, it may seem that these areas in Anza-Borrego are "isolated" from each other, the area in reality, is quite small to birds and coyotes. Furthermore(and even more importantly,) these distances were not even considerable for the aboriginal people who populated the area less than 120 years ago. Even I have traveled distances in desert badlands such as The Anza Borrego which would surprise some. (Fifteen to twenty miles in a hot summer afternoon.) The desert aborigines however elevated desert hiking to an "art-form" traveling incredible distances over very short spans of time. This has been documented in several references that I am aware of. ("Fort Pahute"-Casebier-1974 and "A Southern Paiute History"-Euler -U. of Utah printing service 1966) Even Cornett's own studies show that Coyotes may (and do) travel 36 miles (and more?) in a few days and may hold palm seeds in their digestive tracts that entire time.

To typify these groves in Anza Borrego as "isolated" may be misleading to most modern day lay people who may be unfamiliar with the rigors of aboriginal desert life and the abilities and habits of these people. Additionally, one presumes the word "isolated" refers to the groves themselves being 'isolated' from one another when in fact, the groves were within the same state park. This entire search for genetic divergence completely fails to acknowledge that all the palm populations in the Cahuilla ancient homelands must have been completely and thoroughly and most undoubtedly homogenized over hundreds and possibly even thousands of years by the Aboriginal peoples who used this palm extensively and even traded it's seed.

Chief Patencio of the Cahuilla has stated several times that all the palms in all the locations were the result of early peoples spreading the seed. If nothing else this makes all the area's palm populations suspect of being the result of heavy cultural interaction at one time or another. This would include Joshua tree groves and Twentynine Palms groves as well since these were all within the scope of these aborigines.

In light of what surely must be an unavoidable and inescapable dynamic it doesn't seem surprising at all that the conclusions of such a study recorded this so called remarkable "genetic similarity". For these groves only having been without aboriginal peoples for about 75 years or so, anyone expecting a big jump in genetic divergence may surely be characterized as having "understated" the Anza Borrego Palms' Ethnobotanical past and the unmistakable underlying genetic stability of this genus. (We have 130 years and millions of specimens worth of proof that this genus, unlike other more decidedly unstable genera such as Brahea and Phoenix apparently has yet to produce even one genetic variant. This does not include the variant robusta. Meanwhile Brahea hybridizes freely easily producing variants and Phoenix may produce a variant with each seed planted.)

Additionally, these groves which are so close to one another with respect to Aborigines are no less remote for birds, or coyotes which certainly would likely have contributed much to genetic uniformity in such a restricted study area. Any plant which is used so heavily as a food or pollen resource over a given area is by animals or insects which can easily traverse that region will be far less likely to show any type of genetic variation since the genetics are constantly stirred by the external factors. Plants which are wind pollinated and not major food sources, particularly with regard to the use of the fruit, seed or pollen it would seem would be better candidates for findings of genetic dissimilarities in such studies. It would also seem that such findings ***must*** vary from species to species as far ***as what the data extrapolated can be interpreted to mean*** . Consideration must be given to a broad range of data and factors and not merely the simple finding of genetic dissimilarity ***alone*** . Clearly a finding of genetic dissimilarity in one species of one plant will infer an entirely different ancient picture than a similar finding in another species depending on other information about that particular species.

If the study were actually aimed at finding genetic differences rather than *confirming* genetic stability between populations, then samples should probably have been taken from *less disturbed* groves perhaps in Baja or in KOFA (King of Az..) grove or somewhere more isolated which may not have been so typically heavily used by Aborigines. (Even the casual observer and not so casual observer has noted what ***seem*** to be differences at KOFA which could turn out to be genetic as well as *environmental* .) (See photographic items of interest on this subject.)

