## POPULATION MOVEMENTS

Our main conclusions are:

I. Africa 1835. The number of whites was about 135,000.

This figure is meant to exclude the army and navy. The European military population, excl. Algeria, was probably about 9,000; the number of European troops in Algeria may have been considerable.[1] Our total of 132,205 for the civil population is not all-inclusive, since it does not comprise Mozambique, the Spanish *presidios* in Morocco with numerous convicts, the other Spanish and the Danish and Dutch colonies; on the other hand, it shows for some important colonies figures of a later date than 1835, for example, for the Cape Colony the population as of 31 December 1837, which exceeded that of 1835.

II. Africa, 31 December 1935. The number of whites was about 4,000,000.

In making the entries for whites we have excluded, as far as possible, the army and navy. The number of European troops still included is too small to affect the total of 4,000,000.

The table on page 91 shows the number of whites in 1835 and 1935 by geographical divisions. It appears that on the African continent, excluding the Mediterranean countries and the South, i.e. in a territory considerably larger than Europe, the number of whites was about 1,000 in 1835 and about 100,000 in 1935.

III. America 1835. The number of whites was about 18,600,000, and the number of negroes about 9,800,000.

We have segregated, as far as possible, the number of troops from Europe, because they constitute in some colonies a considerable part of the white population.

[1] See Malte-Brun, *Géographie complète de la France et de ses colonies*, p. 452, Paris, 1857: 'The effectives of the army employed in Algeria vary according to the political circumstances; it numbered 21,511 men in 1832, and was brought to 105,000 men in 1846 . . .' These figures, of course, include native troops.



## APPENDIX

*Whites in Africa 1835 and 1935*

| *Geographical Divisions* | *Sq. miles* | *1835* | *1935* |
|---|---|---|---|
| Mediterranean countries* . | 2,168,000 | 20,000 | 1,660,000 |
| Union of South Africa . . | 472,000 | 66,000 | 1,950,000 |
| Rest of South Africa† . . | 1,884,000 | 3,000 | 190,000 |
| Rest of the continent . . | 6,798,000 | 1,000 | 100,000 |
| Islands . . . . | 242,000 | 45,000 | 100,000 |
| Total | 11,564,000 | 135,000 | 4,000,000 |

* Egypt, Libya, Tunis, Algeria, Moroccos, Spanish Northern Africa, Tangier.

† Angola, South-West Africa, the Rhodesias, Nyasaland, Bechuanaland, Basutoland, Swaziland, Mozambique.

IV. America, 31 December 1935. The number of whites was about 172,000,000, the number of Indians about 53,000,000, and the number of negroes about 39,500,000.

There were besides about 1,400,000 'Others' who were nearly all Asiatics (Chinese, Japanese, East Indians, &c.).

The following table shows the number of whites and negroes in North, Central, and South America in 1835 and 1935.

*White and Negro Population in America 1835 and 1935*

| *Geographical Divisions* | *White* | | *Negro* | |
|---|---|---|---|---|
| | *1835* | *1935* | *1835* | *1935* |
| North America . | 13,800,000 | 124,300,000 | 2,600,000 | 12,400,000 |
| Central America | 1,900,000 | 6,900,000 | 2,700,000 | 8,400,000 |
| South America . | 2,900,000 | 40,900,000 | 4,500,000 | 18,700,000 |
| Total | 18,600,000 | 172,100,000 | 9,800,000 | 39,500,000 |

V. Oceania, 31 December 1935. The number of whites was about 8,300,000[1] and the number of natives about 1,700,000.

There were besides about 400,000 'Others' (mostly Asiatics).

[1] The number of whites in 1835 was about 120,000; see p. 8.

TABLE I. *Whites in Africa about 1835*

| Countries | Year | Population | |
|---|---|---|---|
| | | *Civil* | *Military* |
| Egypt | 1835 | 5,000 | — |
| Morocco | 1835 | 500 | — |
| *British Colonies:* | | | |
| Cape of Good Hope | 1837 | 68,148 | 1,962 |
| Mauritius | 1835 | 15,000 | 1,376 |
| Seychelles | 1825 | 582 | — |
| St. Helena | 1836 | 2,200 | 473 |
| West Coast | 1838 | 500 | 712 |
| *French Colonies:* | | | |
| Algeria | 1835 | 11,221 | .. |
| Senegal | 1836 | 195 | 376 |
| Madagascar | 1838 | 29 | 51 |
| Bourbon (Réunion) | 1836 | 24,000 | 667 |
| *Portuguese Colonies:* | | | |
| Cape Verde Is. and Guinea | 1834 | 2,900 | 574 |
| St. Thomé and Principe Is. | 1844 | 100 | 160 |
| Angola | 1844 | 1,830 | 1,606 |
| Total | | 132,205 | 7,957 |

The sign '—' indicates nil, the sign '..' unknown.

## SOURCES FOR TABLE I

**Egypt.** See Adriano Balbi, 'L'Egitto', *Gazzetta di Milano*, July 1836, reprinted in Balbi, *Scritti geografici, statistici e vari*, vol. iii, p. 4, Torino, 1841.

**Morocco.** See Balbi, 'Cenni sulla regione Sahara-Atlante', *Gazzetta di Milano*, June 1836, reprinted in *Scritti geografici*, &c., vol. ii, p. 302.

**Cape of Good Hope.** See for civilians, Cape of Good Hope, *Blue Book 1837*, pp. 198–9. The number is not known for 1829–36. *Blue Book 1828*, pp. 242–3, gives for 1828, 55,355





Whites. The population figures do not include the resident strangers, some of whom were Whites. See for troops, 1 Jan. 1838, *Parliamentary Papers 1837–8*, vol. 37, p. 133.

**Mauritius.** In 1827 there were 8,111 Whites and 15,444 Free Coloured (see Robert Montgomery Martin, *Statistics of the Colonies of the British Empire*, p. 503, London, 1839). In 1835 there were 29,612 Whites and Free Coloured (see *Parliamentary Papers 1837–8*, vol. 47, p. 492). Martin, *Statistics of the Colonies* (1839), 'Statistical Chart', gives 15,000 Whites. Moreau de Jonnès, *Recherches statistiques sur l'esclavage colonial*, pp. 43–4, Paris, 1842, gives for 1832, 16,000 Whites. These figures probably do not include the aliens and resident strangers, who in 1836 included 670 European aliens (see Martin, *Statistics of Colonies*, p. 503). See for troops, 1 Jan. 1836, *Parliamentary Papers 1837–8*, vol. 37, pp. 134–5.

**Seychelles.** See Martin, *Statistics of Colonies*, p. 519. The troops are apparently included in the figures for Mauritius (see ibid., 'Statistical Chart').

**St. Helena.** See for estimate of civilians, Martin, *Statistics of Colonies*, p. 522; for troops, 1 Jan. 1837, *Parliamentary Papers 1837–8*, vol. 37, pp. 134–5.

**West Coast.** Martin, *Statistics of Colonies*, pp. 537, 553, 'Chart', gives for Sierra Leone, 1836: 105; Gambia, 1836: 43; Cape Coast Castle, 10; Accra, 5; Dix Cove, 1; Annamaboe, 2. He further states: 'In the aggregate we may estimate the number of British subjects, on the western coast of Africa, at about 50,000, of whom but 500 are Europeans.' For troops, 1 Jan. 1838, see *Parliamentary Papers 1837–8*, vol. 37, p. 133.

**Algeria.** See René Ricoux, *La démographie figurée de l'Algérie*, p. 33, Paris, 1880.

**Senegal.** See *Annales maritimes et coloniales 1838*, part ii, vol. i, pp. 630–1. Data refer to 31 Dec. 1836; the civil population includes fifty-one officials and their families.

**Madagascar.** See *Notices statistiques sur les colonies françaises*, vol. iv (1840), pp. 30, 34–6, 158–9. The civil population includes sixteen officials.

**Bourbon.** The number of white residents was 18,125 in 1826 (see Thomas, *Essai de statistique sur l'île de Bourbon*, MS. quoted in *Annales maritimes et coloniales 1828*, part ii, vol. ii, pp. 375–6), and 29,181 on 31 Dec. 1843 (see *Tableaux de population 1843*, p. 30). For the intervening period we have found the following estimates:

1827: 20,000; Moreau de Jonnès, p. 23.

1836: More than two-thirds of the total free population (39,817 incl. officials and troops); *Notices statistiques sur les colonies françaises*, vol. ii (1838), pp. 26, 30, 34–5.

1838: 20,000; Moreau de Jonnès, p. 24.

For the number of officials (125) and troops (667), see *Notices statistiques*, vol. ii (1838), p. 35.

**Cape Verde Islands and Guinea.** José Joaquim Lopes de Lima (*Ensaios sobre a Statistica das Possessões Portuguezas na Africa Occidental*, &c., Book I, part i, pp. 1, 6, 69A, Lisbon, 1844) gives 55,833 as population of the Cape Verde Islands according to the census of 1834, estimates the total population of the establishments in Portuguese Guinea at 4,500 incl. troops, and gives 574 as the strength of the troops of the entire colony on 31 Dec. 1843. He estimates the ratio of Whites to Coloured for the entire colony at 1: 20.

**St. Thomé and Principe Islands.** Lopes de Lima (*Ensaios*, Book II, part i, pp. 2A, 52A, 52B) gives for 1844 as number of Whites and Mulattos in St. Thomé Island 47, and in Principe Island 138, and as troops 80 in each island.

**Angola.** Lopes de Lima (*Ensaios*, Book III, part i, pp. 4A, 139, Lisbon, 1846) gives as number of Whites 1,830, and as number of troops in 1845, 1,606. João de Andrado de Corvo (*Estudos sobre as Provincias Ultramarinas*, vol. i, p. 206, Lisbon, 1883) indicates that the figure of 1,830 refers to 1844. Recent official statistics (see Colónia de Angola, *Boletim Trimestral da Repartição Central de Estatística Geral 1934*, p. 114) date it, probably wrongly, as of 1846.





TABLE II. *White and Total Population in Africa about 1935*

| Countries | | White Population | | Total Population | |
|---|---|---|---|---|---|
| | | Date | Number | Date | Number |
| Algeria | French | 30 June 1934 | 900,000 | 30 June 1934 | 6,910,000 |
| Angola | Port. | 31 Dec. 1933 | 58,698 | 31 Dec. 1933 | 3,098,281 |
| Basutoland | Brit. | 1934 | 2,000 | 31 Dec. 1934 | 570,000 |
| Bechuanaland | Brit. | 1934 | 1,660 | 31 Dec. 1933 | 160,000 |
| Belgian Congo | Belg. | 1 Jan. 1935 | 17,845 | 31 Dec. 1934 | 9,300,836 |
| Cameroons | Br. M. | 31 Dec. 1934 | 316 | 31 Dec. 1934 | 778,352 |
| Cameroons | Fr. M. | 31 Dec. 1934 | 2,047 | 31 Dec. 1934 | 2,230,201 |
| Cape Verde Islands | Port. | 1933 | 854 | 31 Dec. 1933 | 160,000 |
| Egypt | | 30 June 1934 | 200,000 | 30 June 1934 | 15,230,000 |
| Eritrea | Ital. | 21 Apr. 1931 | 4,560 | 31 Dec. 1933 | 600,000 |
| Ethiopia | | 30 June 1935 | 3,000 | 31 Dec. 1933 | 5,500,000 |
| French Equat. Africa | French | 1 July 1931 | 4,591 | 1934 | 3,430,000 |
| French West Africa | French | 31 Dec. 1934 | 18,631 | 31 Dec. 1934 | 14,456,740 |
| Gambia | Brit. | 31 Dec. 1933 | 250 | 31 Dec. 1933 | 208,094 |
| Gold Coast | Brit. | 30 June 1934 | 2,400 | 30 June 1934 | 3,116,265 |
| Kenya | Brit. | 31 Dec. 1934 | 17,501 | 31 Dec. 1934 | 3,094,279 |
| Liberia | | 1933 | 150 | 31 Dec. 1933 | 2,500,000 |
| Libya | Ital. | 21 Apr. 1931 | 49,407 | 31 Dec. 1933 | 720,000 |
| Madagascar incl. dep. | French | 31 Dec. 1933 | 24,610 | 31 Dec. 1933 | 3,820,987 |
| Mauritius and dep. | Brit. | 31 Dec. 1934 | 700 | 31 Dec. 1934 | 404,190 |
| Morocco | French | 31 Dec. 1933 | 160,000 | 31 Dec. 1933 | 5,500,000 |
| Morocco | Span. | 1933 | 32,804 | 1933 | 720,273 |
| Mozambique | Port. | 1935 | 23,131 | 1935 | 4,006,001 |
| Nigeria | Brit. | 23 Apr. 1931 | 4,674 | 31 Dec. 1933 | 19,349,921 |
| Northern Africa | Span. | 31 Dec. 1933 | 90,000 | 31 Dec. 1933 | 115,000 |
| Northern Rhodesia | Brit. | 31 Dec. 1934 | 11,464 | 31 Dec. 1934 | 1,370,490 |
| Nyasaland | Brit. | 31 Dec. 1934 | 1,800 | 31 Dec. 1934 | 1,603,914 |
| Portuguese Guinea | Port. | 1930 | 1,226 | 31 Dec. 1933 | 380,000 |
| Réunion | French | 31 Dec. 1933 | 70,000 | 31 Dec. 1933 | 200,000 |
| Rio de Oro incl. Ifni | Span. | 31 Dec. 1930 | 300 | 31 Dec. 1933 | 20,000 |
| Ruanda-Urundi | Bel. M. | 1 Jan. 1935 | 868 | 1 Jan. 1935 | 3,293,269 |
| St. Helena and dep. | Brit. | 31 Dec. 1934 | 400 | 31 Dec. 1934 | 4,397 |
| St. Thomé, Principe | Port. | 1921 | 1,115 | 31 Dec. 1933 | 60,000 |
| Seychelles | Brit. | 31 Dec. 1934 | 500 | 31 Dec. 1934 | 29,406 |
| Sierra Leone | Brit. | 26 Apr. 1931 | 651 | 31 Dec. 1933 | 1,800,000 |
| Somali Coast | French | 1 Jan. 1931 | 628 | 31 Dec. 1933 | 70,000 |
| Somaliland | Brit. | Apr. 1931 | 68 | 31 Dec. 1933 | 347,385 |
| Somaliland | Ital. | 21 Apr. 1931 | 1,668 | 31 Dec. 1933 | 1,000,000 |
| South-West Africa | S.A.M. | 30 June 1935 | 31,800 | 31 Dec. 1934 | 266,930 |
| Southern Rhodesia | Brit. | 30 June 1935 | 54,000 | 30 June 1935 | 1,258,860 |
| Spanish Guinea | Span. | 31 Dec. 1930 | 1,539 | 31 Dec. 1933 | 120,000 |
| Sudan | Ang.E. | 1933 | 5,341 | 31 Dec. 1934 | 5,816,390 |
| Swaziland | Brit. | 31 Dec. 1934 | 2,830 | 31 Dec. 1934 | 126,560 |
| Tanganyika | Br. M. | 31 Dec. 1934 | 8,193 | 31 Dec. 1934 | 4,988,338 |
| Tangier | Int. A. | 1934 | 16,500 | 1934 | 60,000 |
| Togoland | Br. M. | 30 June 1934 | 43 | 30 June 1934 | 328,077 |
| Togoland | Fr. M. | 31 Dec. 1934 | 418 | 31 Dec. 1934 | 762,629 |
| Tunis | French | 31 Dec. 1933 | 180,000 | 31 Dec. 1933 | 2,500,000 |
| Uganda | Brit. | 31 Dec. 1934 | 1,959 | 31 Dec. 1934 | 3,640,636 |
| Union of South Africa | Br. D. | 30 June 1935 | 1,944,200 | 30 June 1935 | 8,600,300 |
| Zanzibar | Brit. | 1931 | 278 | 31 Dec. 1934 | 244,104 |
| Total | | | 3,957,618 | | 144,879,105 |

TABLE III. *White and Negro Population in America about 1835*

NORTH AMERICA

| *Countries* | *Date* | *White* | *Troops from Europe* | *Negro* |
|---|---|---|---|---|
| United States | 31 Dec. 1835 | 12,445,000 | — | 2,619,000 |
| Canada (Brit.) | 31 Dec. 1835 | 1,300,000 | 4,053 | 5,000 |
| Newfoundland (Brit.) | 1836 | 73,705 | .. | — |
| Greenland (Dan.) | 1840 | 251 | .. | — |
| St. Pierre and Miquelon (French) | 31 Dec. 1835 | 1,472 | 11 | — |
| Alaska (Russian) | 1830 | 940 | .. | — |
| Total | | 13,821,368 | 4,064 | 2,624,000 |

CENTRAL AMERICA

| | | | | |
|---|---|---|---|---|
| Mexico | 1827 | 1,200,000 | — | 50,000 |
| Republic of Central America | 1835 | 50,000 | — | 25,000 |
| Haiti | 1827 | 20,000 | — | 910,000 |
| British Honduras | 31 Dec. 1835 | 222 | 743 | 2,321 |
| British West Indies: | | | | |
| Jamaica | 1835 | 15,000 | 2,881 | 370,000 |
| Cayman Islands | 1827 | 100 | — | 1,500 |
| Trinidad | 1834 | 3,632 | 350 | 39,381 |
| Tobago | 1833 | 304 | 100 | 12,894 |
| Grenada | 1834 | 661 | 200 | 24,761 |
| St. Vincent | 1835 | 1,300 | 250 | 26,000 |
| Barbados | 1834 | 12,797 | 500 | 89,434 |
| St. Lucia | 1835 | 881 | 300 | 13,669 |
| Dominica | 1833 | 720 | 250 | 18,000 |
| St. Kitts | 1834 | 1,200 | 50 | 22,280 |
| Montserrat | 1834 | 312 | 50 | 7,228 |
| Antigua | 1834 | 1,900 | 200 | 33,016 |
| Barbuda | .. | 50 | — | 1,450 |
| Nevis | 1831 | 700 | 50 | 11,142 |
| Anguilla | .. | 200 | — | 1,800 |
| Tortola and Virgin Islands | 1834 | 800 | 50 | 5,735 |





CENTRAL AMERICA (*cont.*)

| *Countries* | *Date* | *White* | *Troops from Europe* | *Negro* |
|---|---|---|---|---|
| Bahamas (Brit.) | 1834 | 4,667 | 200 | 13,195 |
| Bermudas (Brit.) | 1835 | 4,264 | 600 | 4,456 |
| St. Croix (Dan.) | 31 Dec. 1825 | 2,223 | .. | 30,128 |
| St. Jean (Dan.) | 31 Dec. 1825 | 150 | .. | 5,040 |
| St. Thomas (Dan.) | 31 Dec. 1825 | 850 | .. | 2,926 |
| Curaçao (Dutch) | 1830 | 2,781 | .. | 10,059 |
| St. Eustache (Dutch) | 1830 | 1,000 | .. | 15,000 |
| St. Martin (Dutch) | 1830 | 500 | .. | 5,500 |
| Guadeloupe (French) | 31 Dec. 1835 | 12,000 | 2,138 | 116,000 |
| Martinique (French) | 31 Dec. 1835 | 9,500 | 2,020 | 107,000 |
| Cuba (Span.) | 31 Dec. 1835 | 370,000 | .. | 520,000 |
| Puerto Rico (Span.) | 1836 | 188,869 | .. | 168,217 |
| St. Barthélemy (Swed.) | 1827 | 1,992 | .. | 6,210 |
| Total | | 1,909,575 | 10,932 | 2,669,342 |

SOUTH AMERICA

| | | | | |
|---|---|---|---|---|
| Argentine | 31 Dec. 1835 | 600,000 | — | 50,000 |
| Brazil | 31 Dec. 1835 | 1,100,000 | — | 3,700,000 |
| Other Independent States | 31 Dec. 1835 | 1,200,000 | — | 550,000 |
| Guiana (Brit.) | 1834 | 2,883 | 700 | 91,060 |
| Falkland Isl. and dep. (Brit.) | .. | 25 | .. | — |
| Surinam (Dutch) | 1830 | 2,029 | .. | 55,012 |
| Guiana (French) | 31 Dec. 1836 | 1,273 | 623 | 20,665 |
| Total | | 2,906,210 | 1,323 | 4,466,737 |

AMERICA

| | | | | |
|---|---|---|---|---|
| Total | | 18,637,153 | 16,319 | 9,760,079 |

The sign '—' indicates nil, the sign '..' unknown.

All these figures are quite uncertain. Taking into account estimates of other authorities, we have entered for 31 Dec. 1835: 1,200,000 Whites and 550,000 Negroes (including Mulattos).

**British Guiana, Falkland Islands.** See Martin, *Statistics*, 'Chart'; Martin, *British Colonies*, vol. iv, part ii, p. 179.

**Surinam.** See Moreau de Jonnès, pp. 49–50.

**French Guiana.** The number of Whites was 1,280 in 1827 (see Moreau de Jonnès, p. 22), and 1,025 on 31 Dec. 1838 (see *Notices statistiques*, vol. iv, p. 158). The ministry of the navy and the colonies estimated their number for 31 Dec. 1836 at about 1,100 (see ibid., vol. ii, p. 178). For officials (173) and troops (623), see ibid., p. 183 (we presume that these are the figures for Whites only). The total (white and black) population (incl. all officials and troops) on 31 Dec. 1836 was 22,661 (see ibid., pp. 182–3).





## TABLE IV. *Races in America about 1935*

### NORTH AMERICA

| *Countries* | *Date* | *White* | *Indian* | *Negro* | *Other* | *Total* |
|---|---|---|---|---|---|---|
| Alaska (U.S.) | 1 Oct. 1929 | 28,640 | 30,012 | 136 | 490 | 59,278 |
| Canada (Br. D.) | 1 June 1934 | 10,591,000 | 129,000 | 20,000 | 95,000 | 10,835,000 |
| Greenland (Dan.) | 1 Oct. 1930 | 408 | .. | .. | 16,222 | 16,630 |
| Labrador (Newf.) | 31 Dec. 1934 | 3,151 | .. | .. | 1,300 | 4,451 |
| Newfoundland (Br. D.) | 31 Dec. 1934 | 289,272 | 200 | .. | .. | 289,472 |
| St. Pierre and Miquelon (Fr.) | 1 July 1931 | 4,321 | .. | .. | .. | 4,321 |
| United States | 30 June 1935 | 112,763,000 | 1,818,000 | 12,317,000 | 274,000 | 127,172,000 |
| Total | | 123,679,792 | 1,977,212 | 12,337,136 | 387,012 | 138,381,152 |

### CENTRAL AMERICA

| | | | | | | |
|---|---|---|---|---|---|---|
| Antigua (Br.) | 31 Dec. 1934 | 1,015 | .. | 32,045 | .. | 33,060 |
| Bahamas (Br.) | 31 Dec. 1934 | 10,202 | .. | 53,561 | .. | 63,763 |
| Barbados (Br.) | 31 Dec. 1934 | 12,771 | .. | 169,669 | .. | 182,440 |
| Bermuda (Br.) | 31 Dec. 1934 | 11,807 | .. | 17,848 | .. | 29,655 |
| Brit. Honduras (Br.) | 31 Dec. 1934 | 1,095 | 53,102 | .. | 547 | 54,744 |
| Cayman Islands (Br.) | Aug. 1934 | 2,368 | 2 | 3,637 | 2 | 6,009 |
| Costa Rica | 31 Dec. 1934 | 480,613 | 56,543 | 28,271 | .. | 565,427 |
| Cuba | 31 Dec. 1934 | 2,711,949 | .. | 1,252,282 | 23,929 | 3,988,160 |
| Curaçao (Dutch) | 31 Dec. 1934 | 3,330 | .. | 79,091 | 833 | 83,254 |
| Dominica (Br.) | 31 Dec. 1934 | 695 | 420 | 45,183 | .. | 46,298 |
| Dominican Rep. | May 1935 | 59,125 | 7,391 | 1,404,214 | 7,391 | 1,478,121 |
| Grenada (Br.) | 31 Dec. 1934 | 1,031 | .. | 79,451 | 3,406 | 83,888 |
| Guadeloupe (Fr.) | 31 Dec. 1933 | 21,600 | .. | 248,400 | .. | 270,000 |
| Guatemala | 31 Dec. 1934 | 113,334 | 2,153,348 | .. | .. | 2,266,682 |
| Haiti | 31 Dec. 1934 | 10,000 | .. | 2,490,000 | .. | 2,500,000 |
| Honduras | 30 Nov. 1934 | 19,254 | 895,297 | 48,134 | .. | 962,685 |
| Jamaica (Br.) | 31 Dec. 1934 | 18,718 | .. | 1,057,215 | 28,842 | 1,104,775 |
| Martinique (Fr.) | 31 Dec. 1934 | 10,286 | .. | 219,193 | 15,429 | 244,908 |
| Mexico | 31 Dec. 1933 | 1,700,000 | 15,840,000 | 10,000 | 50,000 | 17,600,000 |
| Montserrat (Br.) | 31 Dec. 1934 | 105 | .. | 13,056 | .. | 13,161 |
| Nicaragua | 31 Dec. 1933 | 134,400 | 588,800 | 76,800 | .. | 800,000 |
| Panama | 31 Dec. 1933 | 81,565 | 302,692 | 95,241 | 4,282 | 483,780 |
| Panama Canal Zone (U.S.) | 30 June 1934 | 22,120 | .. | 23,970 | 310 | 46,400 |
| Puerto Rico (U.S.) | 30 June 1935 | 1,239,550 | 2 | 429,310 | 38 | 1,668,900 |
| St. Kitts and Nevis (Br.) | 31 Dec. 1934 | 1,197 | .. | 36,324 | .. | 37,521 |
| St. Lucia (Br.) | 31 Dec. 1934 | 1,000 | .. | 62,804 | .. | 63,804 |
| St. Vincent (Br.) | 31 Dec. 1934 | 2,429 | .. | 49,808 | 1,385 | 53,622 |
| Salvador | 31 Dec. 1934 | 78,725 | 1,495,770 | .. | .. | 1,574,495 |
| Trinidad & Tobago (Br.) | 31 Dec. 1934 | 43,206 | .. | 237,632 | 151,220 | 432,058 |
| Turks & Caicos Is. (Br.) | 31 Dec. 1934 | 160 | .. | 5,140 | .. | 5,300 |
| Virgin Is. (Br.) | 31 Dec. 1934 | 38 | .. | 5,450 | .. | 5,488 |
| Virgin Is. (U.S.) | 1 Apr. 1930 | 2,010 | .. | 19,962 | 40 | 22,012 |
| Total | | 6,795,698 | 21,393,367 | 8,293,691 | 287,654 | 36,770,410 |

TABLE IV (*cont.*)

SOUTH AMERICA

| *Countries* | *Date* | *White* | *Indian* | *Negro* | *Other* | *Total* |
|---|---|---|---|---|---|---|
| Argentine | 31 Dec. 1934 | 11,914,000 | 100,000 | 50,000 | 100,000 | 12,164,000 |
| Bolivia | 31 Dec. 1933 | 450,000 | 2,490,000 | 57,000 | 3,000 | 3,000,000 |
| Brazil | 31 Dec. 1933 | 18,409,000 | 10,327,000 | 15,939,500 | 224,500 | 44,900,000 |
| Brit. Guiana (Br.) | 31 Dec. 1934 | 2,039 | 8,601 | 173,125 | 139,406 | 323,171 |
| Chile | 31 Dec. 1935 | 3,177,000 | 1,299,000 | 20,000 | 10,000 | 4,506,000 |
| Colombia | 30 June 1934 | 2,928,989 | 5,145,021 | 284,530 | 10,000 | 8,368,540 |
| Ecuador | 31 Dec. 1933 | 160,000 | 1,560,000 | 280,000 | .. | 2,000,000 |
| Falkland Is. and dep. (Br.) | 31 Dec. 1934 | 3,087 | .. | .. | .. | 3,087 |
| French Guiana, Inini (Fr.) | 31 Dec. 1933 | 25,000 | 1,000 | .. | .. | 26,000 |
| Paraguay | 31 Dec. 1933 | 90,000 | 800,000 | 10,000 | .. | 900,000 |
| Peru | 1934 | 625,000 | 5,325,000 | 200,000 | 100,000 | 6,250,000 |
| Surinam (Dutch) | 31 Dec. 1934 | 1,886 | 66,682 | 17,000 | 78,517 | 164,085 |
| Uruguay | 31 Dec. 1934 | 2,017,040 | 1,000 | 1,000 | 1,000 | 2,020,040 |
| Venezuela | 31 Dec. 1933 | 99,000 | 2,046,000 | 1,155,000 | .. | 3,300,000 |
| Total | | 39,902,041 | 29,169,304 | 18,187,155 | 666,423 | 87,924,923 |

AMERICA

| | | | | | | |
|---|---|---|---|---|---|---|
| Total | | 170,377,531 | 52,539,883 | 38,817,982 | 1,341,089 | 263,076,485 |

## SOURCES FOR TABLE IV

Where no source is mentioned we have applied the proportions given by Loyo to the most recent population estimate in *Statistical Year-Book of the League of Nations 1934/35*, pp. 19–20 (for Bahamas to the population in *Annual Colonial Reports*, No. 1738, p. 6; for Cuba to the population in Department of Overseas Trade, *Economic Conditions in Cuba*, April 1935, p. 18; for Martinique to the population in *Revue d'histoire des colonies*, 1935, p. 146; for Chile to the population in *Estadística Chilena*, 1935, No. 12; for Peru and Colombia to the population in *Boletín de la Oficina Sanitaria Panamericana*, 1935, p. 279, 1936, p. 187). In a few cases, where Loyo does not take account of the small numbers of Negroes or 'Others' (Chile, Colombia, Paraguay), we have slightly revised his percentages. For Canada, United States, Antigua, Grenada, Jamaica, Panama, Panama Canal Zone, Puerto Rico, St. Kitts and Nevis, and





St. Vincent, we have assumed that the proportions of the races have remained the same since the last census.

**Alaska.** Census 1 Oct. 1929 (see *Census of the United States 1930, Outlying Territories and Possessions*, pp. 13, 15): 28,640 Whites, 29,983 Indians (full-blooded and of mixed Indian and other blood), 26 Chinese, 278 Japanese, 136 Negroes, 164 Filipinos, 29 Mexicans, 11 Hawaiians, 11 Koreans. Total population, 30 June 1935: 61,500 (see *Statistical Abstract of the United States 1935*, p. 10).

**Canada.** Census 1 June 1931 (see *Census of Canada 1931*, vol. ii, pp. 294–7): 10,134,313 European races, 46,519 Chinese, 23,342 Japanese, 14,687 Other Asiatic races, 5,979 Eskimoes, 122,911 Indians, 19,456 Negroes, 681 Various, 8,898 Unspecified. Total population, 1 June 1934, see *Canada Year Book 1934–35*, p. 164.

**Greenland.** Census 1 Oct. 1930 (see Danmarks Statistik, *Population du Groënland 1930*, p. 5): 408 Europeans, 16,222 Natives.

**Labrador.** Total population, see *Statesman's Year-Book*, 1936, p. 348. 'Some 1,300 Eskimo, the remainder of British descent' (*The Dominions Office and Colonial Office List 1935*, p. 134).

**Newfoundland.** Total population, see *Statesman's Year-Book*, 1936, p. 348. We have roughly estimated the number of Indians.

**St. Pierre and Miquelon.** Census 1 July 1931 (see *Résultats statistiques du recensement 1931*, vol. i, part i, p. 113): 4,321 Europeans and Assimilated (4,067 French, 254 foreigners).

**United States of America.** Census 1 April 1930 (see *Census of the United States 1930, Population*, vol. ii, p. 25): 108,864,207 Whites, 11,891,143 Negroes, 1,422,533 Mexicans, 332,397 Indians, 74,954 Chinese, 138,834 Japanese, 45,208 Filipinos, 3,130 Hindus, 1,860 Koreans, 660 Hawaiians, 96 Malays, 18 Siamese, 6 Samoans. Total population, 30 June 1935, see *Statistical Abstract of the United States 1935*, p. 10.

**Antigua.** Census 24 April 1921: 914 White, 3,999 Coloured,

# ESTIMATING ABORIGINAL AMERICAN POPULATION

1

# An Appraisal of Techniques with a New Hemispheric Estimate

*by Henry F. Dobyns*

THE IDEA that social scientists hold of the size of the aboriginal population of the Americas directly affects their interpretations of New World civilizations and cultures. Vaillant (1944: 29), for example, wrote that "population pressure" was "often an indirect cause of war in the Old World," but that it "was virtually non-existent in Indian America." Certainly Germany, Italy and Japan went to war a generation ago claiming they were so overpopulated that they required additional territory (Organski and Organski 1961: 4). Vaillant also concluded, on the other hand (p. 112), that "sheer pressure of population was an important cause for the military exploits of Tenochtitlan." Cook (1946: 83), moreover, independently raised the question whether the religion "centered around human sacrifice" of captives, which was the "focal point of the entire Nahua civilization," was not evidence of a "social urge" toward checking population increase. He pointed out that mass immolation of captives seems to have become common just when the central Mexican population reached its maximum density and the margin of subsistence turned "somewhat precarious."

Such relationship between cultural interpretation and human population in a given area is perceived because social scientists view changing population density as a social dynamic. The transition in social organization from homogeneous to segmented structure has been directly attributed to population increase. An increasing population related to increased food supply "is sufficient to account for the appearance," Oberg (1955: 483) claimed, of a social structure with moieties, sibs, and associations. "Numerical change and concentration appear" to Oberg (p. 484) "to be the principal factors affecting this change." A closely related point of view sees the formation of new tribal groups as a function of increasing population. "If the population increased to the neighborhood of 5,000, most tribes—even nationality tribes—seem to have broken apart from sheer weight of numbers" (Kroeber 1955: 304–05).

As a result of this view of population change as a cultural dynamic, social scientists (Naroll 1956: 688–93; Mason 1959: 87ff.; Ember 1963: 229–32) have taken population density to be one index of cultural development. There is, however, little novel, other than numerical expression, in such views. A Swiss artist who worked in trading posts on the Missouri River in the early 1850's remarked upon "a distinct difference in the development of Indian nations, according as more densely populated communities increased the necessities of existence" (Jarrell and Hewitt 1837: 289).

Discussions of the relative power of national states share this view of population as a fundamental independent variable. Population size is "a major determinant of national power" according to Davis (1958: 199) because, first, it is one of the main factors in the size of a nation's labor force; second, mass production and distribution are advantageous proportionately to the scale of the population they serve; and finally, military personnel for fighting or occupation duty must be drawn from total population ever since the invention of the citizen-army. Organski and Organski (1961: 31) predicted no "dramatic shifts" in population rank of nations because the days of mass migration have passed. Yet they

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The present articles were submitted in final revision to CURRENT ANTHROPOLOGY 27 v 63 (Dobyns) and 10 VIII 64 (Thompson). They were sent for CA treatment to 49 scholars, of whom the following responded with written comments: John W. Bennet, Ignacio Bernal, Oldemar Blasi, A. Carmagnani, Sherburne F. Cook, Eusebio Dávalos Hurtado, William M. Denevan, Henry F. Dobyns, Harold E. Driver, Frederick L. Dunn, Malcolm F. Farmer, R. G. Forbis, Helmuth Fuchs, Alexander Häusler, William A. Haviland, Alice B. Kehoe, Thomas F. Kehoe, N. Keyfitz, Peter Kunstadter, T. J. Maxwell, John Paddock, Milan Stloukal, H. Paul Thompson, and Bruce G. Trigger. The comments written for publication are printed in full after the authors' texts and are followed by replies from the authors.

clearly see population as a major component of national power (p. 3). They assert that a "great power" must have at least 45,000,000 inhabitants (p. 13) although they recognize that population is not the only determinant of relative power (p. 4).

This view of population finds its anthropological expression in a view of society which assumes political integration to be a constant. Murdock (1957: 674) has in effect considered gross size of population the only independent variable for distinguishing between types of social order. In a closely related albeit dynamic view, Spicer (1962: 39, 99) attributes differential results of Indian reaction to Hispanic culture to variations in aboriginal population density.

The whole school of recent thought that relates cultural development to the amount of harnessed energy available within a given society directs attention to the importance of the amount of manpower available in that society. Cottrell (1955: 6), for example, views man as one type of 'energy converter" in terms of which all other such converters can be evaluated. He identifies the relationship between food raisers and other members of a society as "the crucial area for analysis" because of the determination of minimum energy requirements by the size of population (p. 117).

The particular concept that social scientists share of aboriginal American population in turn assumes some direct practical importance in contemporary affairs. It becomes broadly significant because the social science concept diffuses to public policy makers (cf. International Labour Organization 1953: 30–31). It influences such men as attorneys involved in the decision-making process of governmental commissions and courts (cf. Campbell and Clark 1962: 23–24). It colors the conclusions of other scientists; e.g., in a medical history of the conquest of the Americas whose thesis is "that disease had profound and specific effects" which were both "decisive and determining," one glimpses this implicit conditioning of conclusions in a statement about the Indians: "In view of the immense territory they held, their total numbers, probably between 2,000,000 and 2,500,000, were astonishingly small" (Ashburn 1947: xvii).

It seems worthwhile, therefore, to examine the validity of the widely held concept of a low density of American population prior to European conquest and colonization. This paper [1] attempts to analyze some major methodological reasons why estimates of aboriginal American population have yielded a picture of small scale preconquest human population in the Western Hemisphere.

## CONSERVATIVE HEMISPHERE WIDE ESTIMATES

Rosenblat (1935; 1945: 92; 1954: 102) arrived at a hemisphere-wide estimate of 13,385,000 Indians for 1492, taking that date to represent preconquest conditions. Rosenblat's work comprises the most important general summary of the subject published in Spanish to date and is cited approvingly by writers in Spanish (cf. Barón 1942: 143–45). Rosenblat implied an average aboriginal population density in the New World (including Greenland) of one person in just over three square kilometers (0.319 per km²). In the English analysis most widely referred to, A. L. Kroeber (1939: 166) estimated 8,400,000 Indians in the Americas in preconquest times, although he admitted that his figures were low. This estimate implied an average aboriginal population density in the Americas (including Greenland) of only one person per five square kilometers (0.2 per km²). Both Kroeber and Rosenblat proceeded implicitly on the general methodological assumption attributed by Wilcox (1931: 44) to James A. Garfield (who attributed it to Samuel Johnson): "To count is a modern practice; the ancient method was to guess; and when numbers are guessed they are always magnified."

The Rosenblat and Kroeber hemispheric population estimates represent the extreme reaction to those made by earlier commentators on New World population beginning with Bartolomé de Las Casas. In 1541 (MacNutt 1909: 314; Philips 1656: 1), Las Casas undertook to estimate, not the total native population of the Americas, but merely Indian mortality within the area then subjected to Spanish control. He calculated that "more than twelve million persons, men, and women, and children, have perished unjustly and through tyranny, by the infernal deeds and tyranny of the Christians" (MacNutt 1909: 317) or in an earlier translation (Philips 1656: 5) "there have innocently perished above Twelve millions of souls, women and children being numbers in this sad and fatall list."

Las Casas asserted, in other words, that more Indians than Kroeber believed existed at the time, and nearly as many as Rosenblat estimated to exist, perished during the 40 years after 1500. Actually Las Casas felt the true 40-year mortality had been 15,000,000 Indians, more than Rosenblat thought existed, and his jurisdiction-by-jurisdiction mortality estimates totaled over 24,000,000 Indians (Rosenblat 1954: 101 n. 11). In 1560, Las Casas raised his estimate of Indian mortality to that date to 40,000,000 (Las Casas 1951: 363).

Prior to the Rosenblat-Willcox-Kroeber impact on social-science images of preconquest Indian population size, several markedly higher estimates of aboriginal New World population had been made which to some extent at least placed Las Casas' estimated Indian mortality within the realm of possibility. Rivet (1924: 661) estimated from 40,000,000 to 45,000,000 and Sapper (1924: 100) from 40,000,000 to 50,000,000, a density of about one person per square kilometer or a bit more. Spinden (1928: 660) placed the aboriginal population apogee at about 1200 A.D. with from 50,000,000 to 75,000,000 individuals. His figures implied an aboriginal peak population density from 1.2 to 1.8 persons per square kilometer. This wide range of estimates moved Steward (1949: 655) to point out that such discrepancies made evident the methodological difficulties inherent in estimating aboriginal populations and proved that methods or data were faulty. This paper attempts to

[1] This analysis originated from research carried out for the Arizona State Museum with funds from the Arizona State Parks Board. Writing was supported by the Cornell Peru Project with funds from a grant to Cornell University by the Carnegie Corporation of New York. Manuscript preparation and revision was aided by the Comparative Studies of Cultural Change under Cornell University Contract AID/csd-296 with the Agency for International Development's Office of Technical Cooperation and Research. Conclusions are the author's and do not reflect policies of any supporting agency.

TABLE 1

INITIAL INDIAN MORTALITY IN MEXICO AND CENTRAL AMERICA, ACCORDING TO FATHER BARTOLOMÉ DE LAS CASAS

| JURISDICTION | ESTIMATED CASUALTIES | PERIOD | CITATIONS |
|---|---|---|---|
| Guatemala | 4,000,000–5,000,000 | 16 years | MacNutt 1909:356<br>Philips 1656:51 |
| New Spain | 4,000,000 | 12 years | MacNutt 1909:342<br>Philips 1656:34 |
| Honduras, etc. | 2,000,000 | 11 years | MacNutt 1909:351<br>Philips 1656:44 |
| Panama | 800,000 | 8 years | MacNutt 1909:335 |
| Nicaragua | 500,000–600,000 | 14 years | MacNutt 1909:341 |
| slaves* | 500,000 | 10 years | MacNutt 1909:340 |
| Total | 11,800,000–12,900,000 | | |

* Sold in Panama and Peru, where they died, according to Las Casas.

point out methodological faults after comparing population estimates made for areas within the hemisphere.

## AREA POPULATION ESTIMATES

*North America.* Mooney essayed a continental aboriginal population estimation for North America by building up totals from tribe-by-tribe estimates. His calculation of total preconquest Indian population north of Mexico including Greenland was 1,153,000 (1928: 33), and it has greatly influenced subsequent estimates. Kroeber (1939: 31) reduced Mooney's total to 1,026,000 by substituting his own calculation of 133,000 Indians in California (1925: 882–83) for Merriam's (1905: 598) figure of 260,000, which Mooney had accepted. Kroeber (1939: 134) believed that the aboriginal population of North America actually fluctuated around 900,000 inhabitants. Rosenblat (1954: 102) more or less accepted Kroeber's (1939: 143) reasoning in estimating 1,000,000 persons in North America around 1492.

In order better to compare the significance of these figures, one may convert them into densities. Mooney's estimate implied an aboriginal density averaging one person in 19 square kilometers (0.0535 per km²). Kroeber's lowest estimate implied one person per 24 square kilometers (0.042 per km²). Rosenblat's estimate implied 1 individual per 22 square kilometers (0.0465 per km²).

*Mexico and Central America.* Kroeber (1939: 166) estimated that 3,300,000 persons lived in the civilized area of Mexico and Central America (considering Costa Rica and Panama as culturally part of South America) in preconquest times. Rosenblat (1954: 102) estimated 5,300,000 individuals inhabited that area, thus topping Kroeber by 2,000,000 even though he was concerned with refuting the "Black Legend" of Spanish responsibility for depopulating Indian America (Rosenblat 1954: 11). The originator of the legend, Las Casas, certainly claimed that many more Indians perished in this area than either Kroeber or Rosenblat estimated existed (see Table 1). Sapper (1924: 100) calculated an ecological potential of 12,000,000 to 15,000,000 inhabitants for Mexico.

*South America.* Kroeber (1939: 166) postulated 4,000,000 aboriginal inhabitants in all South America including Panama and Costa Rica, placing 3,000,000 of these in the Inca Empire. Thus he viewed the Mexican and Andean civilizations as on a par in population. Rosenblat (1954: 102) estimated 6,785,000 for the continent south of Panama, with 3,300,000 in Peru, Bolivia, and Ecuador. This implied an average aboriginal population density of 0.38 person per km², whereas Kroeber's figure implied an average of only 0.22 person per km². Rosenblat (1945: 92) inferred a smaller population in the Andes (4,150,000 in Columbia, Ecuador, Peru, and Bolivia) than in Mexico (4,500,000).

By way of contrast, Las Casas (Philips 1656: 97–98; MacNutt 1909: 384) estimated 4,000,000–5,000,000 Indian casualties during the first 15 years of the Spanish-licensed, German-led occupation of Venezuela, 1526–41. He estimated that 2,000,000 people (Nuix 1782: 13), captured farther east on the coast of South America and shipped to the islands of Puerto Rico and Hispaniola, died in the mines and other works (MacNutt 1909: 380). Las Casas (MacNutt 1909: 401) also accused the Spaniards of killing over 4,000,000 Indians in the Inca Empire within the first decade of conquest there, relying heavily upon the eyewitness account of Friar Marcos de Niza. This amounted to an estimated mortality of 10,000,000 to 11,000,000.

One measure of the recent dominance of the Rosenblat-Willcox-Kroeber school of thought about the density of aboriginal American population is its wide

imitation. Kubler (1946: 339) also favored an estimate for 1531 of only 3,000,000 persons in the Inca Empire outside the provinces of Quito, i.e., modern Ecuador (assuming a 50% population loss in 30 years), and thought the total could have been no more than 6,000,000 (assuming a 75% loss from 1531 to 1561). Rowe (1946: 185) arrived at an estimate of 6,000,000 persons in the preconquest Inca Empire extending from north of modern Ecuador to northern Chile and Argentina. Steward (1949: 663) proceeded from this base to guess a total of 4,700,000 population in the Inca Empire: 2,330,000 in highland Peru, 1,170,000 in highland Bolivia, 500,000 in Ecuador, and 700,000 in highland Colombia outside the Chibcha area. Steward (p. 665) assumed a preconquest population of 9,000,000 in all South America. Bennett (1946: 7) contented himself with stating "it cannot be directly inferred that the number of Indians at the time of the Conquest was the same or greater than the present census figures indicate," and concluded that the Andean population at the time of conquest was likely not under "4,500,000 and not over 7,500,000" (p. 8). Baudin (1961: 24) had, on the other hand, placed the population of the Inca empire "at the time of the conquest" at between 11,000,000 and 12,000,000. Polo (1913: 53) estimated 10,000,000 to 12,000,000 at the time of the 1533 conquest. The latter figures are consistent with Sapper's (1924: 100) estimated ecological potential of 12,000,000 to 15,000,000 inhabitants in the tropical Andes.

The reasoning behind low population estimations ignored contemporary comment by Spanish imperial officials on the historic process of Indian depopulation. In 1685, for example, the Marques de Varina addressed the Spanish king on the subject. In arguing against attributing depopulation solely to mining operations (a tendency among promulgators of the "Black Legend") he pointed to the complete extinction of the Caribbean Island natives and those of valleys of Peru and nearby areas "where there are no mines and more than eight million Indians have perished" (Fernandez 1949: 36). The contrast between the views of modern social scientists and of a colonial authority whose active career spanned a score of years about the time that American Indian population probably began to recover is very marked.

It can be said with justice that the Marques de Varina may have inflated his mortality figures to try to frighten the King of Spain into action. To make such an assumption does not entitle the analyst to discard those figures; it does require him to seek corroborative or negating evidence. Rosenblat, Kroeber and their imitators have not done so. Their characteristic methodology has included depreciation of all historical population figures. They deprecate the departure of historical witnesses from the "truth" for motives they intuitively impute, but which uniformly led said witnesses to overestimate, in their opinion, aboriginal populations. They ignore the fact that eyewitnesses, whatever their biases, at least observed population trends which the modern analyst can never witness. Thus Rosenblat (1945: 185) claimed, "The old estimates are always hyperbolical," without offering sound evidence to support such a claim. As Sauer (1935: 1) pointed out: "Modern students commonly have been inclined to discount early opinions of native numbers, but rarely specified their reasons for doing so." Although Kroeber (1939: 177) characterized Sauer's work as "another of his revolutionizing studies," he remained convinced by his own preconceptions. Although in 1936, under the impact of Sauer's and Meigs's (1935) analyses, he made explicit some of his previously implicit assumptions, Kroeber (1939: 180) remained antidocumentarian:

> The vast majority of figures by contemporaries are too large. This fact will be generally admitted. The problem is to know when the exaggeration is slight and when it is unreasonable. In general, documentarians tend to cling to the more moderate figures given in the records, ethnologists to distrust them generically. Where Sauer shaves sixteenth- and seventeenth-century statements, I am likely to reject most of them outright.