To this author at least, ***If the object was to gather meaningful genetic data comparisons with regards to relict populations*** , it would appear that trying to find genetic differences between populations that are scarcely 30 miles distant from one another and heavily used by the same aboriginal people, may not have been worth the expense of Electrophoresis. That anyone may have expected this Electrophoretic study to have proven anything other than the fact that the Indians were using Anza Borrego for a long time seems a little odd. The only surprise to this author is that anyone would have been surprised. Humans may have been genetically mixing these plants for the last 10,000 years.

There is one more subtlety however, which has been overlooked in this entire argument. There are in fact, provable genetic variations or examples of 'divergence' right under our noses. First of all, Washingtonia filifera DOES have at least one genetic variant which is distant from most populations of filifera 'proper' in the wild. Washingtonia 'robusta' is not a true species, rather it is a genetic 'variant' of filifera.

There are also two more possible suspects. KOFA palms may also be genetically varied from other Washingtonia populations. One more suspect variant could be the fan palms indicated by the fossils themselves. It would appear premature to suggest there has never been genetic divergence in filifera populations.

To summarize, the forgoing discussion has shown that the contention stating there is no genetic diversity among groves of W filifera has been prematurely overstated, as well as under-substantiated. Certainly not substantiated at least, to warrant a declaration that this plant cannot be a relict throughout it's range.

5- Conclusive Remarks

Finally it is stated that these lines of evidence point to a recently evolved invasive species and not a relict. It is then speculated that the "rise of this cold-tolerant palm is associated with the glacial episodes of the Pleistocene (and) that W. filifera first appeared within the present boundaries of the United States no earlier than the end of the Illinoian glacial episode. First of all, this author disagrees with this and other writings which mistakenly presume that:

(a: These Palms have been shown to be provably obligate to tropical environs in all their ancient affinities and evolutionary associations.

(b: and that somehow these Palms have been shown to have evolved in what we today designate

as southern latitudes and from there spread northward.

In fact it would appear the opposite is more likely the case on all points.

To answer this I would like to point out that on page 55 and elsewhere in Genera Palmarum substantial indication is given that many if not most of the Western Hemisphere's earliest Palms evolved and existed in latitudes far north of current instances of all extant western Palms and that these Palms either retreated southward ultimately becoming restricted to the Southern U. S. or beyond, spread to other parts of the globe, became extinct or evolved into new species. (Note: Some palms whose fossils are found in the west (Phoenix [13] in Texas) are now only found in the east, while some whose fossils are found in the east (Serenoa in Europe) are now only found in the West. (This fact directly contradicts the idea that because one can pinpoint the center of a palms' extant distribution that it logically follows that point is where the Palm evolved. This is clearly not true.)

Very ancient Palm fossils have been found in latitudes all the way into Canada, Oregon, Alaska and Greenland. In fact, the subtribe of 'Livistoninae' (of which W. filifera is a member) not only has a strong Northern hemispherical bias (away from the tropics) but also encompasses some of the most drought and cold tolerant palms known. (Page 60, Genera Palmarum)

A large number of the earliest Palm fossils in the world have been discovered in The Dakotah group of the Rocky Mountains, British Columbia, Wyoming, Vancouver Island, and Northern Oregon. In fact, in Northern Oregon fossil palm leaves have been found in comparative abundance in relation to the rest of the world. [14] This suggests that the most ancient palms in this hemisphere likely spread or retreated southward from the north, NOT the opposite.

One should note that the Palm genera listed as having some of the earliest fossil records (tertiary) (Genera Palmarum- page 55 Uhl and Dransfield 1987) read like a who's who of extremely hardy and frost tolerant palms:

(Following are some of the earliest genera believed to be represented in the fossil records)

Sabal -Known to be cold tolerant to 0 - 5 degrees F in species

Serenoa - (Livistoninae) known to be cold tolerant to at least 10 -15 degrees F or lower

Livistona -(Livistoninae) known to be cold tolerant to at least 10- 5 degrees F in species

Trachycarpus -known to be cold tolerant to at least 5 degrees F

Phoenix - (Livistoninae) Known to be cold tolerant in some cases as low as 4 degrees F

Chamaerops -known to be cold tolerant to at least 10 -5 degrees F

Nannorrhops -known to survive 0 -to MINUS FIVE degrees F. (Another desert Palm)

(Thanks to Richard Woo of the Pacific Northwest Palm Society in Vancouver B.C., for finding the limits of so many Palms.) (By the way, while Washingtonia is said to take down to 10 degrees f. I have known it to survive even lower temperatures in certain instances.)