Certainly Kroeber's description of documentarians applied to many historians. Hammond (1927: 122 n. 463), for example, thought New Mexico colonizer Juan de Oñate "probably exaggerated" when he gave 3,000 as the population of Acoma Pueblo in 1598. Oñate and others "exaggerated the numbers in order to convince the king of the importance of the province," according to Hammond. Yet the Spaniards slew either 600 or 800 Acomas and captured 570 or 580 in a three-day battle (Hammond 1927: 120-21) that involved only part of an anti-Spanish faction—as Indians who had been out of the pueblo cultivating their fields during the conflicts later testified at the Spanish trial of Acoma captives held in Santo Domingo Pueblo (p. 122)—and two pacifist leaders had deserted the pueblo with their followers some time before (p. 113).

Steward (1949: 655–56) sought to state explicitly the methodological assumptions he and other anthropological analysts of like persuasion have employed. His brief summary of various estimation methods failed, however, to do more than describe them from his point of view, which falls within the range of methodology of the skeptical group. He did not evaluate the results of the several methods of estimation employed, nor spell out criteria for data evaluation except in applying some procedural propositions of Kroeber's formulation. It seems appropriate, therefore, to review some of the principal estimation methods that have been employed by various investigators and the relation of these methods to data and their accuracy.

## PROJECTION METHODS

Some form of projection of population figures for small, more intensively studied areas to larger, less well-studied areas of the New World is the method by which a number of Americanists have estimated aboriginal population.

*Hemispheric Projections.* Taking one at least approximately known set of data, the contemporary Indian population, Rivet attempted to project back through time to an aboriginal population estimate using a factor derived from one well-studied area. Rivet's (1924: 601) basic population figure was about 15,000,000 living Indians in the Americas. His projection factor was derived from North America, with 403,000 living Indians as compared to an estimated 1,148,000

at discovery (p. 600). Assuming a constant ratio of depopulation in the hemisphere, Rivet estimated a maximum of 40,000,000 to 45,000,000 aboriginal inhabitants, a density of 1 person per square kilometer over the entire area (pp. 601–2). In 1931 Kroeber (1939: 160) asserted that we "may not infer from present-day large populations to native large ones." In support of this methodological assertion, Kroeber posited a straight-line increase of population throughout historic times by projecting backward in time the apparent rate of increase implied by Humboldt's figures for 1793-New Spain and the Mexican census of 1930. "If it could be assumed that the increase could be continuously projected backward, we should be starting with little more than 300,000 souls in 1500 A.D.—and that in all New Spain . . ." So although he cited Rivet's estimates, Kroeber (p. 165) had already rejected his method.

Under the impact of Sauer's analysis of documents, Kroeber (1939: 180) was willing by 1936 to grant some methodological validity to this projection procedure. "Modern population is some index of original native population, at least under Latin-American rural conditions. Sauer uses this principle." Kroeber displayed his continued aversion to historical evidence by immediately shifting the argument to a discussion of the concept of "subsistence potentialities." (As a matter of fact, Sauer had not "used" a principle of equivalence of modern rural and preconquest aboriginal populations; he merely commented [1935: 32] on their apparent coincidence.)

The fact that enumerated "Indian" population is currently increasing implies that it has done so for some time. Kroeber's straight-line projection of this population trend all the way back to preconquest times took cognizance of this late historic trend. It failed to take into account, however, unassailable early historic evidence that Indian population in fact fell from its prehistoric level to an absolute historic minimum, a nadir from which it is still recovering. Kroeber's choice of 1793 population figures for New Spain was undoubtedly dictated in large part by their availability, but that date unfortunately falls very close to Jenner's perfection of vaccination against smallpox. Vaccination has been, in all probability, the major single factor in New World native population recovery since 1800, so any backward projection of post-1800 population trends which assumes that pre-1800 and post-1800 disease environments were not seriously different falls into fatal error.

Kroeber's objections aside, the method employed by Rivet actually was subject to several limitations. The precision of enumeration of living Indians clearly affects the final estimate obtained by such extrapolation. Racial fusion to produce "a new type, the so-called mestizo," is a major population characteristic of Latin America (Cook 1946: 81) and the difference between the Indian population figures reported by the U.S. Bureau of the Census and by the Bureau of Indian Affairs reflects the difficulty of even defining "Indian" as a result of interbreeding. So the projection of historic "Indian" population figures is always subject to the error of reasoning from a racially mixed historic population to a pure Indian prehistoric population.

Of equal methodological importance, population is not the static quantity such projection implicitly assumes. As censuses subsequent to 1924 have shown, enumerated "Indian" population continues to rise, so that the estimate of aboriginal populace obtained in this manner depends entirely upon which census returns are employed. This defect is well illustrated by the difference between Rivet's figure of 15,000,000 contemporary Indians and Spinden's (1928: 660) figure of 25,000,000, published only four years apart.

An additional defect in Rivet's projection procedure was his dependence upon an estimation of prehistoric North American population in order to derive the depopulation ratio he employed. The hemispheric population estimate obtained by this method could be no more exact than the estimate of aboriginal North American population which yielded the factor used to project the aboriginal population of the rest of the hemisphere. Rivet employed Kroeber's North American estimate, whose merits will be examined below. Still, obtaining a ratio from data however faulty represented an attempt at working out an empirical method not even essayed by Spinden, who simply assumed Indian population about 1200 A.D. to have been two to three times greater than contemporary Indian population.

*United States Projections.* On a less ambitious scale, MacLeod attempted to estimate the aboriginal population of the U.S. by projecting estimated aboriginal population densities of studied areas over a known geographic area. Thus as Willcox (1931: 49) remarked of a different analysis, "in starting with an assumed density of population he used a better method" than mere compilation. MacLeod assumed that 2,000,000 square miles of the U.S. were fit for aboriginal Indian habitation. He accepted Kroeber's figure of about 130,000 Indians in California in precontact times, which yielded a density of one person per square mile (MacLeod 1928 : 15), and reasoned that if the rest of the country were as densely settled as California, there were 2,000,000 Indians there in aboriginal times.

MacLeod checked his estimate by computing aboriginal population density in tidewater Virginia from early English descriptions. Multiplying warrior totals by a factor of three, which he considered conservative since it implied a population barely reproducing itself, MacLeod (pp. 545–46) found a density of two Indians per square mile in the area most familiar to English colonists. Willcox (1931: 53) accepted estimation of the population of India from army size, but surprisingly preferred (p. 55) Mooney's lower figure to MacLeod's higher one for Virginia, on grounds that Mooney presented "fuller evidence." Mooney (1928: 6) presented a whole page of tabular population figures for the South Atlantic states, to be sure, including all of six unsupported lines on Virginia Indians. MacLeod (1928: 545) meticulously marshalled his sources and explicitly stated his hypotheses, so his conclusions would seem much to be preferred on procedural grounds. His estimated tidewater Virginia population density suggested to MacLeod (1928: 16) that the national estimate based on Kroeber's California figure was low, taking into consideration the fertility of the St. Lawrence

River Valley, the Saskatchewan lakes and the north Pacific Coast fisheries. So he estimated "a pre-European Indian population of roughly three million" in the United States, not the 4,000,000 a literal interpretation of his own figures implied.

MacLeod implicitly assumed the Virginia Indian population observed by Englishmen to have been aboriginal in density, an assumption that may be questioned in view of the early prevalence of epidemic diseases. Then he assumed that the density of Indian population in Virginia was appreciably greater than the average over the U.S., which agreed with Kroeber's (1939: 166–71) theory of greater population density along salt-water shore lines.

Later investigation has indicated that MacLeod erred on the side of caution. Aschman (1959: 178, Table 7) recently estimated that aboriginal population density averaged 1.12 persons per square mile in the central desert of Lower California. This is one of the world's areas least hospitable to human habitaton (Aschman 1959: 5–27). Such a density of aboriginal populace in such an adverse environment certainly suggests that the Upper Californian density should have been considerably greater than Kroeber thought, if population pressure were equally adjusted to recources available in the two Californias. This implies that Kroeber's estimate of one Indian per square mile is low, and that MacLeod was overly conservative in not applying his tidewater Virginia population density of two Indians per square mile nationwide instead of compromising on 1.5 persons per square mile, a figure halfway between his and Kroeber's density estimations for the opposite shores of the continent.

Cook employed a similar projection method for estimating aboriginal population of Lower California. He (1937: 7) accepted Meigs's calculations of an average of 1.15 persons per square mile in the northern part of the peninsula, raising the figure slightly to $1.3 \pm 0.1$ to allow for slightly greater density. Projecting this average density over 29,100 square miles of the peninsula provided Cook (p. 8) with an estimate of upwards of 38,000 persons in aboriginal times. This implied a total peninsular aboriginal population on the order of 48,000 individuals.

In view of Aschman's more recent investigations, that figure should be regarded as a minimal estimate. If Aschman's conservative central desert figure, 1.12 people per square mile, is projected over the 42,200 square miles of all Lower California (Cook 1937: 7), one finds that the area held at least 47,264 aboriginals, even if the more favorable northern and southern environments were no more densely populated than the central desert. Since they undoubtedly were more heavily populated, the aboriginal peninsular population must have exceeded 50,000 by a wide margin.

*California Projections.* It is clear from the Rivet and MacLeod uses of the projection method that the aboriginal population of North America has become a key figure in hemispheric estimation, and that the aboriginal population of California occupies a key position within that larger total. The fact that Kroeber's estimate of California aboriginal population was obtained by revising downward Merriam's earlier figure, which yielded a density estimate equal to Aschman's for the central desert of Lower California, calls for a re-examination of Kroeber's and Merriam's methods.

Merriam anticipated MacLeod in projecting the population density of a small, documented area over a larger geographic region. His base line in time was 1834, when upward of 30,000 converted Indians were reported in California (Merriam 1905: 597). This datum Merriam projected outward through space and backward through time. He inferred that there was one unconverted Indian in the area the missions tapped for every three converts (p. 598), raising the 1834 population for the immediate missionized area to 40,000. He took depopulation into account by allowing for a loss of 10,000 due to Spanish contact, bringing his estimate of precontact population to 50,000. Then (p. 595) he calculated the area tapped by Spanish missions to have been $1/5$ of the non-desert part of the state, inferred an equal aboriginal population density, and estimated 250,000 Indians. Adding 10,000 persons in the desert areas, he arrived at 260,000 Indians in aboriginal California (p. 598).

Kroeber (1925: 880) considered some of Merriam's assumptions quite conservative. He thought that Merriam's proportion of 1834 to 1769 population was too low "in view of the enormous mortality at some of the missions" (pp. 880–81). He questioned Merriam's base line population figure, preferring "the exact figure of 24,634 for 1830" (p. 881). He also defined the missionized area as nearly $1/3$ of the state instead of $1/5$.

Starting from an 1830 population rounded off at 25,000 mission Indians, and retaining the proportion of $1/4$ this number of unconverted natives, Kroeber calculated a population of 33,000 for the mission area. He allowed $1/2$ that figure for decrease since 1769, estimating 50,000 in the missionized area as of 1770. Projecting this figure across the state yielded him a total estimate of 150,000 individuals. Using a different method (1925: 882–84), he arrived at a total of only 133,000. Kroeber (1939: 143, Table 9) calculated a population density averaging 0.433 person per square kilometer during aboriginal times in California. This density varied from 0–5 persons per square kilometer in the desert areas to over 70 individuals per square kilometer in favored parts of the Central Valley and Pacific Coast north of San Francisco Bay (p. 154: Map 19).

*Yucatan Projections.* The population density estimates for late aboriginal times arrived at by Kroeber, MacLeod, Cook, Meigs, and Aschman for different parts of North America make appropriate a discussion now of a density estimate of a very different order for the peak population of the Old Maya period in Yucatan. Ricketson (1937: 16) arrived at an estimate of 270.8 persons per square mile by applying a series of explicit assumptions to hard-won archaeological survey data. Ricketson and his associates counted prehistoric house-mounds within a defined surface sample area at Uaxactun, then multiplied the number discovered by a factor of five persons per house-mound. They derived this factor from contemporary studies of household size in Yucatecan settlements. Ricketson arrived at his 270.8 persons per square mile estimate by explicitly assuming that only $1/4$ of the house-mounds encountered were occupied simultaneously. Assuming that all recorded house-mounds had been occupied

at the same time would have implied a peak population density of 1,083.3 persons per square mile of habitable land—a clearly illogical situation.

While the problem to which Ricketson addressed himself in projecting the estimate of population density at Uaxactun to the Yucatan Peninsula was the scale of population in Old Mayan times considerably prior to the epoch of peak hemispheric aboriginal population, his methodology merits mention. Having found only about ½ the area surveyed at Uaxactun to be habitable and to bear evidence of prehistoric habitation, and having found some corroboration for this local ratio in trail characteristics elsewhere, Ricketson explicitly assumed that only ½ of the surface area of Yucatan had been habitable. Employing his lower estimate of density, he arrived at an approximation of 13,300,000 individuals as the peak Old Mayan population.

In his synthesis of Mayan archaeohistory, Morley (1947: 316) followed Ricketson, repeating his "estimates ranging from a minimum of 13,300,000 to a maximum of 53,300,000." The latter figure assumed simultaneous habitation of all house-mounds and was clearly unrealistic. In revising Morley's text, Brainerd (1956: 262) explicitly reduced the proportion of house-mounds occupied at any one time to one in every eight recorded, thus arriving at an estimated density of 136 persons per square mile. He felt that this figure was still higher than reality, arguing that the "normal Maya homestead" includes two houses or more. Brainerd implied, then, a density of from 64 down to 45 individuals per square mile, or a total Yucatecan population on the order of 2,216,666–3,325,000. Elsewhere Brainerd (1956: 47) claimed that there is no evidence for a heavy lowland Mayan population at any time, and suggested the 30 persons per square mile modern Yucatecan density as "a likely maximum."

This guess by Brainerd clearly conflicts with the carefully worked out estimates based on actual archaeological survey data. Yet, whether the "true" figure was 30, 45, or 64 persons per square mile, the Mayan data from the lowland rain forest area of the Yucatan Peninsula indicate one of the peak densities of New World populations. These densities contrast starkly with Kroeber's (1939: 159) average density of only three persons per square kilometer in Mexico and Central America. They suggest that Kroeber was indeed correct when he wrote (p. 160) that: "The actual population in 1500 A.D. may have been more" than his 3,000,000 estimate.

## "DEAD RECKONING" METHOD

In undertaking a systematic examination of the question of the size of aboriginal American population north of Mexico, Mooney (1928: 1) planned to divide the continent into a number of "natural sections," and then to "discuss the population of each in turn, first generally and then tribally." This plan produced tabular listings of tribal populations added together into regional and continental totals, in what Kroeber (1939: 131) labeled the "deadreckoning" method.

When the latter reconsidered the question of aboriginal Californian population, he (1925: 882) estimated each group separately by this method, using:

> early estimates of travelers and settlers; the conclusions of ethnologists familiar with the people at a later time; the number of known villages or village sites; the tribal count in the Federal census of 1910 . . . apparent rapidity of decrease in various areas; the availability of food supply in each habitat, and indications of the ratio of density of population in adjacent areas of differing surface environment.

Using this procedure, he arrived (p. 883) at an estimate of 133,000 Indians living in California in 1770.

Steward (1949: 656) has summarized one of Kroeber's principal working assumptions in these words: "most contemporary estimates, particularly by the early Spanish missionaries and administrators, were too high." He then went on to concur: "Kroeber's suspicion of such estimates is certainly justifiable in the case of soldiers, who obviously exaggerated the number of their enemies. Missionary guesses of independent tribes were also liable to exaggeration" (p. 657). Steward meant, presumably, that Spanish soldiers magnified the number of their enemies through fear, desire for reinforcements, or hopes for glory, and missionaries through desires for increased financial support for conversion activities.

Kroeber's second assumption was that "a competent ethnologist may correct such estimates for an area he knows well" (Steward 1949: 656). Admitting that the assumption might not be true, Kroeber (1939: 180) claimed "it is evidently made by American anthropologists who have concerned themselves with the subject." One of the inherent pitfalls in such an assumption is that the ethnologist attempting a regional or continental synthesis cannot possibly judge data on other groups as accurately as those for natives among whom he has personally carried out investigations. Nicholson (1962: 880) recently called attention to the failing of "viewing the high cultures of Mesoamerica through North American ethnography-tinted glasses" as contributing to "Kroeber's gross underestimate of the contact population of Mesoamerica."

The probable accuracy of corrections in an area he knows well by an ethnologist of Kroeber's unquestionable field experience may be determined by a recheck of his estimate of the number of California Indians in immediate pre-Spanish times, clearly the key case in current estimations of aboriginal American population. Such a recheck has been made and will be discussed below in considering its methodology.

There remain yet other considerations with regard to the "dead reckoning" method. One shortcoming in its previous application to population estimates is the matter of consistency of societal scale. Kroeber (1939: 131) claimed merit for his California population estimate because it followed Mooney's society-by-society reckoning technique. Navigation by dead reckoning involves, however, constant placement of one object in spatial relationship to others. So Kroeber's choice of methodological labels may be questioned, since both he and Mooney ignored available population landmarks; e.g., they apparently did not cross-check their estimates of the population of inimical tribes to discover whether or not they were

logically consistent. The consequences of failing to compare population estimates for one group with those for its enemies may be pointed out briefly using the League of the Iroquois as an example.

A very interesting picture of the efficiency of Five Nations warriors can be built up by some real dead reckoning. Mooney (1928: 4) estimated and Kroeber (1939: 140) accepted a population of 5,500 for the Confederacy, although Kroeber (p. 133) did admit that "the Iroquois proper are put disproportionately low, perhaps under the influence of Hewitt, who seems to have been impressed by the humble beginnings of the great confederacy." Adding up their estimated populations of tribes at war with the Five Nations, one finds the Conestogas estimated at 5,000; Eries, 4,000; Neutral, 10,000; Huron and Tionontati, 18,000; Abnaki, 3,800; Mahican, 3,000; Delaware, 8,000; Western Shawnee, 2,000; Catawba and related tribes, 17,500; Algonkin and Ottawa, 7,300; Montagnais, including Naskapi and others, 5,000; and Cherokees, 22,000 (Kroeber 1939: 141)—a total of 106,100. If one then adds up the populations of tribes which one or another of the Five Nations had reduced to subject, tribute-paying status, one finds the Montauk estimated at 6,000; Massachusetts, 13,600; Pennacook, 2,000; Wappinger, 5,600; Nanticoke, 2,000; and Conoy, 2,700 (Kroeber 1939: 140)—a total of another 31,900 individuals. This adds up to 138,000 Indians the Five Nations had to control or fight. To be sure, the comparative efficiency of Five Nations warriors cannot be gainsaid. Father Jean Pierron said that a Mohawk victory over the Mohicans in 1669 "was more glorious than profitable because they [Mohawks] are very few in numbers, compared with their enemies, who can bring against them fifty men to their one" (Thwaites 1899: 155). Even so, either 50 or 25 to one seems rather unlikely odds in Indian geopolitics and throws some doubt upon Kroeber's claim that he and Mooney employed dead reckoning. The Mooney-Kroeber figure of 5,500 for the Five Nations is clearly a low estimate. Perhaps 10 times that number would be a more reasonable approximation. Since the tribal units formulated by Kroeber represent a considerable range of historic time, and do not necessarily reflect aboriginal times at all, this little exercise in dead reckoning is offered merely to indicate some consequences of neglecting to carry out this kind of cross-checking.

Another shortcoming of the "dead reckoning" procedure as hitherto employed has been lack of rigorous definition of "aboriginal conditions." This has resulted in presentation of historic Indian populations as "aboriginal" which were in fact surviving remnants long after intensive contact with Europeans had begun. Thus Mooney (1928: 4, 6) took the date 1600 as "aboriginal times" in the U.S. North and South Atlantic States, and in eastern Canada (p. 24). He took 1650 for the Gulf and Central states (pp. 8–9, 11), and 1670 for Central Canada (p. 26). Then he retreated to 1690 for the Southern (p. 13) but 1780 for the Northern Plains and Northwest Coast (pp. 15–18) including British Columbia (Mooney 1928: 27-30). To Mooney aboriginal times meant 1740 for Alaskan natives (pp. 31–32) but 1769 for Californians (p. 19). He accepted a date of 1845 for the central mountain region (p. 22), but 1680 for the tribes of New Mexico and Arizona. He dated aboriginal times among Greenland Eskimos at 1721 (p. 23).

It may be argued that differential dating of "aboriginal" population is in fact necessary because the biological and social environment of American natives shifted from conditions favoring population growth to deleterious ones at different times. Even so, the particular dates taken by Mooney to represent aboriginal population conditions are in many cases much too late. A case in point is that of the Northern Plains Indians. In his analysis of Indian population trends there, Wissler (1936: 36) found that: "The history of the areas studied reveals contact direct, and through intermediaries, as early as 1670;" Mooney's figures, from 1780, are 110 years later.

Another pertinent example is that of the Pueblo Indians of northern New Spain. Mooney (1928: 22) estimated their population as "aboriginal" in 1680. Yet that was the very year those Indians rose in revolt against their Spanish overlords and drove the latter south out of the present area of Arizona and New Mexico. The Pueblo Revolt of 1680 was quite clearly a native reaction against directed cultural change. It reacted against Spanish conquest and colonization of Pueblo lands that began in 1598 (Hammond 1927: 94–104, 112–23) and by 1601 had reduced the Indians, upon whose food production the Spaniards subsisted, to "eating branches of trees, earth, charcoal and ashes" so that many starved to death (p. 145). Spanish colonization of the Pueblo area created a regularly-traveled trade route to central Mexico along which disease agents undoubtedly moved with people and goods, so that aboriginal biological conditions must have ended among the northern Puebloan peoples soon after 1598 if not before.

A comparison of Mooney's 1680 population estimates with some others for the same population shows that his "aboriginal" Pueblo estimates were much lower than reality and much later than pre-European times. Nearly half a century before the Pueblo Revolt, a Franciscan administrator had estimated a Pueblo population of over 58,500 (Hodge, Hammond, and Rey 1945: 62, 64–65, 67–69, 71–73, 75) without counting two provinces. Earlier Spanish accounts generally suggest even larger population figures. An early U.S. trader in New Mexico, Josiah Gregg (1844: 270; Thwaites 1905: 56–57; Moorhead 1954: 188) formed an opinion of Pueblo population trends consistent with Spanish documents when he guessed that the Pueblo populace which he estimated at 9,000–10,000, had been 10 times as numerous at the time of conquest.

By way of contrast, Willcox (1931: 55) implicitly approved the aboriginal population cutting tendency by citing Kidder as having "lowered Mooney's estimate of the original pueblo population of Arizona and New Mexico more than two-fifths." As a matter of fact, Kidder (1924: 39) published his guess, unsupported by direct documentation, four years before Mooney's post-humous work. Kidder's "Spanish conquest" could mean 1540, 1598, or 1692, so his time referent was no more specific than Gregg's. He cut 5,000 off Bandelier's (1890: 121–36) estimation of Pueblo population at about 25,000 at the time of conquest. Spicer (1962: 14) recently concluded that the Puebloans numbered no more than 40,000 about 1600 A.D.

Other investigators have utilized documentary sources in order to estimate aboriginal population size in quite different ways. These investigators may readily be distinguished from the "skeptical" group by their empirical approach to documentation and their relatively imaginative procedures for determining the reliability of historic documents rather than assuming these to be systematically erroneous on the side of exaggeration of population.

These investigators have long since met one of the two conditions Kroeber (1939: 131) set forth for revising his figures. "I shall cheerfully admit a larger population for native America," he concluded, when there should be "convincing studies of specific districts in Latin America which, with maintenance of a reasonable balance within the whole of Latin America, compel a total there more than seven or eight times as great as Mooney's." Cook, Simpson, and Borah have demonstrated fairly conclusively from documentary analysis an aboriginal central Mexican population alone more than 10 times Mooney's estimate for North America. Rosenblat has displayed none of Kroeber's openness to proof. The rigidity of his thinking appears in the consistency of his hemispheric population estimate in two revisions of his original work despite new evidence and interpretations. Rosenblat's bibliography expands, but his estimate remains static.

### Cross-Checking Source Consistency

A generally accepted method of checking the reliability if not the validity of cultural data collected by interviewing informants is cross-checking what one says about a phenomenon against what another says about it. This is perhaps the simplest empirical method of source evaluation, firmly embedded in Anglo-American court procedures and Civil Service Commission background checks as well as ethnological field investigation. It may be applied equally well to statements preserved in historical documents and contemporary respondents' statements.

Cook (1956) has provided a model of cross-checking method by directly comparing data collected from different informants by different ethnographers. Commenting upon one ethnographer's criticism of a local county history, Cook (1956: 111) implied criteria for evaluating the accuracy of sources: "If we are going to discredit the testimony of the chief concerning his own village thirty years previously, we had better throw out along with it the information secured from septuagenarians who have to recount at second hand what their forefathers told them."

Cook consistently estimated greater populations for north coast California Indians than had Kroeber. Cook (1956: 84) found that Kroeber's decision that there were two Yurok houses for every three housesites was in error, and by comparing village and house counts compiled by Kroeber (1925), Waterman (1920), and Merriam, (1905) estimated 3,100 Yurok where Kroeber set 2,500 as absolute maximum. Comparing Kroeber (1925: 116), Loud (1918) and Merriam, Cook (1956: 93–97) estimated 3,300 Wiyot in contrast to Kroeber's 1,000. Cook (pp. 99–100) doubled Kroeber's (1925: 130) estimate of aboriginal Hupa population by comparing his facts with those presented by Goddard (1903), Merriam, and an 1852 map. Cook (p. 101) more than quintupled Kroeber's estimated Tolowa population as of 1850, utilizing data from Waterman (1925) and Drucker (1937); he doubled (p. 120) Kroeber's (1925: 275) Coast Miwok population estimate by comparing his information with that furnished by Barrett (1908) and Catholic mission registers.

It is not necessary to summarize all Cook's reestimates to point out that he ably and amply demonstrated by cross-checking what different informants remembered and told ethnologists about different houses and villages. The implications for demographic methodology are clear. Cook concluded (p. 81) that information obtained from native respondents possesses only limited demographic utility. He emphasized the psychology of human memory, which retains qualitative facts such as village names and locations better than quantitative facts such as the number of inhabitants in a settlement or the number of villages. "This failure to retain and transmit accurate knowledge of number or mensuration becomes intensified," he commented (pp. 81–82), "if the informant is required to reach across an intervening period of unrest and confusion, both physical and mental, to an era of stability long since vanished." Another factor making for variation between informants is that a given individual cannot be familiar with the entire content of his culture.

The methodological assumptions made by "skeptical'" analysts excused them from engaging in such a laboriously scientific method as systematic cross-checking of sources. Kroeber's sweeping imputation of early estimates by missionaries, administrators, and soldiers, sanctioned by Steward, precluded even cross-checking statements of one type of European against those of another.

Estimates of aboriginal American population recorded by literate Europeans on the scene during historic times can in fact generally be cross-checked against other similar estimates, or estimates derived from other types of documents by the analyst.

*Northwestern Mexico.* Sauer (1935: 2) employed the latter type of cross-checking technique for arriving at estimations of aboriginal population in the northwestern part of modern Mexico. Since the documents available seldom recorded total populations, he converted "warriors, families, baptisms or other items" into approximations of total populations. In converting number of families into total population, Sauer used a factor of six, one person less than the average contemporary family size in the same Mexican region. In converting baptisms into total population in completely Christianized areas, Sauer explicitly assumed a birth rate of 40 per thousand, following contemporary Pueblo and past rural Scandinavian rates. Missionary records also frequently yield figures on baptisms performed on small children of the "age of innocence" when parents proffered their offspring for the rite. Sauer (1935: 3) found this age span to have been "four years of age and younger" and determined that contemporary age distribution placed 1/10 of the total population in that age range. He also assumed explicitly that children

under doctrinal instruction were aged 5 to 12 and constituted about 1/5 of the total population. Inasmuch as historical data were available for "small and discontinuous areas" Sauer necessarily also indulged in many interpolations, aware of the hazards in such a projective procedure. As a result, he arrived at a series of population estimates "in apparent disagreement with the opinion of American anthropologists"—particularly Mooney and Kroeber.

Sauer's (1935: 5, Table 1) results are worth comparing directly with Kroeber's in order to indicate the order of difference obtained by their different methods. Sauer calculated a range in aboriginal population density from 10 persons per km² among the southern Totorame to 0.2 persons per km² on the Gulf of California coast of Sonora. Kroeber (1939: 136, Table 7; 158, Table 11) calculated a density of 0.28 persons per km² for the entire "Southwest" cultural area in Mexico. Thus, Kroeber's over-all estimated density was only slightly greater than the density that Sauer calculated for the most sparsely settled tribe in the area.

Such a difference in estimates of population density reflects, of course, very different estimates of the total population in the region. Sauer estimated a total of 540,000 individuals, at 2.6 persons per km², or nearly 10 times that computed by Kroeber from his estimated total of only 100,000.

Direct tribe-by-tribe comparisons between the Sauer and Kroeber estimates are possible only for the Northern Pimans. Sauer (1935: 5, Table I) estimated 30,000 individuals, an aboriginal density of 0.3 persons per km². Kroeber (1939: 136, Table 7) estimated 6,600 Papagos with a density of 0.0924 persons per km² and 4,000 Gila Pimas with 0.266 persons per km², a total of 10,600. The main difference between Kroeber's and Sauer's figures is one of comprehensiveness, Sauer having estimated the entire Northern Piman population and Kroeber only two parts of it. Sauer (1935: 30–32) calculated the following numbers for the Northern Piman subgroups: Sonoran River headwaters 1,000; Himeri 4,000; Altar River drainage 2,000; Desert south of the Altar 4,000; Tumacacori area 1,000; San Pedro River Valley 2,500; Middle Santa Cruz 4,500; Gila River 1,000—a total of 20,000 riverine Northern Pimans plus a total of perhaps 10,000 Papagos.

Surprisingly, Sauer estimated only 1,000 Gila River Pimas where Kroeber estimated 4,000. For this subgroup a later independent estimate is available. Analyzing documents from roughly the same period as Sauer (including the same sources used by Sauer but additional ones as well), Ezell (1961: 17) concluded that "even a figure of 3,000 would be too low an estimate for the Gila Pima population of pre-Columbian times." Ezell reasoned that the aboriginal population was greater than the 3,000 reported because Spanish travelers who recorded village populations failed to visit all extant settlements and also did not see possibly some or even most of the women and children in settlements they did reach, and because epidemic disease had taken some toll prior to 1700, when the documentary record began. Neither Sauer nor Ezell nor I could go back in time earlier than extant documentation in this area, which lagged behind the spread of Old World disease agents which almost certainly reduced the aboriginal populace even before literate explorers arrived on the scene.

Combining Sauer's "Middle Santa Cruz" and his "Tumacacori area" yields a total of 5,500 Indians. I have estimated 3,100 persons in the lower valley in 1700 (Dobyns 1962: 27) and 2,400 in the middle valley then (1963*a*: 181), recognizing that the 1700 population was no longer aboriginal nor full strength.

Thus such independent studies as have been carried out suggest that Sauer's estimate of Northern Piman population was conservative even for the period of initial contact with Spaniards (disregarding the isolated early explorations of Vazquez de Coronado and the friar Marcos), and that Northern Pimans had earlier been more numerous. As Kroeber (1939: 178) remarked of Sauer's study, "if he is right all our figures for the American Southwest must be far too low." Sauer's entire estimation procedure for northwestern Mexico may be taken to have yielded conservative figures. If that be the case, then Kroeber's (1939: 159) forecast that "it may in the end be proved that Mooney and I have throughout cut figures too low" has been borne out.

*Lower California.* Cook (1937: 7) subjected a colonial estimate by Baegert of 40,000 to 50,000 persons in Lower California in pre-Spanish times to the same sort of cross-checking. Baegert was a Catholic missionary who had worked on the peninsula half a century after settlement began (Cook 1937: 2), so his estimate was necessarily itself a backward projection of first-hand data obtained by direct observation under rapidly altering conditions. The Mexican historian Lemoine (1959: 251) followed Clavigero in accepting Baegert's estimate but apportioning to the Pericus ¼ of the total. He asserted, however (p. 250), that one cannot state precisely the number of inhabitants of the peninsula in the years immediately prior to the arrival of the Spaniards.

In attempting to cross-check Baegert's estimate against other types of sources, Cook (1937: 14) utilized three other classes of data: (1) projection of population densities worked out for restricted areas from historic documents, (2) projection of smaller population components extrapolated from baptismal records, and (3) projection back through time of maximum enumerated population, using a constant factor.

Cook borrowed (p. 7) the population density figure of 1.15 persons per square mile that Meigs (1935) had worked out for northern Lower California, increasing it to 1.3 ± 0.1 persons per square mile to allow for a "slightly denser population in the extreme south" and a greater ratio of coast line to total land area south of the region Meigs analyzed. Substracting Meig's area from the peninsular total, Cook (p. 8) then projected his density figure over the remaining area to obtain an estimate of 33,000 persons. Making the same areal reduction in Baegert's estimate reduces it to 35,000.

In working out a procedure for deriving estimates of aboriginal Indian population "in fully Christianized missions," Cook (p. 6) used a constant factor to convert number of children baptized into a total population approximation. Sauer (1935: 3) had employed the factor 10 in northwestern Mexico, where missionaries baptized children aged four and under very soon after contacting a pagan population. He as-

sumed, in other words, that children aged four and under constituted 10% of the total population. Cook (1937: 9) followed Sauer's reasoning, but because missionaries in Lower California tended to baptize children up to age 10, he explicitly assumed that children up to that age constituted 1/5 of the total native population.

Analyzing reports of numbers of children under 10 baptized at several missions whose area of native occupancy he calculated, Cook (1937: 8–12) then projected his estimated total population to the remaining area for which he had no baptismal records available. Thus he calculated (p. 12) 45,100 ± 9,000 native inhabitants in what later became Jesuit mission country in Lower California. This would have meant a population density of 1.56 ± 0.31 persons per square mile.

In order to obtain yet another cross-check on Baegert's historical estimate, Cook employed an additive or standard factor increase method originated by Meigs to derive an estimate of aboriginal population from still another set of historical data. This method employed the maximum reported enumerated population in Christian missions as the base datum, and explicitly assumed that the aboriginal population was in every case 2½ times as large as the maximum number of people at any time following conversion. This procedure provided Cook (p. 13) with an estimate of 48,000 aboriginal Lower Californians in the Jesuit mission area. This method obscured the actual scale of depopulation between aboriginal and Christian times by assuming that it was a constant and uniform ratio of population loss over the entire area.

Finally Cook (1937: 14) averaged the four estimates of aboriginal Lower California population derived from different sets of data, finding that the extreme values deviated by 15% from 41,500 individuals. Inasmuch as Cook relied entirely on documentary sources, his estimate must be taken as conservative, since his data could not escape the temporal limitations of conversion-period documentation, as in the Northern Piman case.

*Central Mexico.* The consequences of the "skeptical" and ethnohistorical methods may be directly compared in the case of estimates of the aboriginal population of the central Mexican civilization. Rosenblat (1954: 97 n. 1) followed Humboldt in deprecating Franciscan estimates of the number of Indians baptized immediately after the conquest on the grounds that these were inflated through a desire to aggrandize the evangelical labor of the order. "All parties were equally interested in exaggerating the flourishing state of the recently discovered nations." In other words, Humboldt imputed the reliability of priests' baptismal figures by charging them with ambition: "the Franciscan fathers vainglorified themselves for having baptized more than 6,000,000 Indians from 1524 to 1540."

Cook and Simpson (1948: 19), on the other hand, rejected the idea that priests could be uniformly charged with willful desire to magnify their exploits or with slipshod bookkeeping. From clerical sources, then, they (1948: 20–22) calculated a preconquest population of 9,030,000 in Central Mexico, by projecting the Franciscan figures back in time to 1519 and outward in space to the civilized area. Their projection only slightly exceeds the figure that one of the 12 pioneer Franciscan friars in New Spain, Toribio de Benavente, or Motolinia, opined had been baptized from 1521 to 1536—more than 9,000,000 Indians (Steck 1951: 183).

Addressing the Spanish King in 1609, Pedro Fernandez de Quiros (Torres 1866: 507) asserted the initial population of the New World was approximately 30,000,000. He used as his base figure a total of 16,000,000 Indian baptisms he said he saw recorded in a Franciscan convent at Xochimilco (p. 508). Quiros reasoned that the Indians baptized by priests of other orders, those not baptized, and the population of the Caribbean Islands would raise the total Indian population to 60,000,000. Although Rosenblat (1954: 97 n. 1) cited the Fernandez figure as inflated, he did not criticize Fernandez's methodology, which was in fact defective. Fernandez did not specify the time period during which the Franciscans baptized 16,000,000 Indians, so his base cipher is highly suspect.

There is, on the other hand, internal evidence in Motolinia's own account which suggests that he, and probably other priests, underestimated the number of baptisms actually performed during the first 15 years of mass conversion following Spanish conquest in New Spain. Motolina recounted that he and another priest on one occasion baptized 14,200 Indians "by actual count" in five days (Steck 1951: 190), a rate of 1,420 baptisms per priest per day. Another priest has been credited with baptizing 14,000 Indians during a single day, and on other occasions 10,000 and 8,000 (Rosenblat 1954: 97 n. 1), so Motolinia's account of his own activities rings true. Called to a town near Tlaxcala on one occasion, Motolinia baptized 1,500 individuals in one day besides preaching, confessing, burying, and marrying (Steck 1951: 203). Since Motolinia presumably wrote of memorable events, it is not likely that a typical day's baptisms approached 1,400 or 1,500 Indians. Assuming an average day's baptisms numbered 700 per priest during this period of mass conversion, and assuming that each priest baptized in this manner only once per week, then each would have baptized at the rate of 36,400 persons annually. Motolinia reported 60 Franciscans in New Spain in 1536, 20 of them not yet baptizing for lack of knowledge of local languages, more than balanced by 20 pioneers who had died after baptizing "more than one hundred thousand" in three instances, and by 20 who had returned to Spain (Steck 1951: 181–82). Sixty priests would have baptized 2,184,000 Indians annually, granted the assumptions previously stated. When it is remembered that Dominican, Augustinian, and secular priests augmented the Franciscans (Steck 1951: 182 n. 5), it may be seen that Motolinia himself very possibly failed to appreciate the total magnitude of postconquest mass conversion in New Spain, even though he credited himself with baptizing 400,000 Indians (Clavigero 1781: 282 n. t; Rosenblat 1954: 97 n. 1).

Cook and Simpson did not stop with their Franciscan baptism-based population estimate, nor with consulting only one class of documentary source. They went on to compare clerical estimates with other types. They considered the two primary chroniclers of military conquest, Hernan Cortez and Bernal Diaz, as "competent and responsible," expressing doubt

that they had motives for exaggerating army size estimates, and pointing out that their estimates accorded "very closely in size" with those of native annalists describing preconquest aboriginal campaigns (Cook and Simpson 1948: 23). Willcox (1931: 53), who doubted aboriginal American population was very great, nonetheless accepted estimates of the early 17th-century population of India drawn from reported army size plus extent and type of agriculture. Rosenblat (1954: 97–98) accused Cortez of overstating the size of native armies he fought in order to emphasize the valor of his troops and his own merits as commander, but offered no evidence to support his accusation.

A method of estimating Indian population trends that is feasible when documents from two or more time periods report appropriate information (even when it cannot be directly compared to other types of sources as in central Mexico) is projection from army size to total population. A series of records of military expedition sizes is particularly useful for this purpose. MacLeod (1928: 44) employed this method to estimate the extent of decimation of the Massachusetts tribe during the epidemic of 1616–19 at about 90%. He simply extrapolated from the pre-epidemic estimate of 3,000 warriors to the post-epidemic estimate of 300.

In order to convert army size into total population estimates, Cook and Simpson (1948: 26) computed a factor by finding the proportion of adult males in the Mexican population enumerated in 1930, which was about 1/5. They applied this factor to army size estimates for Tlaxcala, Mexico-Tenochtitlan, Texcoco, Cholula, Huajotzingo, and Chalco plus Michoacan, and projected this to central Mexico, estimating 8,950,000 total population based upon military figures (p. 30). In both these estimation attempts, Cook and Simpson, in contrast to Rosenblat and Kroeber, relied upon eyewitness accounts, a generally preferred procedure among historians including Clavigero (1781: 281) who also relied heavily upon the "testimonie oculare" of Bernal Diaz and Cortez.

Finally, Cook and Simpson (1948: 3–9) compared their estimates derived from two contemporary types of sources with a third estimate obtained by projecting back in time their determination of the 1565 population of central Mexico, a determination made from administrative reports of a wide range of types. Kubler (1942: 615–16, Table 1) anticipated their procedure, but did not attempt to calculate aboriginal population. He felt "this method is not suitable for determining total populations" (p. 607) so failed to follow up his methodological advance. Cook and Simpson (1948: 30) obtained a projection factor by establishing "for a reasonable sample of towns or provinces the ratio of population in 1519 to that in 1565." The ratio they worked out indicated that the 1565 population had been about 40% of that in 1519. Applied to their 4,409,180 figure for the central Mexican populace in 1565, this factor yielded an estimate of 11,002,450 persons living in the preconquest year 1519 (Cook and Simpson 1948: 38). They accepted the method of projecting an established population figure as the most nearly accurate of the three they employed. Their 1565 population estimate, being derived from several types of settlements, provided a more reliable basis for projection than Kubler's (1942: 614) figures which were "almost entirely dependent upon encomienda lists."

In other words, Cook and Simpson found considerable consistency between the three types of documentary sources they interpreted and compared. It is significant that they showed that military and clerical population estimates tended to underestimate rather than to overestimate as Willcox, Kroeber, Rosenblat, and Steward assumed.

Whether by intent or by accident, analysts of the "skeptical" school of thought have emphasized the motives priests may have had for overreporting unconverted American population, and the motives soldiers may have had for overestimating enemy forces (Steward 1949: 657). Empirical evaluation of sources by comparison of documentary statements produced under different circumstances reveals impelling motives to underreport Indian populations. Kubler (1942: 614) explicitly assumed that clergymen being "usually pro-Indian, striving constantly to moderate the burden of tribute" tended to underreport Indian populations. He also assumed that *encomenderos* overreported population from opposite motives. Cook and Simpson (1948: 5) tested this latter assumption against actual data. They compared the populations of the Cortez estate in terms of the tribute values reported by its owners with those shown on a royal inventory made after it was confiscated. The owner's statement for 1560 was 37,314, but the crown agents found 72,139 persons seven years later. Cook and Simpson point out that *encomendero* reports of population or of tribute receipts tend generally to be depressed in this way out of motives of economic gain. It was a "plain case" of concealing assets from the tax collector (Cook and Simpson 1948: 4). Another partial check on aboriginal central Mexican population is found in a report by Alonso de Zorita (Pacheco and Cardenas 1864: 114–15) to the king of Spain. The Spaniards forced the Indians living within a 30–40 league radius of Mexico City to erect a great stone wall to contain their cattle. "They say that over two million people between masons and laborers were occupied . . . four months or a little less." Presumably the Spaniards or the Indian foremen kept books on the work crews so the figure talked about should have been fairly accurate. That such a work force could be raised after the decimation of the first smallpox epidemic certainly bespeaks a large central Mexican aboriginal population. An area with a radius of 100 miles (40 leagues of 2.5 miles) would clearly not include nearly all central Mexico. Yet to raise a work force of 2,000,000 in such an area would require a total population considerably larger to sustain the workers, who built the wall at their own expense. Assuming that all the workers were men, an equal number of women is implied. It is doubtful whether the Spaniards could muster all living males for such a project, or even all the able-bodied males. If the conquerors were able to turn out even as much as two men out of every three for their project, 1,000,000 more would have stayed at home, implying another 1,000,000 women, or 6,000,000 Indians altogether. Such a number would have meant a population density of about 190 persons per square mile or 73.4 per km² in the most densely populated part of central Mexico.

The comparison of populations at 2 points in time to calculate a ratio for use in further estimation is an obvious method used by several investigators already mentioned. Rivet obtained a depopulation ratio for the hemisphere by comparing contemporary with estimated aboriginal North American population. Cook and Simpson obtained another depopulation ratio for central Mexico by comparing 1565 counts with estimated preconquest population in several cities or areas. Rowe (1946: 184, Table 1) obtained a ratio of four to one for Peru by comparing Spanish reports of 1525 or conquest population and 1571 populations five provinces.

*Tribute-payers.* Cook and Borah have produced an analysis based upon the largest community sample yet obtained from New World population records for the first postconquest century, using the biochronic ratio determination method. Their comparison between 1550 and 1570 populations in 64 central Mexican tribute-paying communities on the coast and in the highlands yielded an average annual depopulation rate of 3.8% (Cook and Borah 1957: 465). This was depopulation from endemic disease, since no epidemics occurred in central Mexico during this 20 year period (p. 467). This computation effectively disposes of the Kroeber (1939: 160) claim that any shrinkage in Mexican Indian population occasioned by the conquest was transient and made good by increase resulting from the "new experience" of internal peace under Spanish colonial administration. It also contradicts the Kubler (1942: 623) contention that "Between 1546 and 1575, it will be seen that an extraordinary rise in population occurred." Apparently Kubler's sample of *encomienda* towns—the defects of which he himself pointed out—was very seriously different from other Mexican towns of this interepidemic period. It would appear that the *encomiendas* must have been attracting migrants, since their inhabitants are not likely to have been any more immune to disease than other Indians.

The Cook and Borah computations were based on tax records so the underreporting motive of tribute recipients presumably produced minimal population figures in these documents. All the more significant, therefore, is the Cook and Borah (1957: 466) estimate of about 25,300,000 central Mexicans living in immediate preconquest times. This was obtained by projecting back to 1519 the population estimate for 1565 derived from tax records by Cook and Simpson (1948: 50–165). This higher estimate of the size of aboriginal central Mexican population should be considered a minimal estimate inasmuch as it projects only the endemic disease depopulation rate Cook and Borah found to have obtained between 1550 and 1570, and does not take into account the tremendous depopulation produced during severe epidemics in 1545 (Ocaranza 1934: 84; Bancroft 1883: 756; Zinsser 1935: 256; Kubler 1942: 623, 631), in 1531 (Ocaranza 1934: 84; Steck 1951: 88), and in 1520–21 (Shattuck 1938: 40; Ocaranza 1934: 83; Sahagun 1955: 61–62; Diaz del Castillo 1956: 293, 328, 349, 336; Steck 1951: 87–88).

In a later study of central Mexican population over the shorter period between 1531 and 1610, Cook and Borah (1960: 49) present evidence that their previous estimate of preconquest population was lower than reality. Their new calculations show a population trend "fundamentally the same" as that Cook and Simpson originally described. The new study, however, "differs from that work in postulating a far higher initial aboriginal population and a far greater and more rapid decrease."

If the first three major epidemics reduced the central Mexican population by 4,700,000 individuals, then the 1519 population was on the order of 30,000,000. The 1st epidemic alone probably reduced it by more —early chroniclers thought half the Indian population died (Nuix 1782: 81), a reasonable statement of smallpox epidemic mortality in any non-immune population with no knowledge of proper care of patients infected simultaneously so that many perish from lack of care, including food. A population of this size would have had a density of 58.4 persons per km$^2$.

Borah (1962*a*: 175) has noted that the native population on Mexico's Gulf and Pacific coasts virtually disappeared in a single generation.

An additional check on preconquest central Mexican population derived from a different class of documents has been provided by Borah and Cook. The documents most likely to yield direct information on preconquest affairs would be preconquest documents—virtually all destroyed by zealous Spaniards. Borah and Cook (1963: 22–44) have employed the next best sort of sources now available—three very early colonial records of tribute payments to the preconquest Triple Alliance. Two of these are codices with pictographic representations, one annotated in Spanish, and the third a set of testimonies on preconquest tribute practices the Spaniards elicited in 1554 from aged informants.