First of all since these palms are thought to be at once some of the oldest and the most cold tolerant genera in the world, it would be impossible to infer that "cold tolerance" is a late occurrence. This being true, cold tolerance would appear to have a far closer association with relict genera rather than late ones. Secondly there is no basis to suggest that Washingtonia evolved in more southern latitudes or near Brahea and from there spread northward. In fact there is a preponderance of evidence supporting just the opposite. In Genera Palmarum. (page 14) we read the following:

"..If we compare the distribution of palms... it is also clear that palms with adaxially split, induplicate, palmate, and costapalmate blades show a marked association with less tropical or more seasonal climates ..." (Furthermore:) "...If we assume that protopalms were slow-growing plants adapted to cooler, drier, more seasonal, and more open habitats, as are many of the more primitive genera today, and that the adaptation to the tropical rain forest followed (Moore 1973a), then it may be suggested that the elaboration of the pinnate leaf (reduplicate pinnate tropical Palms in South and Central America for example) is an adaptation (invasive ?) to the more limited sunlight of the rain forest." ***(Comments in parentheses and italics -the authors'.)***

In other papers it has additionally been suggested that since the Brahea genus is found near W. filifera in Baja it is likely to have descended from it. There is however, to this author's knowledge, no substantiated morphological basis for such a suggestion which seems arbitrarily based on geographic proximity, or superficially visible similarities , neither of which suggest evolutionary order of appearance nor genetic order of relationships.

There are a lot of palms in the Livistoninae subtribe which bear ***resemblances*** to Washingtonia. Washingtonia however is very different from Brahea and if either descended from the other (which this author does not believe), there are absolutely no mitigating extant or apparent suggestive morphological characteristics which at this point could not equally be argued occur as well in Palms such as Pritchardia or Colpothrinax and others to warrant such a statement. It appear to be based solely on geographical proximity and nothing else. In fact, the distinctive flowers of Washingtonia compared with the rest of the subtribe appear to suggest that even if there were a connection it would likely be so ancient and so indirect it would still undo all the other arguments which were made. In fact again Genera Palmarum states that: "The twelve genera of [Livistoninae] subtribe are so widely distributed...with so many disjunctions as to suggest a very ancient origin. . . . ***(and) Washingtonia stands apart from the rest of the members of the subtribe because of its unusual, more or less woody, sword like bracts and large, chaffy perianth segments."***

My own ***un-expert opinion*** is this: Washingtonia filifera comprises a monotypic genus (robusta being a variant) and is morphologically very distinctive which suggests in itself, a long period of isolation. Others who are qualified to make such observations ***may*** bear this out.

The truth is that Brahea is considered most closely related to Serenoa or Acoelorraphae NotWashingtonia , even though the closest occurrence of either of these genera is Florida. (Neither of these two genera bear as close a physical outward resemblance to Brahea as does Washingtonia. These two genera however are related to Brahea while Washingtonia has not been considered so in any taxonomic descriptions!) Sabal on the other hand, although strongly similar in outward appearance to Brahea, (much in the same way that Brahea is similar in outward appearance to Washingtonia), and although found in the same areas with Brahea genera. . .is in an entirely different subtribe. (Sabalinae) [Brahea is even known to have been used concurrently with Sabal by the Pima and Papago]. If one were to use such 'proximity' line of reasoning, one could theoretically suggest that Sabal must be immediately or directly related to Brahea. Clearly this would be a big mistake.