In estimating population from tribute records, Borah and Cook (1963: 6–17) assembled evidence that "systems of community taxation and allocation did indeed exist throughout central Mexico." They took into account slaves and others not subject to tribute exactions (pp. 18–19), and the areas of central Mexico independent of the Triple Alliance (pp. 20–21). They had to make several arbitrary decisions as to the frequency of tribute payments and the relative values of commodities paid, which they reported quite explicitly (pp. 45–59). Besides calculating average family size, they had also to determine family tribute quotas in order to estimate population (pp. 60–71).

Assuming average family size to have been 4.5 persons, Borah and Cook (1963:88) estimated 25,200,000 central Mexicans on the eve of Spanish conquest, pointing out that an average family of 5 members implied a population of 28,000,000. These two estimates imply population densities of 49 and 54.5 persons per km$^2$ in central Mexico as defined by Borah and Cook (1963: 157). Certainly the density of all central Mexico would have been less than in the lacustrine area, where the wall-building figures cited above indicated 73.4 persons per km$^2$.

The Cook and Borah and the Borah and Cook calculations emphasize the Cook and Simpson finding that computations of total populations based upon

baptismal figures or army size tend to produce underestimations. This conclusion accords with ethnographic experience that the investigator with limited opportunities for personal contact with peoples of a different culture finds it very difficult to form a realistic idea of how many individuals there actually are in a sizeable population.

A projection exercise may be employed, for what the comparison is worth, to evaluate the various estimates of central Mexican population. Vaillant's (1944: 127, 137) figure of 300,000 residents of Aztec Tenochtitlan just prior to Spanish conquest is reduced by Borah and Cook (1963: 78–79) to 235,000 by excluding 125,000 in Tlaltelolco from a combined city total of 360,000 (an average of 6 persons per house). This can serve as a basis for projection to the Empire and non-imperial peoples of central Mexico. The imperial capital contained 7.1% of the total population if that were 3,300,000, but only 4.4% if the total were 5,300,000, 2% if the total were 11,000,000, 0.93% if the total were 25,200,000, and .78% if the total were 30,000,000. Since Tenochtitlan derived its economic support primarily from the Aztec Empire, which had to maintain other urban centers, it is doubtful whether it could have comprised 7% of all central Mexican population. The larger estimates of total population appear progressively more reasonable by comparison to the city.

## Additive Methods

In estimating the size and density of aboriginal populations in the central Lower California desert, Aschman (1959: 147) employed what he termed "the additive method," which appears to be quite similar to bichronic determination. Ascertaining the "maximum recorded population" in Roman Catholic mission records, Aschman then simply added numbers to take into account documented population events. He (pp. 152–53) added the number of deaths during disease epidemics the priests reported had occurred prior to the date of recording the maximum recorded population. In calculating the number of natives in a defined area, Aschman (p. 154) also took into account those Indians shifted administratively from one mission jurisdiction to another and inhabitants of settlements omitted from the maximum recorded population. Aschman figured, moreover, what proportion–20% of the total populace in one area—remained still unconverted to Christianity and not, therefore, reported at the time the maximum recorded population was noted.

Being based upon church records of actual Indian convert populations under direct control of missionary priests who were subject to periodic inspection by ecclesiastical and civil authority, Aschman's additive method neatly invalidates any assertion that the figures in question represent priestly overestimation of little-known aboriginal populations. His method shows very clearly that church baptismal records grossly underrecorded true aboriginal population for a number of reasons, among them Indian mortality prior to the first conversions, resistance to baptism by native religious leaders on grounds that it was a lethal ceremony, and sheer missionary ignorance of unconverted native populations with which little or no contact occurred. Indeed, the major criticism that must be directed at Aschman's method is that it seriously underestimates true aboriginal population because it is bound to contact-period records which reported maximum *recorded* populations long after initial contact and which all too often obscure the real scale of depopulation (Aschman 1959: 244).

## Aboriginal Social Structure Reconstruction

Means' reconstruction of the sociopolitical organization and probable population of the Inca Empire was one of the most original estimations of the size of an aboriginal American population using an ethnohistoric method. Means analyzed Spanish chroniclers' descriptions of imperial Inca administration as related by Indians who survived the conquest. These accounts show reasonable consistency in describing the main features of imperial administration, and from them Means (1931: 292) worked out the hierarchy from emperor to smallest administrative unit. He had the advantage that Inca political administration was ordered, at least in theory, in terms of administrative units of similar population size at all levels, and the model numbers were well and consistently remembered by the chronicler's native informants. Multiplying out the ramifications of the administrative system and computing average family size as 5–10 persons, Means (1931: 296) arrived at an estimate of from 16,000,000–32,000,000 persons in the preconquest Inca Empire at its largest extent. The higher figure is consistent with the estimate of 25,200,000–30,000,000 for aboriginal central Mexican population suggested above, in the sense that previous students of aboriginal numbers have accorded approximate parity to these two populations. Thus Sapper (1924: 100) placed the ecological potential of both Mexico and the tropical Andes at some 12,000,000–15,000,000 inhabitants. Kroeber (1939: 165) assumed 3,000,000 for each area. Rosenblat (1945: 92) calculated 4,500,000 in Mexico and 4,150,000 in Colombia, Ecuador, Peru, and Bolivia.

This method has the innate disadvantage of limitation to a hierarchically organized state whose administration is based upon a principle of equally populated administrative units. It is not clear, for example, whether the Aztec Empire was governed by such a system. Cervantes (1914: 295) copied by Herrera (1936: 192) stated that Moctezuma had 30 lords with up to 100,000 vassals, and 3,000 "lords of places." *If* the 3,000 local chiefs were under the 30 lords, and over a base population of about 3,000,000, the Aztec would have been organized rather like, although more simply than, the Inca Empire. The scant historic references to such neatly dovetailing figures foster the suspicion that the 3,000 local chiefs were not underlings of the 30 lords, the latter being simply heads of larger units conquered by the Aztecs. The system of requiring all to reside part of the year in Tenochtitlan and leave their relatives as hostages in order to depart (Cervantes 1914: 296; Herrera 1936: 192) greatly resembled the Inca device for controlling conquered rulers. The Aztec tax-collectors (Cervantes 1914: 298) appear to have been independent of the chiefs, and a direct arm of imperial authority, but Spanish destruction of nearly all of their "painted books, where there were so many accounts and such reason as to be a marvel" prevents a direct estimation of population based

upon their table of organization. In indirectly estimating population from fragmentary pictographic records, Borah and Cook (1963: 156) found the populations of various central Mexican provinces to have been quite unequal, judging from the tribute levied on each.

Mason (1957: 175) raised some common-sense objections to accepting the ideal Inca administrative system as having been rigidly applied in practice, claiming that it would have been "unworkable." Births and deaths of members of ayllus would have required "continual" adjustments in order to maintain any unit's population at the ideal level. The conquered provinces, moreover, varied considerably in population (Mason 1957: 174). Baudin (1961: 137; 1928: 121–22) had earlier made the same observation that mathematically accurate groups would not remain so for very long. Baudin (1961: 24) estimated the population of the Inca Empire "was probably between eleven and twelve million" at the conquest. Means (1931: 295) did not refute Baudin's stricture, but preferred to think that adjustments of actual units to the pattern by creating new units or reallocating households in old ones were in fact continual and effective. The Baudin-Mason criticisms are undoubtedly valid insofar as one might attempt an exact reconstruction of peak Inca Empire population, yet they do not seriously impute the utility of Means' method for obtaining an approximation of the actual population. Baudin (1961: 138), like Means, concluded that the Incas did adjust actual populations to the administrative model, contenting themselves with an approximation of the ideal.

The really serious limitation on the use of the method of estimating population upon the base of an ideal socio-political structure is simply the extreme scarcity of hierarchically organized states whose administrative structure is based upon a theory of uniformly populated governmental entities. An ancillary limitation on the use of this method is the rarity of state boundaries which coincide with those of cultural or geographic areas whose population an investigator wishes to discover.

## Resource Potential Estimation

Another indirect method of estimating aboriginal American population, derived from the intellectual current of environmental determinism, consists of computing potential human carrying capacity of the various geographical regions of the hemisphere, and then assuming that actual aboriginal populations reached their ecological maximum. By this method Sapper (1924: 100) estimated a hemispheric aboriginal population of 40,000,000–50,000,000, an estimate quite consistent with Rivet's.

A closely related procedure is projection of a population density figure based on an ideal or model societal type over a known geographic area. Guerra (1952: 228) estimated preconquest Cuban population at from 22,000 to 88,000 individuals by classifying those Indians as hunter-harvesters and projecting Ratzel's figure of 0.5–2 persons per square mile density for such societies across Cuba's 44,000 square miles. As an alternative, Guerra estimated that Cuba's preconquest population may have reached a maximum of 220,000 if the Indians relied heavily on fishing.

When the Spaniards went into Cuba in 1511, wrote Las Casas, they found there "many fair Provinces, inhabited with an infinite number of people" (Philips 1656: 21) or "great and populous provinces" (MacNutt 1909: 329). Guerra (1952: 227) estimated the preconquest population of the island at possibly 1,000,000 even though Cuban historians and demographers may prefer much lower estimates. Willcox (1931: 56) for example, inferred that Humboldt would have "put the pre-Columbian population of Cuba at less than 200,000." Yet all he had to go on methodologically was Humboldt's analogy between the impressions of populousness which might have been gained by Columbus and by the Englishmen who landed on Cuba in 1762. Spinden (1928: 643), too, thought most of Cuba to have been "scantily" peopled, but on archaeological evidence postulated and aboriginal native population of at least 100,000 in Puerto Rico, with more than 1,000,000 in the "Taino nation" on Puerto Rico, Santo Domingo and Cuba. Las Casas claimed that "above 600,000" Indians had perished on Puerto Rico and Jamaica by 1541, leaving "scarce 200" (Philips 1656: 20).

Las Casas (MacNutt 1909: 321) described Hispaniola as aboriginally divided into five kingdoms. The population was so large that in one kingdom the "lords" were "every one of them" (Philips 1656: 11) or "one alone of them" (MacNutt 1909: 321) able to muster 16,000 warriors in the king's service. Perhaps the native leaders whom Las Casas said he knew sought to impress him. In any event, a large native population is implied even if the Indians were boasting. Another Spaniard attempting to impress upon the Spanish crown the extent of Indian depopulation to 1685 claimed that the island of Hispaniola had a population of over 3,000,000 Indians when discovered (Fernandez 1949: 29).

## Direct Observation

Ordinarily the social scientist prefers first-hand observations and interviews with native respondents to the type of procedures thus far discussed for collecting data on population trends. In the geographically isolated Amazon Basin of South America, a few native populations have survived in areas so inimical to European exploitation that their first effective contacts with non-Indian society seem to have occurred recently enough for ethnologists to collect information on precontact population characteristics by interviewing informants. Vellard had this experience in 1938 among the Sabané and Tagnani divisions of the Ñambikwara. The former began to approach Brazilian camps about 1926, and in 1929 their first influenza epidemic almost annihilated them (Vellard 1956: 80). Three hundred in one band died of pulmonary oedema in 38 hours (Lévi-Strauss 1961*b*: 286). In 1931 a group of 300 Sabané visited a Brazilian post, where they waited two months for a wagon train carrying them gifts and rations. Shortly after the wagons arrived, epidemic bronchial pneumonia virtually wiped out this group. The few survivors who

fled to their settlements spread the epidemic.

Only 21 Sabané survived in 1938, by which time they had had to join the Tagnani. From an unknown pre-epidemic population, this group was reduced to less than 100 by 1938, including the Sabané refugees (Vellard 1956: 81). Holmberg (1950:9) reported that a Siriono school population halved during the years 1940–45. Métraux (1962: 107) reported a Kukraimoro group whose members were reduced by half during one year for lack of resistance to disease agents of the whites.

So few native American groups have survived unaffected by Old World disease agents to the present that the direct interview and observation methods can be but little used in studying the questions of pre-contact hemispheric population and historic population trends outside the Amazon Basin. Ribeiro (1956) has summarized numerous other samples of directly observed decimation of modern tribal peoples in Brazil. Métraux (1962: 108) summarized the contemporary situation of the Indians under the Indian Protection Service succinctly: "Pacification is synonymous with disappearance." The surviving unaffected groups are, moreover, by their very nature among the least important populations for determining the extent of preconquest human proliferation in the hemisphere. The civilized Indians subject to the native American empires were by far the most numerous of all New World groups, and estimations of their pre-Columbian magnitude must rest upon methods adapted to documentary analysis.

## SCALE OF DEPOPULATION BY DISEASE

If American Indian depopulation still occurs within the present century on the scale Vellard encountered among previously isolated Amazon Basin tribes, the central question in attempting to estimate how many Indians inhabited the Western Hemisphere prior to European discovery would seem to resolve itself into determining whether such severe depopulation was the rule or the exception.

The key case of the California Indians merits reexamination in these terms. Kroeber's comparison of the number of Indians enumerated in California during the 1910 census with his own estimate of aboriginal population convinced him (1925: 886) that only one Indian remained where eight had lived prior to the coming of the white man. He declared, however, that: "The causes of this decline of nearly 90 per cent within a period ranging, according to locality, from only 6 to 14 decades, are obscure. New diseases and alteration of diet, clothing, and dwellings have undoubtedly contributed largely" (p. 887). Because other peoples subjected to such influences have thrived, Kroeber went on to posit a cultural explanation for the decrease he recognized. He pointed to a direct correlation between intensity of contact with Europeans and depopulation (p. 888).

Kroeber (p. 891) ended on a final note of skepticism: "The decrease is saddening, however cautiously we may assume the absolute numbers. But excessive exaggerations need also be guarded against, such as the statement sometimes cited that 70,000 California Indians died of epidemic diseases in a few years following 1830."

Cook (1943: 161) asserted that "it is to be doubted if Kroeber's conclusions can ever be subject to fundamental or drastic revision," but he nonetheless pointed out (p. 187) that "Kroeber, as wel as other investigators, have not realized the tremendous death rate at the California missions." In an ealier ethnohistorical study, Cook (1940: 7) found that the mission Indian population of California was only 18,100 in 1830 (compared to Kroeber's figure of 24,634) and fell to 14,900 in 1834 (compared to Merriam's figure of nearly 30,000). Such a revision of terminal mission-period population figures might, at first glance, seem to require a revision downward of the Kroeber and Merriam estimates of aboriginal population. Although he did not attempt to estimate the aboriginal population of the missionized area of California at the time, Cook (1940: 24) showed that the mission Indian death rate varied between 70 and 85 per thousand population, compared to a birth rate which declined from an initial 45 to 50 per thousand to about 30 per thousand (p. 16).

This discrepancy between death and replacement meant an absolute biological loss of hundreds of mission Indians annually; e.g., Cook (1940: 23 n. 4) found records of absolute losses of 608 human beings in 1796, 773 in 1811, and 616 in 1814. The drop in mission population from 1830 to 1834 when missions began disintergrating averaged 800 annually, probably signifying a net biological loss of around 600 per year. If the annual loss were in fact 600 on the average each year between 1770 and 1830, the total Indian loss within the missionized region approximated 36,000 individuals. This figure is double the number of 1830 survivors, not half the number of survivors, as Kroeber calculated. It indicates a pre-mission population on the order of 54,000 Indians in the missionized area.

In a venture into "dead reckoning" estimation of aboriginal Indian population, Cook (1943: 194) succeeded in raising Kroeber's figures for the California Indians (excluding the Modoc, Paiute, Washo, Eastern Mono, and Colorado River tribes) by only 7%; even using many documentary sources, he arrived at 133,550 for the same groups Kroeber assigned 125,000.

Cook (1939: 177) had already published mortality figures from seven California missions during the smallpox epidemic of 1828, showing a death rate of 51.5 per thousand population from that cause alone. Cook (p. 185) also noted a severe smallpox epidemic in 1837–39 in California with contemporary estimates of Indian mortality expressed in tens of thousands.

Precisely the matter of epidemic mortality in 1830–33, which Kroeber had expressly discounted, provided Cook with the evidence necessary to revise fundamentally and drastically his own and Kroeber's conclusions. Utilizing mainly traveler's accounts of pre-epidemic populations, the spread of the contagion, and its impact on the Indians, Cook found that Indian mortality was approximately the same in the Willamette Valley and the central San Joaquin- Sacramento Valley of California—about 75% of the pre-epidemic population. This meant that ¾ of the lowland riverine dwelling Indians who had already survived some six decades of Spanish and Mexican rule or contact with Indian Christian converts, were wiped out during a single summer ( Cook 1955a: 322).

In other words, the aboriginal population of the San Joaquin-Sacramento River Valley exceeded Kroeber's estimates by a factor of two or three. The Wintun pre-epidemic population was on the order of 24,000–36,000 using Kroeber's (1925: 883) 12,000 ethnographic estimate as a base, or 28,500–42,750 using Cook's (1943: 179–81) revised ethnographic estimate as a base. Wintun population may well have been even larger in 1770. The Yokuts pre-epidemic population was on the order of 36,000–54,000 prior to the 1833 epidemic, rather than 18,000 as estimated by Kroeber and previously accepted by Cook (1943: 192–93). Even that total was certainly lower than it had been in 1770, since the Yokuts in the San Joaquin Valley were near enough to missionized populations to have contracted earlier epidemic diseases. In a seperate analysis of San Joaquin Valley aboriginal population based on Spanish sources, Cook (1955*b*: 70) estimated a total more than four times that of survivors in 1850. He reiterated that "warfare, massacre, forced conversion, starvation and exposure" plus "sweeping epidemics" together "destroyed in the aggregate fully seventy-five per cent of the aboriginal population."

Since Kroeber approximately halved Merriam's estimated aboriginal Californian population, Cook's factor of two indicates that Merriam's estimate of 260,000 aboriginal Californians was more or less accurate as an estimate of pre-epidemic population. Cook's factor of three yields a figure of approximately 400,000 aboriginal population. This sum needs to be reduced only sufficiently to omit the mountain and coastal tribes, which were not affected, from the calculation of epidemic depopulation. An aboriginal Upper Californian population on this order would have had a density averaging perhaps two persons per square kilometer. In view of the greater ecological potential of Upper as compared to Lower California, such a density is much more logically consistent with Aschman's minimal one person per square mile in the central Lower California desert than Kroeber's estimated density of less than half as much for the more generous ecological region.

Cook's (1955a) analysis of this California epidemic demonstrated the operation of two very important processes in the human ecology of aboriginal American populations. First, he showed the magnitude of mortality which a single epidemic can cause in a non-resistant population. Second, he called attention to the biological fact that epidemic infection is not limited to tribal populations in immediate face-to-face contact with Europeans. The decimation of native Californians was not limited to missionized Indians, but extended outward as far as disease agent and vector could spread infection from intrusive (white) carriers to aboriginal populations. It is necessary to maintain constant awareness of these two processes or fundamental trends among natives of the New World. Any interpretation of reported native populations during the early years of contact with Europeans which ignores the tremendous mortality caused by epidemics inevitably underestimates the size of the aboriginal populace.

Cook's study showed quite clearly that one fatal defect in both Kroeber's data and method was ignoring disease, and particularly epidemic disease, mortality. This defect led to projecting post-epidemic ethnographic populations backward in time on the naive assumption that such simple projection could yield even approximations of truly "aboriginal" populations in the biological sense. Here reference should be made to Steward's (1949: 656) summary of the remainder of Kroeber's (1939: 180) methodological assumptions.

> (4) a rich ecology usually means a greater native density, but such factors as iron tools and friable soils must be taken into account in comparing modern and native densities; and (5) a rich culture is usually an index of a high density.

Nowhere are Old World disease agents mentioned as a factor changing native populations before the ethnographer's arrival among them, although as Vaillant (1944: 288) later pointed out: "European diseases, like smallpox, measles, and tuberculosis, wiped out great sections of the population." Kroeber (1939: 160) discussed apparent fluctuations in the Indian population of the central Mexican area without any mention of the impact of the introduction of European diseases to the civilized Mexican Indians and the toll of life these took in both endemic and epidemic form. Kubler, in contrast, concluded that: "Widespread and repeated epidemics was of course the major determinant and the most obvious one" of population decline in Mexico. Kroeber (1939: 160) did take into account warfare as a possible cause of depopulation, but omitted his own earlier recognition of diseases as one cause of native decline in his analysis of California Indian population (Kroeber 1925: 887).

The significance of epidemic depopulation among North American Indians had been pointed out by many writers before Kroeber and Rosenblat. Three anthropologists, upon two of whom Kroeber drew for material, did so in 1928. After concluding that there "are fewer Indians today than at the coming of Europeans," Spinden (1928: 660) asserted that: "The greatest factor in depopulation has been disease in epidemic form." Mooney (1928: 5, 7, 12–14, 21, 24–25, 27) repeatedly mentioned epidemic depopulation of specific North American Indian groups. MacLeod (1928: 40–52) also pointed out the ravages of epidemics among North American Indians. Wissler (1936: 36) explicitly recognized epidemic depopulation as an independent variable in Northern Plains Indian settlement. He concluded that because they largely escaped the smallpox epidemic which decimated other tribes in the Saskatchewan area about 1780, the Assiniboin were able to multiply rapidly and spread over most of the area. The previously marginal Cree, in turn, "expanded in numebr over the same territory" after ⅔ or more of the Assiniboin perished during a smallpox epidemic in the 1830's.

Later on, Steward (1949: 668) at least recognized that European diseases were one factor in native population decline, calling attention to their differential effect in neighboring tribes, some being wiped out and others surviving in strength to the present. Hoebel (1949: 401) noted pestilence and warfare as the major causes of Indian depopulation.

Such scientific awareness of epidemic impact on

native American population rests, of course, upon a very firm historical record. Las Casas was merely the earliest and best-known commentator upon the biological plight of the Indian. The controversy he initiated has probably waxed so hot as long as it has because he attributed Indian depopulation to Spanish cruelties—which, being expressions of human will, were presumably controllable—rather than to diseases whose transmission to virgin populations became inevitable once contact was established between Indians and Europeans. Transmission could not be avoided in any event, given the state of medical knowledge in both Europe and Indian America in the 16th century.

Defenders of European colonialism in the Americas have pointed out this defect in Las Casas' argument. Nuix (1782: 81) counted smallpox as one of four major causes of native depopulation. He blamed Spanish mining, (p. 46) the nature of Indian agriculture (pp. 76–77), and the lack of communication and trade with Spain, the last of which he attributed to the actions of other nations (p. 85). Lopez de Velasco (1894: 26) who mustered information on the entire Spanish colonial empire in the Americas, commented in 1574, "In every place discovered, the natives were in the beginning much more numerous than those who have existed later, because in many provinces where there used to be a great multitude of them, they have become almost entirely extinct." He attributed the depopulation he recorded to warfare and abandonment of fields to avoid subjugation, "from which there followed during the first years general mortality and illnesses never seen in those parts, such as smallpox which the Spaniards transmitted." Many local historians in the Spanish colonies recognized the prime role disease played in Indian depopulation. A 1579 observation on the decrease of Indian numbers in the Province of Tabasco was: "It has declined to this point because of great illnesses and pestilences which have occurred, both those characteristic of this province and those of general character throughout the Indies . . ." (Scholes and Roys 1948: 166). Clavigero (1781: 282) recognized that an infinite number of Indians perished in the epidemics of 1520, 1545, and 1576 in New Spain.

A defect in Kroeber's hemispheric estimation method was, as in Rosenblat's, omission of information on disease mortality patterns in populations estimated. The defect in method might be stated in the form of another implicit working assumption which Steward did *not* state: (6) A disease agent introduced into aboriginal American populations from the Old World has no significant effect on them.

Such an assumption is, of course, demonstrably unjustified, as Cook's analysis of the 1830–33 California epidemic, my (Dobyns 1963*b*: 496–515) analysis of epidemics among Andean Indians between 1525 and 1720, and many medical studies of epidemiology have shown. Any method of aboriginal population estimation which seeks to project back through time census or ethnographers' enumerations of recent Indian populations must assume that disease agents have affected the population trend of the group under investigation and seek data as to the nature and extent of their effect.

## DEPOPULATION RATIOS

Having indicated dissatisfaction with several previous estimates of the scale of population of aboriginal America, I find it incumbent upon me to indicate a workable method. I suggest another projection method employing a depopulation ratio established by comparison of relative numbers of a given group at two points in time. One such time should be that when the population analyzed fell to its lowest numerical strength.

Clearly, it would be preferable methodologically to establish depopulation ratios for each societal unit from pertinent documents, but that is impossible for lack of records. Even to establish regional depopulation ratios for each significant cultural area lies beyond the scope of this paper. The present attempt aims only at outlining a reasonable method for obtaining depopulation ratios.

In order to determine a "standard" hemispheric depopulation ratio for demonstration purposes, a ratio will be established between the known or confidently estimated preconquest population in 1 area and a known or closely estimated nadir population in that same area. The ratio thus obtained will then be tested for consistency against comparable ratios for other areas in order to arrive at what appears to be a generally applicable ratio.

*Central Mexicans.* Turning again to the Cook and Simpson (1948: 47) analysis, one finds that they estimated the nadir of central Mexican population as about 1,500,000 Indians. Comparing this with their estimate of 11,000,000 central Mexicans living in 1519 (p. 38) yields a depopulation ratio of 7.33 to one for central Mexico. In other words, where one Indian survived in 1650, 7⅓ had lived prior to conquest.

The Cook and Borah calculations of a 1519 central Mexican population on the order of 25,300,000 compared to the same nadir population provides a depopulation ratio of 16.9 to 1 for central Mexico.

Since those calculations ignored the tremendous depopulation caused by the first of three historic epidemics in central Mexico, a higher aboriginal population on the order of at least 30,000,000 is suggested. Such a figure means a depopulation ratio of at least 20 to 1 over the 130 years from aboriginal times to population nadir. In one small area, as a matter of fact, Cook (1958: 20–22) estimated an aboriginal population of 10,000 for Ixcatlán which stabilized at about 500 survivors in the 17th century—a depopulation ratio of exactly 20 to 1 parallel to a reduction from eight settlements to one.

*Coastal Mexicans.* A local historian in 1579, only 58 years after the Spanish conquest, asserted that the Province of Tabasco then contained less than 3,000 Indians. When conquered, it had held 30,000 inhabitants (Scholes and Roys 1948: 166). This meant a 10 to 1 depopulation ratio in less than 60 years.

*Andeans.* The best test for the central Mexican depopulation ratio would be comparison to such ratios for the native populace of the Andes, where civilization and urbanization also flourished prior to conquest. Unfortunately, early historic population trends in the area of the prehistoric Inca Empire have not yet been so systematically studied as those in central

Mexico, so the data required for a direct comparison are simply not available. The best that can be done is to consider some sample areas.

The Indian population of the central coast of Peru almost completely disappeared during colonial times. There the population of Rimac (the modern Lima area) declined from 16 to 1 and that in Chincha from 25 to 1 in less than half a century after conquest (Rowe 1946: 184, Table 1). Rowe, like Means, estimated the preconquest population on the basis of socio-political structure, allotting five persons to each of the families in the three "jurisdictions of 10,000 families each" in the valley (Cobo 1956: 301). Since Rowe based his family of five on 1571 conditions, after conquest and infection altered family composition, the actual aboriginal population (and, therefore, the depopulation ratio) was almost certainly greater.

Another useful sample is the work force compelled to dig ore from the Potosi mines and extract metals from it. Viceroy Toledo designated 16 Andean highland provinces to provide this labor in 1575, a generation after conquest, assigning 95,000 Indians. By 1663 the number had fallen to 40,115, and by 1689 it was down to 10,633 (Vellard 1956: 85). While other factors, such as out-migration from these provinces to avoid compulsory mine labor, helped to lower their population, the toll of disease is certainly reflected in the indicated depopulation ratio on the order of 9 to 1 over 115 years.

In contrast, the Marques de Varinas in 1685 wrote a description of the road from Lima to Paita very similar to travelers' accounts of California Indian settlements after the 1830–33 epidemic. "One recognizes at very short intervals mounds of skulls and bones of these miserable beings which horrify those travelling the road" (Fernandez 1949: 30). He estimated that scarcely 20,000 Indians remained of over 2,000,000 who had once inhabited this region (Fernandez 1949: 29), thus suggesting a depopulation ratio of 100 to 1.

Unsatisfactory as these figures are, they permit some comparisons with central Mexican ratios. The 9 to 1 ratio for the Potosi mine tributary area is nearest the central Mexican ratio derived from the Cook and Simpson low estimate of aboriginal population, or the Tabasco Province 10 to 1 ratio over only the first six postconquest decades. Yet the Peruvian ratio comes from a period beginning over 40 years postconquest, so cannot possibly take into account early population losses.

The figures for the Rimac Valley may have been skewed by immigration of mountain Indians to the colonial capital of Lima, so Rowe's 16 to 1 ratio is perhaps too low. It agrees most closely with the Cook and Borah estimate of aboriginal central Mexican population which fails to take into account early epidemic depopulation. The remaining Chincha area ratio of 25 to 1 is somewhat higher than the 20 to 1 ratio derived from a suggested 30,000,000 aboriginal population in central Mexico.

*Californians.* Lacking really adequate comparative data from the Andean region, one turns again to the California Indians, who have played an important role in forming anthropological conceptions of the size of pre-contact American populations. The Cook and Simpson central Mexican depopulation ratio of 7.33 to 1 and the Peruvian mine tributary ratio of 9 to 1 are most nearly consistent with Kroeber's (1925: 886) 8 to 1 California Indian ratio. Actually, that ratio should be stated as 8.3 to 1, even if the 1910 census figure of 15,850 is rounded off at 16,000.

Raising Kroeber's estimate by Cook's suggested factor of 2 to take into account epidemic mortality during the 1830–33 epidemic yields a depopulation ratio of 16.6 to 1. This is very close to Rowe's Rimac River Valley ratio in coastal Peru and the ratio for central Mexico based on the Borah and Cook estimate of preconquest numbers.

Multiplying Kroeber's low California estimate by Cook's suggested factor of three provides a depopulation ratio of 25 to 1. This is consistent with Rowe's Chincha ratio from Peru, and higher than the central Mexican ratio based on the 30,000,000 estimate. Since Spanish missionization in Upper California began in the 1770's, the period of population decline to the 1910 nadir was 130–140 years. That is comparable to the time from conquest to nadir in central Mexico, but considerably longer than the time span of the two coastal Peruvian ratios cited.

*Tierra del Fuegians.* The natives of Tierra del Fuego appear to be on the road to biological extinction. Cooper (1946*a*: 108) reported Ona survivors to number 50. Bridges (1948: 521) reported less than 150 full-blood Yahgan, Ona, Aush, and Alakaluf survived in 1947, plus as many half-breeds. Lipshutz and Monstny reported only 40 Ona in 1950, with 63 Yamana and 80–100 Alakaluf (Vellard 1956: 82–83). The Ona alone were estimated at 2,000 toward the end of the last century, and Bridges (1948: 521) estimated the total population at from 7,000–9,000 Indians in 1871. A single measles epidemic played a key role in upsetting the biological balance, killing half the Yamana in one area in 1884 (Bridges 1948: 125–26). Thus the Ona had a depopulation ratio of 50 to 1 from 1870 to 1950, a period of only 80 years.

*Amazonians.* In the process of depopulation of the Sabané Indians already alluded to, this division of the Ñambikwara dropped from about 1,000 in 1926 to 21 persons in 1938; only one Sabané remained where 47.6 had lived only 12 years earlier. The Sabané appear to be approaching extinction even faster than the Ona.

Surviving Sabané have joined forces with survivors of other Ñambikwara groups, so an amalgam social unit may eventually survive. When Rondon first penetrated Ñambikwara country in 1910, he encountered a very numerous population estimated at 20,000 in 1916 (Vellard 1956: 80). Not only the Sabané and Tagnani, but also other divisions were virtually if not entirely extinct by 1938 and the total population reduced to under 1,000 individuals (Vellard 1956: 81). Thus the depopulation ratio for the entire Ñambikwara group had been on the order of 20 to 1 over a period of 22 years.

Even more extreme was the case of the Cayapo, living on the banks of the Araguaya River. Dominican priests from France established outposts on the stream in 1903 to convert these warlike natives, who frequently attacked rubber tappers and prospectors. A few years later the Dominicans estimated the tribal population at 6,000–8,000 individuals. Inter-

group contact was limited almost entirely to the priest, who kept alcohol, tuberculosis, and venereal diseases from reaching the Cayapo. Despite their efforts, the Cayapo were reduced to 500 by 1918 and to scarcely 27 individuals by 1929, and two or three survivors by mid-century (Vellard 1956: 78–79). By 1918 the depopulation ratio was already 12 to 1, and by 1929 it was 222 to 1, indicating virtual extinction for the group.

The Caingangs dropped from 1,200 in 1912 to about 200 in 1916, and 80 in the 1950's, a ratio of 15 to 1. The Mundurucus decreased from 20,000 to scarcely 1,200 between 1915 and 1950, a ratio of 16.6 to 1. The Timbira, who numbered 1,000 in 1900, mustered barely 40 by 1950, a 25 to 1 depopulation ratio (Lévi-Strauss 1961*a*: 14–15).

*Northern Pimans.* American Indian groups can survive great depopulation through continual social and political amalgamation of survivors if the aboriginal population base be sufficiently populous (and, perhaps, geographically dispersed). This truism is, of course, rather obvious in the cases of the heavily populated native empires. The total aboriginal population necessary to insure eventual survival of an amalgamated populace is apparently not so large as the extreme depopulation ratios encountered among peoples with simple technologies in adverse environments in South America might suggest. Several thousand Northern Piman-speaking Indians in the United States today represent a recovering population that evidently reached its nadir sometime during the last century. Yet mortality was historically extremely high among the missionized riverine divisions of the Northern Pimans. One riverine group on the lower Santa Cruz River suffered a depopulation ratio greater than 51 to 1 between 1700 (earlier records are not available) and 1801, yet survived through continued interbreeding with immigrants from the same language group (Dobyns 1962: 28).

The fact that this group barely survived suggests that a ratio on the order of 50 to 1 over the period of a single century approaches very closely the maximum possible depopulation that will permit survival and recovery by a given population. A ratio any higher almost certainly means extinction for the group. Yet it must be remembered that aboriginal disease conditions ended among the Northern Piman Indians long before 1700, so survival of even greater population losses seems possible.

*General Ratio.* From the wide range of depopulation ratios encountered for various native American populations in widely scattered portions of the hemisphere, it appears that a ratio of 50 to 1 over a century marks the approximate outer limits of human survival and population recovery. Criteria for identifying a minimal depopulation ratio for all native American populations are less readily established. The data analyzed in this paper do indicate that ratios clustering around 7 or 8 to 1 over a period of 130–140 years between contact and population nadir are to be regarded as much too low to reflect the reality of American Indian depopulation. These data also indicate that depopulation ratios on the order of 16 or 17 to 1 are approaching historical reality, but are also very likely low.

The data that best withstand testing are those indicating a depopulation ratio of 20 to 1 for central Mexico from conquest to the beginning of population recovery and a ratio of 25 to 1 for the California Indians and the coastal Peruvian Indians of the Chincha region. It is, therefore, interesting that a Spaniard writing in 1685 should have opined that the Spanish government "has now left of twenty parts one" (Fernandez 1949: 29) on the Caribbean coast between Vera Cruz and the Orinoco and in southeastern South America. The depopulation ratio of 20 to 1 appears to be a sound, if perhaps conservative, tool to employ as a hemispheric minumum. It agrees with Borah's (1962*b*: 179) estimate of 95% reduction in native population over a century, although a longer period for such loss is accepted in this paper.

## ESTIMATED ABORIGINAL POPULATION

Assuming a "standard" hemispheric depopulation ratio of 20 to 1 between initial contact and the beginning of population recovery should permit estimating aboriginal American population if sound figures on the size of the various Indian groups when they reached their respective nadirs can be found. If a population did not recover and the group became extinct, a depopulation ratio has little utility for calculating its original pre-contact scale. It may be possible to use a ratio to approximate the aboriginal magnitude of such extinct groups based on the number of survivors about 130 years after initial contact. There is some evidence that this was frequently the time native American populations required to reach nadir and begin to recover—at least it was so among the central Mexicans and California Indians.

As an antidote to previous overly low estimates of aboriginal American population, and as a working hypothesis to stimulate research into this question, a summary of the calculations performed in this paper is offered in Table 2.

Borah (1962*b*: 179) recently advanced the figure of 100,000,000 as very possibly the population of the New World at the end of the 15th century. The calculations in the present discussion support his approximation, for his estimated population would indicate a hemispheric average depopulation ratio between 20 and 25 to 1. My figure implies a hemispheric average population density of 2.1 persons per km². Borah's implies 2.4 per km².

## Abstract

Social scientists often consider population size as an independent variable of major importance. The author analyzes, therefore, methodological reasons why most prior estimates of aboriginal American population imply small scale preconquest societies and concludes that the population was far larger than has been thought. The range of previous estimation is so great as to indicate that some methods or data must have been faulty. Skeptical anthropologists and historians have regarded historic population figures reported by contemporary observers as larger than reality. Ethnohistorical estimations based on careful cross-checking of direct and indirect sources of popu-

TABLE 2

ESTIMATED ABORIGINAL POPULATION

| AREA | NADIR POPULATION | DATE OF NADIR | PROJECTIONS × 20 | PROJECTIONS × 25 |
|---|---|---|---|---|
| North America | 490,000[a] | 1930+ | 9,800,000 | 12,250,000 |
| Mexican Civilization | 1,500,000[b] | 1650 | 30,000,000 | 37,500,000 |
| Central America | 540,000[c] | 1650 | 10,800,000 | 13,500,000 |
| Caribbean Islands | 22,150[d] | 1570 | 443,000 | 553,750 |
| Andean Civilization | 1,500,000[e] | 1650+ | 30,000,000 | 37,500,000 |
| Marginal South America | 450,000[f] | ? | 9,000,000 | 11,250,000 |
| Western Hemisphere | | | 90,043,000 | 112,553,750 |

lation data demonstrate that contemporary observers underreported the true magnitude of native American populations.

Estimates obtained by projecting backward through time population data from modern Indian census counts are seriously defective, as are past applications of the the dead reckoning mode of estimation. Ethnohistorical methods are less deficient. Cross-checking sources serves to increase the accuracy of estimation, and informant knowledge is evaluated in terms of opportunities for accurate observation and especially for accurate memory, since it is more difficult to retain quantitative than qualitative data.

Additive reconstruction of Indian population from historical records is handicapped because the geographic spread of literate Europeans lagged behind the diffusion of new disease agents which decimated aboriginal populations. Direct observation of Indian population trends by anthropologists suffers the same limitation, even though several have recorded extremely rapid depopulation of Amazon basin peoples within the century.

Calculation of aboriginal population from preconquest social structure is possible only in 1 area governed by an imperial ideal of administrative units consistent in population.

a Rosenblat (1945:22) gives figures for Canada, 127,374; the United States, 332,397; and Alaska, 29,983; a total of 489,754.

b Cook and Simpson (1948).

c Rosenblat (1954:59).

d Rosenblat (1945:81). The 1650 population of the Caribbean Islands was only 10,000 Indians, according to Rosenblat (1954:59). By 1825 the natives had been exterminated (pp. 36–37). Since contact began earlier in the islands than on the mainland and insularity probably hastened the depopulation process, the 1650 population was already well under comparability with the nadirs of populations which recovered. Using the 1650 figure, for lack of a better, yields an estimate of 200,000 persons almost certainly less than the actual aboriginal population. . . . . .

Using a shorter time period than 130 years for the Caribbean Islands may compensate for the apparently faster rate of population decrease there than on the mainland. The Indians of Jamaica were already extinct by 1570 (Rosenblat 1945:81), but the other islands marshaled some 22,150 surviving aborigines. Taking this figure as equivalent to the nadir of population that recovered implies a pre-contact populace of about 443,000 individuals, compared to Willcox's (1931:56) "less than six hundred thousand" obtained by assuming that 1/3 of the modern Caribbean population lived on that island, and Rosenblat's (1954:102) 300,000.

e The figures Rosenblat (1945:36, 57) assembled indicate that the Indian population of South America may have reached its nadir later than the population of central Mexico, because his estimates for Indian survivors at the epoch of national independence are appreciably lower than those for 1650. Taking his 1825 approximations as representative of the Indian nadir, and using the factor of 20, one would postulate an aboriginal population in all South America on the order of 72,500,000 persons.

Such a high figure is not consistent with previous images of the relative population density of Andean and Mexican civilizations. These have generally been assumed to have encompassed populations of about the same magnitude.

It would appear, therefore, that Rosenblat's approximations for 1825 represent in fact a recovering South American Indian population already past its nadir, despite the fact that his 1825 estimates of Indian survivorship are lower than those of 1650 and in spite of Kubler's (1946:336, Fig. 32) graph of colonial population decline in Peru until after 1781.

f Sapper (1924:100) allotted tropical eastern South America a population 17–20% of the size of his estimate of that in the Andes. Rosenblat (1954:102) allowed Brazil, Venezuela, the Guianas, and Paraguay somewhat over 40% of his Andean figure. Steward (1949:666, Table 3) allowed these national areas plus lowland Colombia 42% of his Andean estimate. These proportions of 20–40% imply from 6,000,000 to 12,000,000 aboriginal Americans in this area, based on an Andean total of 30,000,000. A Spanish decree of 1639 claimed that Portuguese slave hunters had captured over 300,000 Guaro Indians from 1 province in the Uruguay-Paraguay area during a 20-year period. Hernandez (1912:123) supposed that 2 Indians escaped for every 1 enslaved, and estimated a 1,000,000 preconquest population, with possibly 500,000 more in Paraguay proper. 6,000,000 Indians living in tropical South America prior to contact seems a reasonable supposition in view of historical population movements of this magnitude.

For temperate South America, Sapper (1924:100) estimated an aboriginal population 8–13% as large as his estimate of Andean civilized populace. Rosenblat allotted Uruguay, Argentina, and Chile a population about 22% the size of his Andean figure (Rosenblat 1954:102). Steward (1949:666, Table 3) allocated these areas 28% of his Andean population size. An aboriginal population 10–20% as large as the Andean total implies another 3,000,00–6,000,000 Indians in this section of the continent.

The aboriginal Araucanian population alone has been estimated at from 500,000 to 1,500,000 (Cooper 1946*b*:694). If the nadir in this group's population occurred around 1900 with approximately 100,000 survivors (Cooper 1946*b*:695), then the higher estimate for aboriginal numbers appears conservative, inasmuch as it would imply a relatively mild depopulation ratio of only 15 to 1 for an amalgam native group constantly at war with the Spaniards for centuries, as well as exposed to contagion.

Approximately accurate estimates of aboriginal American population may be achieved by comparing the population of a given area at two or more times in order to establish population trends expressed as ratios of the size of the population at 1 time to its size at another. Early historic depopulation was great in the Americas: a well-documented instance of recovery following depopulation from 50 to 1 over one century indicates limits to such a trend. Greater population loss probably results in extinction. A hemisphere-wide historic depopulation ratio of 20 to 1 is postulated. Applying it to more or less well-established historic Indian nadir populations suggests that the New World was inhabited by approximately 90,000,000 persons immediately prior to discovery.

## Comments

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Since I am not an Indianist, there is little I can say about the specific issues centering around the estimating of Amerind population magnitudes. I shall confine my remarks to more general considerations of method and aim in anthropological demography.

I am struck with the fact that the enormous labors reported in and represented by these two papers are reducible to a single line or two in a census volume. I recently completed a study of cultural ecology in a modern agrarian population for which census data was available over a period of 70 years, almost the entire history of settlement in the region. The availability of this material permitted us to give immediate attention and energy to other matters, since the demographic data were, or would be, well in hand.

On the other hand, somewhat later in the study we found that even census data is not detailed (or accurate) enough in some instances to permit the finegrained ecological analysis we sought in this study. We therefore selected a group of geographical areas (townships) representing key demographic (census) and econecological variants and constructed genealogically based censuses for the entire history of settlement, that is, a complete accounting of the population for these districts in terms of actual family and kin-group histories, including a record of in- and out-migration, destinations and origins, etc. These genealogical studies incidentally served as a check on the accuracy of census data for the region in general. We found it to be good on gross magnitudes, fair or poor on sex and age enumeration. This particular finding suggests that anthropologists who use social structure as a basis for population size estimations may be on the right track, and further, that an approach to population dynamics—i.e., something other than gross magnitude estimates—might be possible even with ethnological-historical data and interpolations.

If the investigator begins to think in terms of population dynamics (rates of increase, age pyramids and their changes, migration, etc.) instead of absolute magnitudes, a somewhat different view of the estimation problem emerges. Thompson's paper is quite convincing, but his methods require him to deal with Chipewyan population and its econecological determinants as if they were constant and static. The access by Chipewyans to bison, moose, and other "secondary" food sources is assumed or ascertained to be minimal in order to obtain the single major food animal demanded by the formula with which Thompson works. Thus the Chipewyan "population" which results is simply that population which was more or less probable for any time during which the Chipewyan subsisted only or mainly on caribou. If this changed, as I have no doubt that sometimes it did, then presumably the population would change also—unless some other factor began to operate at that moment. The "population" thus derived is therefore not—or only in a qualified sense—an estimator of the actual population of these Amerinds.

The distinction made above may not be in all cases a vital one, but it is worth considering. Dobyns confronts the problem represented by it in some of his reviewed estimates, but does not quite come to grips with it. He treats all of the estimates, however critical he may be of them, as estimates of actual population, and I have no doubt that their authors did likewise. On the basis of this distinction, Dobyns might well have thrown out some of these "estimates" completely as too experimental or conditional for historical use.

At the same time, these conditional "estimates" lead in the direction of a completely different kind of research: ecological, with special reference to population dynamics and change. All Thompson has to do to make his study demographic-ecological is to vary the quantities in his formula and then compare the results with data from any other group of similar aborigines. When the specified conditions are changed, and the results are found to agree with some independent figures of size, sex ratio, migration rates, etc., then one has made a true demographic study of a tribal population and not merely a population size estimate. The size estimate is, of course, interesting to ethnologists, but it makes very little contribution to the science of demography and ecology. Thompson is on the edge of making such a contribution; Dobyn's paper, however informative and clarifying it may be, makes no move in this direction.

If one considers populations as potentially fluctuating entities, one must begin looking at curves rather than figures. There are curves hidden all through the two papers, but they get lost in the single-minded attack on the magnitude-estimation objective. Dobyns does not consider the possibility that some estimates of population size which he rejects because they are "too small" may in fact be good estimates of the possible *minimum* population; the estimators, in their caution and "skepticism," may have hit on the specifications of conditions necessary to produce a low point in the curve. We simply need not assume that all Amerind populations had flat curves. While Dobyns, of course, does not, he considers disease, etc. only as they influence his pre-Columbian figure and therefore neglects the possible value of varying estimations for indicating *fluctuation*.

The use of population-density figures for standard ecological subsistence-technologies is an approach which goes back to Ratzel and which is here elaborated by Thompson. This traditional approach usually neglects or ignores the question of population fluctuation. Only in Birdsell's remarkable studies (1953, 1958), based on Tindale's data, have the conditions for fluctuations, here linked to social structure, been suggested. This neglect of dynamics is not entirely due to the lack of accurate demographic data or multichronic figures, but rather to a failure to come to grips with the problem of what conditions would lead to population changes. What influences migration rates, sex ratios, fertility rates, death rates? These proportions change quite systematically, and accurate projections can be made given minimal data. Or, at least, assumptions no less well-founded than Thompson's can lead to results no less (or no more) accurate than his.

In inquiries of this kind, gross magnitudes of large areas or continents are not the major objective, because one selects populations on a regional basis. Since migration is a key factor—*all* human populations, not merely wheat farmers on the Great Plains, are to a degree controlled locally by migration —one's focus moves toward particular ecological-demographic areas and away from "total" populations; that is, toward Thompson's type of unit, not Dobyns'.

A start could be made with existing census materials on both Indians and Whites in selected regions of North America. My own work indicates that where livestock is the major source of income, population curves are low and only slightly undulating; i.e., they reflect a consistently small population, with just enough out-migration to keep it in control. This type of curve seems similar to that described, by implication, by every ethnological writer on hunting-gathering peoples. The possibility that ranchers and farmers in North America and hunter-gatherers since the Upper Palaeolithic have the same basic demographic curve is one that needs investigation.