Geographical proximity & ***superficial*** external appearances appear to have little to do with establishing genetic order of lineage nor do (Sabalinae has more specialized gynoecium compared with Livistoninae.) The suggestion that Brahea is precursor to Washingtonia simply based on the idea they are historically found in the same areas or that they have similar outward appearance is to ignore certain accepted lines of morphological comparison. The separation of Baja from Mainland Mexico surely came about long after Washingtonia was well established and distinctively different from the other palms in it's own right. The fact that the subtribe

Livistoninae is found in Hawaii and Australia is proof of that. Apparently the subtribe had it's beginning before the continents themselves broke up. This is very ancient indeed. Cacti found with these palms had a very recent beginning comparatively since all cacti are basically only endemic to the Western Hemisphere. One could say that compared to these palms, they are

probably invasive. Yet I don't believe anyone is saying that. (Euphorbia in Africa the result of convergent evolution.) Washingtonia, it has been pointed out has very distinctive morphological characteristics which set it apart as 'unique' from all its' Livistoninae fellow members. This alone suggests a very ancient origin.

Washingtonia may indeed be one of the oldest plants in the western hemisphere. While Brahea and Washingtonia may be contemporaries or one may be older than the other, it seems impossible at this point to speculate about evolutionary order. The reality is that Washingtonia's differences and geographical occurrence may more reasonably argue that it is in fact more ancient in its' subtribe since no clear genetic order has been described relating it directly to any existing Livistoninae genera. Simultaneously it is a known fact that great numbers of fan palms existed very anciently to the immediate and far north of the palms' existing habitat, and that today no other fan palm now exists (wild) north of Washingtonia in western North America.

The flat palmate sometimes spineless KOFA palms which Cornett has himself more than once

referred to as: "perhaps the only relict populations of Washingtonia" ***may indeed be therepresented Taxon of the Mojave fossils.*** It is more than likely that this genus developed in and gradually retreated from the north evolving very anciently (as suggested by it's uniqueness). Any clear links or clues between this genus and others of the Livistoninae may have been destroyed forever beneath tons of ice or beneath Oceans or perhaps they simply became fossils all over the U. S. where it is known some of the world's most ancient palms existed. This palm may have thus been gradually limited in it's present range to scattered groves in it's most northerly location. None of Cornett's contentions suspend this possibility with any decisiveness.

Given the lateral world wide distribution of Livistoninae and the distinct uniqueness of the morphological characteristics of Washingtonia which set it apart from the rest of the subfamily, it is neither objectively possible to suggest that Brahea is precursor to Washingtonia, nor that Washingtonia evolved in the south and spread northward. It is just as likely they both developed from farther to the East, West or from the North and that they have no direct relation to each other at all. In lieu of more evidence, all of this is simply too difficult to speculate and the contested observations simply appear far too tenuous to allow one to substantiate arguments suggesting evolutionary order of appearance and genetic relationships.

6- Overall Summary

In summary then, it is the conclusion of this paper that the subject of Washingtonia's relict status was not ***in any way shape or form finalized*** by Cornett's published contentions. Many very important aspects of the problem have only been cursorily examined in the papers and research by Cornett. None of his major points have been sufficiently or *decisively* validated, and none lend any *observable* credence to his stated half digested theories. Therefore it *is* justifiable to contend that Axelrod's original work has been upheld and even validated while *not successfully discounted in any way* . All of the arguments considered by this report in regards to Cornett's contentions most vigorously uphold the original idea that the Washingtonia filifera palm is in fact a distinctly relict taxon.

-End

(Please Click HERE to go to photos and extra notes)

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WASHINGTONIA FILIFERA ITS HISTORY IN NEVADA REVISITED

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WASHINGTONIA FILIFERA: Nevada's Rejected Ancient Palms

END OF "Relict" REPORT

Condensed Moapa Palms Report | Petroglyphs & Palms | Plants & Climate in Oases
The Moapa People | The Cahuilla of Palm Springs | Global Warming: Palm Rebuttal
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Please allow these documents to *fully* load before attempting to *JUMP* to *footnotes* ...otherwise the footnote links will not work.