Archaeological evidence for migration has been almost wholly ignored. Migrations as an explanation of anything fell into disrepute among archaeologists because they were too easy a

way of explaining similarities in material culture over long distances. Still, people do move, and demographically this is important. Accumulating evidence from the Cahokia prehistoric settlement near the confluence of the Mississippi and Missouri Rivers suggests a proto-urban community covering an area of 5 square miles. This is only a guesstimate, however, because Warren Wittry's habitation excavations suggest a series of more or less separate village nuclei rather than a mass urban aggregation. At intervals large groups from the Cahokia settlement must have moved along the rivers to create major satellite centers —Aztalan in Wisconsin is one of the best known. In light of these possibilities, Cahokia population size is only one datum; fluctuation and movement are equally important. Archaeological evidence exists or is accumulating to get at these.

My final comments refer to an entirely different problem: the intriguing question of why Kroeber and others consistently underestimated Amerind population. Why the "skepticism" of which Dobyns writes? The underestimations of Kroeber and some others were due at least in part to the fact they were made in the 1930's, when very little was known about settlement pattern (the term was not even in use by anthropologists), the magnitude of certain New World prehistoric urban settlements, and the extent of modification of New World ecology by Amerinds. Whatever the specific techniques used to estimate size, in the 1960's the mindset would be in the direction of larger magnitudes for the simple reason that knowledge of the impressive scope of Amerind settlement and centers is now available.

Another reason for underestimation is related to the reluctance of American prehistorians of this same period to attribute age to Amerind culture history. A kind of competition became established among archaeologists to see who could furnish the latest data for a given horizon. (When radiocarbon dates began to roll in after World War II, there was some embarrassment, but little examination of why the disposition toward recency took hold.) A mood of "realism," or perhaps a kind of humility, had infected Americanist anthropology, so long isolated in its museum of tribes; an unconscious sense of inferiority at studying vanished or vanishing and unimportant people became translated into a cheerful and bold confession, and then an exaggeration, of this unimportance.

A third reason may be that after three decades of triumphant culture-reconstructing, American ethnologists came to the conclusion that only they knew anything about American Indians. This helps account for the consistent derogation of highly qualified Spanish, English, and French observers. These people—not only lacking Ph.D.'s, but also cruel to the Indians—could not be put in the same category as highly trained modern ethnologists, deeply sympathetic with their objects of study. These attitudes radiate from monograph after monograph, article after article in the 1920's and '30's. They became a source of consistent bias which objective analysts like Dobyns and Borah have now overcome.

*by* IGNACIO BERNAL☆

*Mexico, D.F., Mexico.* 4 II 66

Dr. Dobyns has gathered and criticized with great exactness the different techniques used by various investigators to calculate either the total population of America before its discovery by Europeans or the population of a given area in that epoch.

Other studies exist which, perhaps because they deal with more local subjects, have not been considered by Dobyns. I mention this because it seems to me that, although studies of settlement patterns and agricultural techniques are only beginning, they make up another very important method with which to form an idea of past demography. I shall refer to only two of these, both in Mesoamerica.

Termer (1951) has tried to estimate the population density in the central and northern Maya areas by considering both the archaeological and the geographical aspects of these zones. According to Termer's calculations, the northern zone, the Peninsula of Yucatan, had about 500,000 inhabitants in an area of 135,000 square kilometers, which gives a population density of 3.7. He thinks that the central zone, with an area of 45,000 square kilometers, might have had 300,000 inhabitants, which gives a density of 6.6.

Sanders(1953), basing his conclusions on geographical aspects, agricultural possibilities, and data furnished mainly by the *Relaciones Geograficas de Nueva España* and later census figures, calculates that the density of population in what is today the central zone of Veracruz was 20 inhabitants per square kilometer, which is higher than the density generally proposed. Furthermore, he indicates (1956) that the density in the Valley of Mexico was even greater.

I do not pretend to express an opinion about these results, since I do not possess data that either corroborates or contradicts them. On the basis of a recent partial survey in the Valley of Oaxaca, however, I reached the conclusion that in the period from the end of the Classic Period up to the Conquest (approximately between the 7th and 16th centuries), there were more inhabited sites than there are present-day settlements, in spite of the fact that the population density at the present time is quite high. Of course, this is only a suggestion, as I have not yet been able to determine the size of the ancient sites and therefore can not calculate the possible number of inhabitants of each.

Any estimate which includes the entire continent must take as its starting point the tremendous demographic difference that must have existed between (1) the civilized areas of Mesoamerica and the Andes, (2) the regions that, although sedentary were not occupied by high cultures, and (3) the nomadic or almost uninhabited areas. This difference is clearly seen in Dobyns' Table 2, where the areas are divided geographically and the presence of these two centers of high culture is taken into account. Dobyns presents a tentative total here without indicating how he arrived at it.

On page 397, Dobyns refers to Nicholson's (1962) comment on "viewing the high cultures of Mesoamerica through North American ethnography-tinted glasses." Some scholars in the United States still place the Iroquois on the same level of social and cultural development as, for example, the Aztecs. This view, introduced by Morgan and developed by his at first reluctant pupil, Bandelier, is impossible to maintain today. As long as this is not clearly understood, it will be very difficult to make more or less precise calculations of population density in Mesoamerica.

Cook and Borah have discussed brilliantly the attribution of exaggeration to the writings of both the conquerors and the Spanish chroniclers of the 16th century. I think that today we are inclined to believe that there may have been exaggeration due to political motives or the desire to increase the prestige of the Conquest; but if there was such exaggeration, it was fairly modest, and the numbers given by the chronicles can at least be taken into account.

*by* OLEMAR BLASI☆

*Curitiba, Brazil.* 7 III 66

Dobyns' study suggests broad and deep considerations on the unanswerable question: What must have been the number of American aborigines at the beginning of the conquest?

As Dobyns demonstrates, there are many ways in which one can try to estimate this population, and the results are quite variable. His technique seems to me to be the most coherent one, with the objection that it could not, perhaps, be applied indiscrimately to all the culture areas of indigenous America.

The following considerations will better express my point of view:

Annual reports and other manuscripts of the priests of the Society of Jesus written between 1628 and 1635 say that in 23 catechistic missions (religious Indian villages) situated in two different territorial regions of southern Brazil, about 40,000 Indians had been *converted to Christianity* (Cortesão 1951; Porto 1943). Of these villages, 13 were in the Province of Guairá (the present state of Paraná) and 10 in the Eastern Missions of the Rio Uruguai (the present state of Rio Grande do Sul). The total area of these two regions of Jesuit influence is estimated at approximately 469,000 square kilometers. Admitting as accurate the total of Indians converted in the 23 villages, and presuming that the total number of Indians was double the number enrolled by the priests, the native population of the regions of Guairá and Rio Uruguai could be estimated, for the year 1635, at 80,000.

Considering also that between 1500 and 1635 there was reasonable depopulation, due to wars, plagues, intermarriage, etc., in the two areas and that this depopulation could have occurred in the proportion of 1:2 (the minimum index which can be assigned), it is reasonable to estimate a population of 160,000 for the two areas in the year 1500.

As the two regions indicated constitute portions of the *Marginal and Tropical Forest Areas* (Steward 1949), whose populations depend upon collecting, hunting, and fishing, and incipient agriculture, and since according to Steward all of present-day Brazil lies within these two areas, the estimates being considered could also be valid for all of Brazil. In this case, given that the area of Brazil is about 8,500,000 square kilometers (18.4 times the size of the regions of Guariá and Rio Uruguai), approximately 2,944,000 Indians could have been accomodated in it in 1500. (Applying these same calculations to the total area of the Marginal and Tropical Forest Region in South America, estimates at 12,000,000 square kilometers, its aboriginal population at the beginning of the conquest could have been about 4,160,000.)

Finally, assuming that native depopulation in Brazil occurred in the proportion of 1:2, and taking as the base figure our estimate for the year 1500, the demographic decline in Brazil could have been the following:

| | |
|---|---|
| 1500 . . . . . | 2,944,000 |
| 1635 . . . . . | 1,472,000 |
| 1770 . . . . . | 736,000 |
| 1905 . . . . . | 368,000 |
| 1965 . . . . . | 163,000 |

The 163,000 Indians estimated for 1965 almost coincides with the estimate published by the Indian Protective Service of Brazil (Ribeiro 1957) of about 100,000 (97,700 ± 10%) Indians remaining in 1957.

In conclusion, Dobyns' ratios of 1:20 and 1:25 can be applied if one admits the premise that the sedentary life of the Andean region greatly favored demographic growth. Accepting the converse, however, that is, that nomadic life is unfavorable for population growth (Childe 1954:89) these proportions appear to me inadvisable for the populations of the Marginal and Tropical Forest Areas. For these two areas, I find the proportion of 1:2 more reasonable, especially in view of the relative numbers of remaining aborigines in the Marginal and Tropical Forest Areas and the Andean Area found in the 1960 surveys of the Instituto Indigenista Interamericano (*Boletim Indigenista* 1961).

*by* SHERBURNE F. COOK☆

*Pacific Grove, Calif., U.S.A.* 25 II 66

Dobyns' paper is an interesting commentary upon the nature of evidence in the borderline field between natural and social science, in particular the quantitative data which lie at the foundation of all sensible estimates of population.

In other than a strictly contemporary context, we have to discard the legalistic concept of evidence, which is limited to something actually seen or heard by a witness testifying before a court and subject to cross-examination. For the historian and the scientist, evidence must include what was seen or heard and then written about by witnesses who are dead and who therefore can not be put under oath by the clerk. This principle was grudgingly conceded a decade ago by the commissioners who conducted the hearings in the various claims cases brought by American Indian tribes against the federal government. Thus, for the California case, what happened in 1850 could be described only by someone who lived in 1850. Consequently, evidence gathered at the time was admitted as legally acceptable. The question then becomes: How good is evidence recorded in the period of a past event?

Dobyns states that Kroeber, Rosenblat, and their followers have taken a position which rests upon a "depreciation of all historical population figures" (p. 394). Assuming this statement to be correct in general (for there are exceptions), one may inquire why it should be true. Two factors are usually adduced in answer: (1) the ordinary, unavoidable human error of estimate, and (2) falsification by deliberation or by negligence.

Error in enumeration may acquire substantial proportions if the reporter is careless, or if the society is of too great extent to be encompassed physically. However, if the reporter is competent and the field sufficiently restricted to permit adequate observation, the inaccuracy due to miscount should not be fatal. For example, deviation from the true value of ± 50% is minor compared with the range of estimates of aboriginal populations, which may extend from 100% to 1,000% of the minimum. On the other hand, an error of ± 50% would be regarded as intolerable by a demographer who works with modern censuses. Thus Grauman (1959:559) presents a sweeping, uncompromising indictment of all of the direct, relatively well-controlled population estimates employed by anthropologists.

It is not, however, the errors of computation which have been most bitterly denounced by historians and ethnographers, but the warping influence of motivation. It is argued that field estimates are invariably subject to wild exaggeration for reasons of personal advantage, profit, or glory. Most sensitive to these objections are the reports of the clergy and the military. Conceding the human desire for exploitation of personal achievement, we have to ask ourselves first whether a shameless propensity for gross mendacity is characteristic of all individuals, even the most responsible and intelligent clergymen, commanders, and administrators, and whether a complete, uncritical repudiation of their statements is justified.

Secondly, we have to take into account the controls under which such men always operate. Every priest, every soldier, every official, every agent of a commercial enterprise, has always been accountable to some higher authority, and woe unto him who deliberately confounds policy by submitting false reports. Indeed, with the exception of a few egomaniacs, the great majority of observers, historically, have had strong incentive *not* to exaggerate. Moreover, to maintain that a type of unidirectional misinformation has existed over centuries is to argue for an incredibly perfect conspiracy, one which has never been detected by some of the most powerful ecclesiastical and civil organizations the world has ever seen.

This last consideration points to our best built-in safeguard for reasonable accuracy in estimating populations, that is, the multiplicity of sources which often can be brought to bear. Thus if numerical values can be derived from direct estimates, even if these have to be projected in order to obtain a total for a large area, they can be

combined with other types of information. Among the latter are house counts, data on family organization and settlement patterns, fiscal and property surveys, reports on agriculture, food supply, and carrying capacity of the land—in short, the whole range of social and economic activity in a human aggregate. Then if a picture emerges which is reasonably coherent and consistent, the magnitude of any single parameter, such as population level, can be accepted with confidence. Indeed it must be so accepted if our understanding of extinct, or even contemporary, societies is to be permitted to advance.

It is Dobyns' contribution that he has marshalled the facts and attempted a classification of those methods whereby such a synthesis of sources pertaining to the aboriginal Western Hemisphere might be initiated. Regardless of numerous details which may be subject to criticism, he has emphasized a mode of thinking which eliminates the preconceptions and prejudices of an earlier generation and opens the way to a truly scientific approach to historical demography.

With regard to Thompson's paper, I would like to discuss a few rather restricted points:

1) Perhaps because my mathematical background is very modest, I find myself somewhat at a loss to understand the use of the basic equation (1). Wholly disregarding the ecological parameters which are represented by the terms, and thinking only algebraically, it would appear that $N_E$, which occurs on both sides of the equation, should drop out. Indeed, in equations 7 to 9 (p. 420), Thompson first substitutes the value 750,000 for $N_E$ and then eliminates it. If this is the end result, one may ask why the term is there in the first place.

Another puzzling feature is the role of X, which is defined as the unknown function of human population size. In the expression:

$$\left(\frac{N_{DH}}{X}\right) X,$$

if X is to have a definite, fixed value in any solution of the equation then it must here cancel out, for there is nothing to indicate a lack of identity. The equation then reduces to

$$N_B - (K + 1) N_{DH} = O.$$

If, as on line 2 of page 417, is arbitrarily given the value of 1, then

$$N_B - 2N_{DH} = O.$$

Since $N_B$ represents the number of animals born and $N_{DH}$ the number dying of non-human causes, this formulation leads to a very dubious conclusion. The author gets around the difficulty by substituting assumed coefficients H and F, so that

$$\left(\frac{N_{DH}}{X}\right) = \left(\frac{N_{DH}}{X}\right) H + \left(\frac{N_{DH}}{X}\right) F.$$

If this is proper, it would have been more logical in the first place to have written the expression as

$$\left(\frac{N_{DH}}{X}\right) Z,$$

where Z is *not* the unknown, X, but is any coefficient or group of coefficients derived from the data. In any case, both the notation and the development of the equations are confusing and ought to be simplified and clarified.

2) I question whether the population figures obtained by the author refer to truly aboriginal conditions. In footnote 2 on page 414 Thompson places "first contact" of the Chipewyans with the white man at *ca.* 1770, because at this point "we first have reliable, relatively detailed accounts of the Chipewyan"; but he has just finished stating that Mooney set the date at 1670, when the Hudson's Bay Company was established. This seems to be a quite doubtful interpretation of *first* contact, although it is entirely in conformity with Thompson's rather ill-considered rejection of the "accounts of early travellers" as being substantially useless for sophisticated statistical treatment. If the Hudson's Bay Company began to operate in Chipewyan territory in 1670, the woods would have been full of hunters, trappers, traders, and explorers by 1770, and indeed we know that this was actually the situation. MacNeish is quoted in the same footnote as stating that "all reports show the effect of culture contact," but the implications of this statement are thrust aside. There can be little doubt that during the 100 years between 1670 and 1770 this tribe was subject to the usual disruption of native society and economy, together with the almost certain exposure to introduced diseases. Consequently Thompson's population figures on page 420 (e.g., a mean probable value of 6,426) refer to a period more than a century post-contact. The true pre-contact population must be regarded as having been considerably higher.

3) The author analyzes (p. 415) the effect of the smallpox epidemic of 1781, only a decade after his date of first contact. If such a devastating epidemic of smallpox occurred, at this late date it is highly probable that there had previously been other epidemics. Hence a population of 2,000 at or just before 1781 would have been purely residual.

This figure of 2,000, furthermore, is based upon the assumption that the population increased from 200 to 400 in the 20 years from 1781 to 1801. Such a recovery is contrary to all human experience, regardless of whether or not the environment is unlimited. Even under the optimum conditions of the modern "population explosion" the inhabitants of such countries as those of Latin America have not doubled in number in 20 years. The Chipewyan had just undergone a terribly traumatic setback, with profound disintegration of social organization and economy. Disease was still present, for an epidemic does not come to an abrupt and total conclusion. The remnant of the people must have been highly susceptible to all manner of secondary or endemic infections. Moreover, in all the history of epidemics there is no authentic, recorded instance in which a population has demonstrated a recovery of the type implied here. Hence, if the population was 400 in 1801, it is likely that it was at least 400, or, allowing for reasonable recovery, 350, in 1781. Then, with a 90% reduction due to smallpox, the pre-1781 population would have been 3,500, a figure still below the author's calculated aboriginal minimum.

4) Thompson estimates (p. 419) "an average intake of 4,000 calories per day per person" for the Chipewyan. In modern civilized communities, 4,000 calories per day is regarded as required only by an adult engaged in moderate to strenuous physical labor. For a sedentary person, a small person, or a child, the adequate intake is much less. Since the active, adult men are in a distinct minority, the *average* daily caloric intake will not exceed 3,000. In many parts of the world, such as the Orient and Africa, huge populations exist on no more than 2,000 calories a day and there is good reason to believe that groups of Indians in North and Mesoamerica with relatively copious food supply consumed not much more than 1,800 calories per person per day. The subject is too extensive to warrant discussion in detail here, but it can be stated that to assume 4,000 as the *average* actual daily intake for the Chipewyan is contrary to established nutritional doctrine. It would require that the author of this paper adduce very strong and concrete data in order to support his thesis.

5) In general, it is not my desire to point out minor flaws in what has the potential for a very valuable contribution. As one professional ecologist to whom I showed the article remarked, "It was a good try." I do wish, however, to suggest that the work rests upon a series of rather doubtful assumptions and is characterized by a certain degree of carelessness in manner of presentation. I hope the author will continue to pursue this category of investigation. but with increased attention to the accumulated store of

human knowledge and a reduced emphasis upon mathematical formulation for its own sake alone.

*by* WILLIAM M. DENEVAN☆

*Madison, Wis., U.S.A.* 25 II 66

Henry Dobyns is to be complimented for providing a much needed critical summary of previous research and opinion on the size of pre-conquest New World populations. The general lack of agreement on how many Indians were in the hemisphere is now more apparent than ever; however, Dobyns has presented a good case against the extremely low estimates of Kroeber, Rosenblat, and others. I suspect that eventually a fairly solid figure somewhere between 50,000,000 and 100,000,000 will be arrived at, as has been suggested by Spinden (1928), Borah (1962*b*), and now Dobyns.

While Dobyns' total population figure of 90,000,000 may not be unreasonable, his rough rule of thumb for arriving at it, an average 20:1 ratio of depopulation from contact to nadir, is not satisfactory for many specific regions because, as Dobyns has clearly indicated, the actual ratio did vary considerably from region to region. Some declines were much greater than 20:1 and some seem to have been much less, and if major regions diverged significantly from the average, then the final total might differ considerably.

The most questionable figure derived by Dobyns' 20:1 ratio is 30,000,000 for the Andean Civilization. (It is not clear whether northern South America is included here or with Marginal South America.) Dobyns' main support for his 20:1 ratio for the Inca Empire is Rowe's depopulation ratios of 16:1 and 25:1 for the Provinces of Rimac and Chincha between 1525 and 1571 (Rowe 1946:184-85), ratios Dobyns rightly considers low for the depopulation from contact to the actual nadir, which was probably sometime after 1571. However, Dobyns does not mention that Rowe gives depopulation ratios of 3:2, 3:1, and 4:3 for three other Peruvian provinces during the same period and, most important, obtains an average of 4:1 for the 5 provinces. (Rowe's ratio of 4:1 is mentioned earlier by Dobyns in a different context.) On the basis of the 4:1 ratio and using the 1571 population total for the Central Andes of 1,500,000 (which is the same as Dobyns' 1650 + nadir figure), Rowe arrives at a 1525 population of 6,000,000 for the Central Andes. Granting that the three provinces with low depopulation ratios may have been considerably devastated after 1571, we are still left without a reliable basis for a depopulation ratio for the Central Andes. A compromise of 12:1 midway between Rowe's 4:1 and Dobyns' 20:1 would provide a figure of 18,000,000, which is still higher than any previous estimate except that of Means (1931:296). Significantly, it is about the same as the present Andean and Coastal population of Bolivia, Peru, and Ecuador. It is doubtful if aboriginal land use in the Andes could have supported a population much greater than that of the present, much of which is barely subsisting, even though some of the land that was in production in pre-Columbian times is now used for export products or is not used at all.

While a 20:1 ratio is probably too high for the Central Andes, it is undoubtedly too low for most of the tropical lowlands of the Americas. Dobyns points out that the ratio ranges up to several hundred to one or virtual or actual extinction of once sizable tropical forest tribes. Dobyns is forced to abandon his 20:1 ratio, lacking any total nadir figure, but does arrive at a rough estimate of 6,000,000 for tropical South America. My own work in eastern Bolivia and Peru suggests that this figure is not unreasonable, and is perhaps low by several million. For the tribes of the Llanos de Mojos of northeastern Bolivia, I found good documentary evidence for a population of about 100,000 in 1690, 110 years after initial contact, in contrast to a nadir of about 10,000 in 1900. The over-all depopulation ratio from contact to nadir was probably at least 35:1 (Denevan n.d.), and I would expect a comparable minimum ratio throughout most of the Amazon Basin where there has been fairly continuous contact. I think that such a drastic decline will eventually be substantiated by archaeology, documentary studies, and closer examination of tropical population densities under present aboriginal economic systems.

A 20:1 depopulation ratio for the tropical Caribbean Islands, giving a total of 443,000, also seems very low in view of historical reports of several million people. The early chroniclers' accounts of large populations in Central Mexico have been vindicated. Were some of the same sources so far off in their estimates for the Caribbean (Sauer 1966:65-69)?

The main point that I wish to make is that if one is going to try to arrive at a hemispheric aboriginal population by means of depopulation ratios, then consideration should be given to the use of several different average ratios for different types of situations or regions—an objective admittedly "beyond the scope" of Dobyns' paper. To what extent and why have depopulation ratios varied? Certainly the nature of the contact situation would seem to be significant. Disease, for example, probably spread much more rapidly, thus reducing the time available for acquiring immunity, in the compactly settled Valley of Mexico than in widely separated Andean valleys or among dispersed Amazon tribes where initial contact was sporadic. Or did a lesser degree of contact simply retard the date of an inevitable population nadir? On the other hand, there may have been important enviromental differences in the susceptibility of the natives to, or in the distribution of, fatal epidemic diseases. Is there a tendency toward more virulent disease in the humid tropical lowlands than in temperate and cold latitudes and altitudes? There are apparent major regional differences in depopulation ratios in the New World—probably 12:1 or less in the Andes, 15 to 25:1 in Central Mexico and California, and 35:1 or more in the tropical lowlands—and I emphasize the need for research to futher delimit and explain these differences and thus refine the depopulation ratio technique of estimating aboriginal American populations.

*by* HENRY F. DOBYNS☆

*Ithaca, N.Y., U.S.A.* 24 II 66

Thompson reports what impresses me as an imaginative approach to the difficult problem of estimating aboriginal populations. The article constitutes, I believe, a distinct advance in methodology for the resource-potential method of estimating human populations.

In one respect only would I differ somewhat from Thompson, and that is in thinking that his results throw more doubt upon Mooney's Chipewyan population figure than Thompson modestly claims. Thompson's calculations yield a minimum population estimate that is ⅓ larger than Mooney's estimate of the same population. His maximum estimate, and the one more convincing to me, is very nearly three times as large.

Thompson's calculations seem to me relatively conservative in that they are based on a single food resource, the barren-ground caribou. He dismisses woodland caribou, moose, bison, musk-oxen, and fish as a source of 5%—10% error, although he himself recognizes that the availability of fish for winter food possibly constituted the principal limiting factor on population. Several other types of food were utilized by the Chipewyan population, so I doubt that all wild plant foods collected plus the alternative sources of meat amounted to so small a proportion of the total diet as Thompson asserts.

*by* HAROLD E. DRIVER☆

*Bloomington, Ind., U.S.A.* 11 II 66

Dobyns' review is the best general paper of its length so far on aboriginal American population: (1) It shows how few solid facts on early populations have survived in the documents. (2) It gives ample evidence of the enormous differences in population estimates by everyone from early 16th-century eyewitnesses to 20th-century scholars. (3) It suggests that the estimates of Kroeber, Rosenblat, and others were much too low. (4) It shows a steady increase in population estimates from Kroeber's (1939) scheme to the present. (5) It concludes with the highest estimate for the hemisphere so far made.