Footnotes follow. Don't forget to look at the Photos supporting this article.

1. "The ...expansion north suggests ...the climate has warmed to allow this species to invade." -Cornett, James W. 1991 Population Dynamics of the

Palm, Washingtonia filifera, and Global Warming. SBCMA Quarterly 39(2) page 46. (back to text)

2. This author's observation is that a theory of "Global warming" as a contributing factor in a so called "expansion" [sic] of W. filifera northward, fails to consider important and observable factors. See author's rebuttal:"W. Filifera, and Global Warming, -A Rebuttal". -unpublished paper -1996. (back)

3. Genera: (In general order of descending occurrence.) Washingtonia, Phoenix, Trachycarpus, Chamaerops, Brahea, Syagrus, Sabal and Livistona. (back)

4. See climate data references and table 2. (back)

5. Logandale interestingly, was the location where Cornett collected his information about the Winter lows of that area.: James Cornett, [pers. comm.] (back)

6. Although Jeanne Clark on page 32 of Nevada State Museum anthropological papers no. 19 states that the area has only 275 frost free days on average I have found absolutely no hard local data to support this vastly conflicting assertion. (Yet this has been repeatedly used as reference.) This incredibly high number of average days per year with temps 32 deg F. or lower amounts to three full months of daily average frost episodes per year. The author lived in the area and collection of frost data was one extremely important pursuit. Much time was spent at the county extension in Overton pouring over frost data, If frost incidence had ever been so high it would have most certainly have been noted. One possibility is that Las Vegas rather than local data was used under the incorrect assumption that it was all the same. Another possibility is that average temps for Barstow or other far colder Mojave locales were used or lumped together with Moapa...all of which are quite different from each other with respect to frost data, elevation and plant communities. The author's observation is that the Moapa valley's bear far more resemblance to Sonoran communities than to other Upper Mojave communities to the south with respect to all aspects of Flora, Fauna, Climatological and elevation considerations. In fact, the author would suggest that the Moapa drainage is actually a very small and isolated portion of the larger Colorado Desert community to the south with only insignificantly cooler winters. . . (Logandale of course being a possible exception to this as noted.) (back)

7. Arizona's Own Palm: Washingtonia filifera - Desert Plants Vol 5, No. 3 1983 -Victor J. Miller -University of Arizona for Boyce Thompson Arboretum. (back)

8. Jeanne Clark's report in State anthropological papers appears to suggest that Flora studies (by Bradley and Deacon in 1965) apparently omit W. Filifera from their accounts of riparian communities specifically & conspicuously in Moapa Warm Springs headwaters while simultaneously admitting Tamarix "Gallica" -(chinensis?) (an undisputed nuisance introduction by whites in the 1900's) even though at the time of the survey they had to have been surrounded by literally hundreds if not thousands of Palms. This is disconcerting to this author. (back)

9. "The taxonomic affinity of fossil palm leaves from CA is based (in part) upon the presence or absence of spines on the petioles." Cornett, J. W., "Spineless Petioles in W. Filifera" -Madrono, Vol 33 No.1, p 77 1986 (back)

10. [ibid.] (back)

11. Brown, D. E., N. B. Carmony, C. H. Lowe, and R. M. Turner. -1976. "A Second Locality for Native California Fan Palms in Arizona. Journal of the Arizona Academy of Science 11:37-41. (back)

12. The Leavitt family ranged cattle for at least 40 years in this area. The cattle likely heavily impacted the springs. (back)

13. Phoenix, is more closely related to Washingtonia than is Sabal even though both the latter

are Fan palms. (back)

14. It shouldn't be necessary to point out that fossils are most likely to be found where the conditions have repeatedly been very suitable for such occurrence during the evolution span of the target subject. (back)