While Dobyns' conclusions are important, what is more important to me is that he has made explicit a greater variety of methods used to estimate population than has anyone else so far. When these methods are compared with the generally accepted rules of statistical inference, they fall far short of the mark. Samples are rarely chosen in any systematic way, because the early observers and chroniclers knew nothing about sampling as we know it today and had no incentive to be more explicit. They often gave estimates based on hearsay rather than direct observation or systematic tabulation of vital statistics. Nevertheless, the documentary research of Cook, Simpson, and Borah in Mexico is based on unusually large samples of highly particularistic data and is a model of critical handling of materials.

Dobyns' (Table 2) nadir population for the U.S.A., taken from Rosenblat (1945:22), is 332,397 in 1930. This is much higher than my estimate of 200,000 in 1900 (Driver 1961:604) and Kenneth Stewart's 250,000 in 1920 (Spencer and Jennings 1965:500). I would also prefer the term Anglo-America for North America north of Mexico, although I am aware that everyone in Latin America calls it North America, as does Dobyns.

Thompson's article presents a sophisticated formula for estimating the maximum human population that a given area can support at the culture level of the particular population within the time span chosen for study. This is undoubtedly better than more impressionistic methods, but since the values substituted in the formula are estimates, or are based on small samples of observations, the final result is still subject to considerable error.

Population estimates would be easier to evaluate if all estimators would include an estimate of their error, offering, for example, a figure for the hemisphere of 100,000,000 ± 30%. If the amount of error is regarded as one unit of standard deviation from the estimate, it would undoubtedly be smaller than ± 30% (the range) and might be of the order of ± 10%. Anyone bold enough to estimate aboriginal population ought to be bold enough to estimate his own error.

*by* FREDERICK L. DUNN☆

*San Francisco, Calif., U.S.A.* 24 II 66

These papers on techniques in aboriginal population estimation offer striking methodological contrasts. Thompson favors a mathematical approach in ecosystem analysis and relies upon projection of modern data (for caribou) backwards in time to arrive, indirectly, at a human population estimate. Dobyns, after providing us with a valuable review of estimation techniques, concludes that projection and dead reckoning methods are "seriously defective." He finds ethnohistorical methods less deficient, particularly when secular trends can be determined by comparing populations in given areas at several points in time.

Although I am not in a position to comment on the population estimates offered by Dobyns in the concluding section of his paper, I find his arguments reasonable or at least plausible in most respects and applaud his concern with disease as a factor to be considered in population estimation. In the space available I shall be able to focus on only one or two of many epidemiological issues raised in his appraisal. As a general comment, I do not feel that Dobyns' discussion of disease provides the reader with full awareness of the complexities and problems that exist in analysis of the behavior of disease in human populations. The problems are many-times compounded, of course, when the historical record must be interpreted. His review of techniques would have been even more valuable if more space had been devoted to discussion of the kinds of limitations one may encounter in perusing the historical-epidemiological literature. Malaria, for example, appears in the early American literature as "fever and ague" or "intermittent fever." It is usually possible to identify malaria as the responsible disease (for example, see Cook 1955*a*), but it is far more difficult to determine in the earlier New World accounts which of the several species of *Plasmodium* might have been responsible for a particular outbreak. *P. falciparum*, *P. vivax*, and *P. malariae* are epidemiologically distinct and differ profoundly in their morbidity and mortality impact. Without knowledge of the parasite species, many of our guesses about the impact of malaria in a given region in the past are certain to be spurious. Accounts of smallpox epidemics in the past may be even more unreliable if we seek to project the mortality experience in local epidemics on populations of larger regions. Smallpox viruses are apparently subject to strain variation, and mortality rates between epidemics have been noted since at least the 17th century (Greenwood 1935). In addition, smallpox viruses today belong to two groups: "variola major" and "variola minor," the latter with case-fatality rates in the 5% range. We do not know whether these groups existed in the 16th century. Dixon (1962) notes that early statistics are often meaningless because chickenpox was reported as smallpox. Even measles and secondary syphilis were probably often recorded as smallpox three centuries ago.

Dobyns states that "vaccination has been, in all probability, the major single factor in New World native population recovery since 1800." This leads him to the inference that Kroeber's choice of 1793 population figures as a base for backward projection was a "fatal error"—which may be overstating the case a little. I would challenge his statement about vaccination, on several grounds. First, it is malaria that has been the most important endemic (and epidemic) disease in the New World for at least several centuries, and we may speculate that acquired immunity to malaria, and perhaps quinine, together represent that "major single factor." The full force of smallpox in the Americas was felt long before the time of Jenner, but after 1800 smallpox continued to prevail in Europe for many decades, in part because of opposition to vaccination, and introductions of the virus from across the Atlantic continued throughout the 19th century. Smallpox, in contrast to malaria, appears to have occurred largely in sporadic epidemics and outbreaks, particularly at coastal points of reintroduction. It seems not to have become an entrenched, self-sustaining endemic infection in large areas. 19th-century vaccinating efforts in the Americas appear also to have been deficient, for many reasons: loss of vaccine potency, failure to reach substantial segments of the population, failure to recognize the need for regular revaccination, etc.

I find that I cannot wholly agree with Thompson's opening statements about factors limiting population size. Important though Thompson's three factors may be, they are certainly not the only "primary" ones championed today by workers in the mammalian population field. It is in fact the existence of an array of interplaying factors—primary, secondary, and otherwise—that makes the study of population limits and controls so challenging. Because factors affecting

population size cannot, in my opinion, be reduced to such a simplistic statement (or at least to this one), I find Thompson's presentation rather unrewarding except as an interesting technical exercise. Proceeding farther, I find myself troubled by his definition of first contact—even for the purposes of his paper—as the time (*ca.* 1770) "when we first have reliable, relatively detailed accounts of the Chipewyan." This is perhaps reasonable if the only issue is the establishment of an effective date of "culture contact," but contact with new infectious disease agents and strains (influenza, measles, tuberculosis, etc.) must have occurred much earlier, and certainly by the 1670's when the Hudson's Bay Company was established. I think we must assume that the impact of introduced diseases on the Chipewyan had already affected population size in the 17th century, if not earlier.

Without in any way faulting the mathematical techniques used, I find the maximum likelihood estimates given in the conclusion unconvincing, for this exercise in estimation depends entirely upon the accuracy of another estimate: that for the aboriginal caribou population. There is no inherent reason why man or caribou, in aboriginal times, should have been more or less adapted to conditions in that particular ecosystem. This suggests to me that one man's estimate-by-projection for the aboriginal caribou population cannot be accepted as necessarily more reliable than another man's guess for the aboriginal human population. In this exercise everything depends on $N_E$, the equilibrium number of caribou, and $N_E$ as used seems a precarious figure indeed. First, it is based on a single estimate (Banfield 1954) whose confidence limits are not given. It is based secondarily on a series of assumptions about the area of aboriginal Chipewyan territory, the extent of overlapping of aboriginal caribou and Chipewyan territorial areas, and the even disperal of caribou and Indians within these territories. Other critical values, for $N_B$ and $N_{DH}$, are of course based upon the $N_E$ estimate and an additional series of assumptions about aboriginal conditions (for example, that the wolf-caribou predation rate was the same in aboriginal times as it is today). It may be, and probably is, purely coincidental that Thompson's estimate agrees fairly well with Mooney's.

*by* MALCOLM F. FARMER☆

*Whittier, Calif., U.S.A.* 25 II 66

Studies of the demography of aboriginal peoples have been neglected and are now needed in all branches of anthropology. Thus the papers by Dobyns and Thompson are timely and useful.

The method suggested by Dobyns, assuming a "standard" depopulation ratio of 20:1 between initial European contact and the beginning of population recovery, is a useful approach. It depends on the determination of the nadirs for the ethnic groups. This is particularly useful for estimations of populations for large geographic areas but appears to have some disadvantages when applied to particular ethnic groups. Each group has a demographic history which must be detailed as completely as possible. The history of the Navaho, for example, suggests some of the problems. Some years ago, as a part of research on the Navaho land claim, I searched the literature for estimates and counts of Navaho and found considerable variation. When all of the figures were plotted on a chart, the increase after A.D. 1900 was clear, and in fact estimates and counts made as early as A.D. 1845 suggested an expansion trend. The official figures for A.D. 1863, the time of the Navaho round-up, suggested that not all of the people had been captured, and the rapid increase in the years after the treaty of 1868 probably included the influence of these previously unenumerated people. Estimates for the years before A.D. 1845 tended to be low, if the Navaho expansion of the period is considered and archaeological data used as a check on the estimates. Benavides' estimate for 1630 of 30,000 Navaho appears large, but no doubt included not only Navaho but also other Apacheans on the upper Chama River and in that immediate vicinity. The later data suggest that the Navaho population curve was on an increasing trend in the late 1700's and early 1800's, a reflection of the development of herding. It appears that the Navaho may have had an increasing rate of population growth ever since they became agricultural. The population increase among hunting peoples of the Plains area of North America of the period after A.D. 1650 also shows the need for detailed study of the history of the particular group.

The paper by Thompson suggests the potential of a specialized approach which can be applied in an area where anthropological and biological data is available. Further development of such methods is certainly in order. Computer techniques can make a major contribution.

The earlier demographic work of L. Krzywicki (1934), and A. M. Carr-Saunders (1936), should be noted. Demographic research by physical anthropologists is surprisingly rare, and aboriginal and non-literate peoples need demographic treatment. Perhaps these papers will suggest a revival.

*by* R. G. FORBIS☆

*Calgary, Canada.* 21 II 66

If Dobyns has done nothing else, he has shown that attempts to estimate aboriginal American population in any absolute sense range from risky to hazardous. His depopulation ratio method strikes me as totally unconvincing, merely resulting in estimates based upon other estimates. While Thompson's mathematical method of calculating population size on the basis of available natural resources follows a generally promising line of research, its usefulness would seem to be effectively limited to historical tribes who are not primary food-producers. Even then, it serves only to indicate probable upper and lower limits to group size:

If population density is regarded as a social and cultural dynamic (this would appear to be Dobyns' main reason for estimating it), then it is clearly his obligation to demonstrate at what points density triggers social and cultural responses. Trigger points can obviously not be established with precision for any area where population estimates are as widely variable as they are for the Americas. To be significant, these densities would have to be determined from known populations; working from the known to the unknown, they might then be applied with some profit to the aboriginal New World.

If population density is regarded as a social and cultural dynamic, I can see little reason for concern with hemispheric population estimates. It is of some intellectual interest to establish relative population densities for each major region of the Americas, but it is, after all, among specific social groups that population density comes into play. Hemispheric estimates avoid the basic problem by lumping groups of vastly variable complexity and size.

Incidentally, I find it very hard to swallow the idea that population is an independent variable, if "independent" means free of culture.

At any rate, unless radically improved methods supersede the old, estimations of New World population will necessarily prove relative, as Kroeber (1939:181) was well aware. As such, they can clearly only be used relatively in studies of the effect of population density on social and cultural change.

*by* HELMUTH FUCHS☆

*Washington, D.C., U.S.A.* 28 II 66

It is by no means astonishing that a discussion on the estimation of aboriginal population should arise. Wherever exact data are unavailable, speculation is assumed to be appro-

TABLE 1

ESTIMATES OF PANARE POPULATION

| ANTOLÍNEZ (1944) | CENSUS (1950) | | | | RILEY (1953) | | | WILBERT (1959) | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | ♂♂ | ♀♀ | Σ | | Σ | | | ♂♂ | ♀♀ | Σ | |
| La Cañada | — | | | | — | — | | — | — | — | — | |
| El Loro | — | | | | — | — | | — | — | — | — | |
| Guaratero | — | | | | Guarataro | 50–75 | | Guarataro I | 46 | 41 | 87 | |
| | | | | | | | | Guarataro II | 12 | 13 | 25 | |
| — | — | | | | — | — | | — | — | — | — | |
| Parichima | — | | | | — | — | | — | — | — | — | |
| Las Rayas | — | | | | La Raya | 50–100 | | — | — | — | — | |
| El Hacha | — | | | | — | — | | — | — | — | — | |
| El Limón | — | | | | — | — | | — | — | — | — | |
| Colorado | Colorado | | | 55 | Colorado | 100–125 | | Colorado | — | — | — | |
| Los Guaros | — | | | | — | — | | — | — | — | — | |
| — | Guaratarito | | | 38 | Guaratarito, Camatagua, San Agustín | 100–200 | | Guaratarito | 14 | 29 | 43 | |
| Subida del Piriteño | — | | | | — | — | | — | 7 | 4 | 11 | |
| — | — | | | | Piñaguero | 23 | | Piñaguera | — | — | — | |
| Montañas de la Culebra | — | | | | — | — | | — | 14 | 15 | 29 | |
| — | — | | | | — | — | | Montaña | — | — | — | |
| — | — | | | | Piñal | 45 | | Piñal | — | — | — | |
| — | El Tigre | | | 53 | — | — | | — | | | | |
| — | — | | | | Uroné | 25–50 | | — | — | — | — | |
| — | — | | | | Raudal Alto | 50–100 | | Raudal Alto | — | — | — | |
| — | — | | | | Las Ponchas | 25 | | Las Ponchas | — | — | — | |
| — | — | | | | — | — | | Zariapo | 21 | 16 | 37 | |
| TOTALS | | 76 | 70 | 146 | | 468–743 | | | 114 | 118 | 232 | |

NORTH GROUP

SOUTH GROUP

priate, and it is left to the reader to accept or to refute the results. Another question, perhaps of more importance at present, is the reliability of recent census data on aboriginal population. Among the Panare Indians of Venezuela, for instance, various counts have been made and published, but it is difficult to identify communities and compare the populations counted or estimated therein (Fuchs 1959, 1964). Table 1 shows in column 1 the dwelling sites of the Panare according to Antolínez (1944); no population was estimated. Column 2 shows the Census figures (1950). (The 1960 Census is still in preparation.) Colum 3 gives counts or estimated ranges by Riley (1953), and column 4 shows Wilbert's data (1959). Cruxent (1948) estimates Panare population at 1,800, while the Census (1950) gives 146, Riley a range of 468 to 743, and Wilbert 232. The question remains: How many Panare are there, really?

Perhaps anthropologists should insist on an improvement of census methods in order to obtain reliable data for the present-day aboriginal population. Estimates based on a 20:1 ratio (Dobyns) or a range of almost $\frac{n}{2}$ to almost $2n$, $n$ being the probable maximum (Thompson), do not differ much from what we already know. Evidently there has been a lack of basis for counting or reliably estimating aboriginal population in the past as well as

in the present. It is the intention of this comment to direct our interest towards this problem and to stimulate the creation of such a basis.

*by* ALEXANDER HÄUSLER☆

*Halle/Saale, DDR.* 23 II 66

Dobyns and Thompson have doubtless made substantial contributions to the reconstruction of a picture of past population. Another palaeodemographic procedure which might be considered is that of Ascady and Nemeskeri (1957), which is based on the anthropological interprepation of skeletons from completely excavated cemeteries. In using this method, it must be taken into consideration, of course, that many American Indian tribes did not bury infants in the same cemeteries as adults. Furthermore, in a great many investigations some of the female skeletons have been interpreted as male because of the unrecognized high masculinity of the entire population (Häusler 1965), apparent, e.g., in the Indian settlement of Pecos (Howell 1960:173). When these sources of error are taken into account, the method produces relatively exact data which should then be compared with an estimate of the population of a small area. It would have been useful if Dobyns and Thompson had said more about the numerical proportions of the sexes.

*by* WILLIAM A. HAVILAND☆

*Burlington, Vt., U.S.A.* 4 II 66

Dobyns' review of techniques for the estimation of aboriginal American populations and Thompson's presentation of a new technique are timely and helpful contributions to this problem area. In the past, population estimations have had considerable effect on anthropological reconstructions of various pre-Columbian American cultures. For example, in the Lowland Maya area (with which I am most familiar), much of the confusion which exists as to the nature of aboriginal sociopolitical organization stems from widely disparate views on the size of the Maya population at any given time. If we can develop our methodology so as to achieve reasonable estimations of aboriginal populations, then we will be in a better position to attack the larger problems such as sociopolitical organization.

Dobyns has discussed at some length Ricketson's population estimate for prehistoric Yucatan. Among the other workers who have addressed themselves to the problem of the size of the ancient lowland Maya populations, are Bullard (1960), Cowgill (1962), Haviland (1965), Sanders (1960, 1963), Smith (1962), Termer (1951), J. E. S. Thompson (1951), and Willey, Bullard, Glass, and Gifford (1965). All except Cowgill and Thompson (in the instance cited) have taken an archaeological approach to the problem, as did Ricketson. These sources, and others more indirectly concerned with the problem, are reviewed in two forthcoming papers (Haviland 1966*a*, *b*).

The impression I have after reading the present two articles is that we must approach the problem of population estimation first on a local basis. An over-all hemispheric estimation, then, will be only as accurate as the several regional estimations on which it is based. As for methodology, it is my feeling that methods will have to vary according to area. Thompson's method, which may work very well for hunting and gathering groups, would I think be difficult to apply in the case of the Maya. On the other hand, archaeological research may provide the basis of a reliable population estimation for the Maya, who were sedentary, but not for a nomadic hunting and gathering group. In the lowland Maya area, one can map ruins over given areas and determine through excavation how many were in use contemporaneously and what percentage of these were in fact houses, as opposed to domestic adjuncts, temples, and other structures. This has been done at all thoroughly only at the Post-Classic site of Mayapan, Yucatan (Smith 1962), and at the Classic Maya site of Tikal, in northern Guatemala (Haviland 1965). To take the latter site as an example, most houses were comparable in size and arrangement to modern Maya houses, so that *in this particular instance* it seemed reasonable to project the figure of 5.6 persons per house, the present average for Maya communities in Yucatan, backwards in time to Tikal. In this way, I derived a minimum estimation of 10,000-11,000 persons for the population of Tikal about A.D. 700-900, which would represent a time of peak population in the Maya area. This estimation does not allow for the possibility that many of the so-called palaces at Tikal were residences of the elite, nor for the possibility that Tikal was actually larger than the 16 square kilometers which has so far been mapped. Further research, therefore, will permit the refinement of this population estimation.

The present Tikal estimation represents a population density on the order of 800-900 persons per square kilometer (1,000-1,100 persons per square kilometer if allowance is made for uninhabitable terrain). This compares with Ricketson's figure for Uaxactun of only 104.6 persons per square kilometer of habitable land. Even had Ricketson calculated on the basis of 5.6 persons per house instead of 5, and had he not eliminated 75% of his sample, the Uaxactun density still would not equal that of Tikal. Therefore, the full Uaxactun figure may not be as illogical as Dobyns (p. 397) seems to feel.

Cowgill (1962) has approached the problem of the size of the aboriginal Maya population from the standpoint of carrying capacity of the land, and suggests that a population density on the order of 38-77 persons per square kilometer was possible. That this is probably too low an estimate is suggested by thorough reconnaissance of 2.5 square kilometer located more than 7 kilometers from the center of Tikal, where population density appears to have been on the order of 136 persons per square kilometer. Cowgill's study was based on modern milpa practices, which may differ somewhat from aboriginal practices, and she does not take into account the probability that the ancient Maya grew crops other than corn. This illustrates the problem of a straight ecological approach to population in this area.

To sum up the situation with respect to the Maya, it would appear that current research will soon make possible an improved estimation of aboriginal population for this area as a whole. As this is done in other areas, by whatever means seem appropriate, then we will have a more realistic estimation for the hemisphere as a whole. I agree with Dobyns that past estimations will generally prove to have been too low.

*by* EUSEBIO DÁVALOS HURTADO☆

*Mexico, D.F., Mexico.* 28 II 66

One of the merits of Dobyns' work is the fact that, in spite of the brevity of the exposition, it presents and discusses all the published calculations and numerical estimates of the prehispanic Indian population. The simple fact of summarizing the technique and results of demographic extrapolations contained in an extensive and heterogeneous bibliography gives the study unquestionable usefulness and value.

For Dobyns, establishing exactly the amount of a society's population is not an end in itself; his opinion is that the density of population is an index of cultural development. He supports this viewpoint by quoting the concepts of Melvin Ember, Kingsley Davis, Katharine Organski, and others.

Dobyns quotes Vaillant's statement that in the Valley of Mexico there did not exist a large human concentration like the one which developed in 1936-40 in Italy, Germany, and Japan, nations which went to war maintaining

that they were overpopulated and needed additional territory. Dobyns believes that no real demographic pressure was manifested in Mexican territories, but he is inclined to think that the size of the indigenous population has been underestimated.

The figures assigned to the different geographic and ethnic areas that constitute the American continent are discussed, and, what is extremely important, it is shown that interest in this subject has not declined with the passing of time. First broached by the chroniclers and missionaries of the colonial period, the subject has been handled in well-documented works by Cook, Borah, Rosenblat, Rivet, and several others as recently as the past two decades. Some of these latter investigators have studied sources not taken into consideration before, such as *Papeles de la Nueva España* (a summary of visits to town), *El Libro de las Tasaciones de Pueblos de la Nueva España. Siglo XVI* (General Archives of the Nation) and others, with highly satisfactory results.

It is unquestionable that the aboriginal population greatly diminished from the 16th to the 20th centuries and that the main causes of the decrease were the conquest itself and diseases such as smallpox, measles, and tuberculosis. The destruction of native centers did not, however, take place with uniform intensity, either in time or space. Numerous groups on the Pacific Coast and in the northern part of the country were totally extinguished, and we are only acquainted with them nowadays through historical and ethnic references. On the other hand, the great cultures of the highlands, although decimated, were able to survive. In what proportion did the population diminish? It is simply impossible to conclude with a *general index* that would be valid for the different ethnic groups of America.

Thompson's very simple method is a serious and logical contribution. The structure and nature of its formulas allow one to go on to more complicated problems, but I do not believe that the method is applicable to such complex cultures as those of prehispanic Mesoamerica. For example, statistical data are lacking for game animals, and therefore one could not estimate how many of them were hunted and thus deduce the number of hunters.

Taxes or tributes, which have been used to calculate the amount of tributary population, are also problematic, since the amount of tribute was not always applied proportionately but depended at times upon the resistance offered by the conquered.

The methods employed by the different investigators of the problem are all very interesting, and they demonstrate how far we are from results which, when analyzed with precision, could approximate a possibly correct calculation.

To the methods enumerated, others could be added which would bring the investigator closer to a tangible reality. One might calculate, for example, the number of individuals that would be necessary to erect a certain monument in one of the known archaeological zones whose boundaries can be established. If one added to the estimated number of workers the population necessary to support them and adjusted the percentages in reference to the time calculated for the work, taking into consideration the technique used, the place where the materials were obtained, etc., one could calculate the population that occupied that area in a given period.

Other evaluating methods could be supplied by the organic and inorganic residues (detritus) left in a certain zone, by cultivated areas, irrigating systems, the number of wells, cemeteries, etc. Finally, deductive methods and techniques for approximating the calculation of a given population might be multiplied if one could rely upon the possibility of carrying out the investigations.

Dobyns' fundamental contribution consists in providing a new figure, higher than that of other investigators. His extensive bibliography indicates that he offers this figure knowing full well what he is saying. One defect of his work is his use of figures from very different dates in his table of estimated population.

*by* Thomas F. Kehoe and Alice B. Kehoe☆

*Lincoln, Nebr., U.S.A.* 10 II 66

Dobyns' critique of previously accepted population estimates for aboriginal America should become a watershed in the study of cultural development in the New World. Both his illumination of methodological assumptions and his contribution of a new population estimate call for a re-evaluation of the classic discussions of prehistory and colonial history in the Western Hemisphere.

Perhaps the most valuable aspect of Dobyns' paper is his trenchant analysis of the foundations of previous population estimates. It is only just to apply his standards to his own population estimates, lest these become a new dogma. One can quibble at such statements as (p. 402), "Assuming that all the workers were men . . .;" considering the Spaniards' inhumanity toward Indians, one could as well assume that women were also pressed into laboring at the wall, and that agricultural producers were not spared when gangs were conscripted. Two million might then represent a large proportion of the adult and adolescent Indian population of central Mexico in the 16th century. One can complain that MacLeod's mention of unusual fertility for the Saskatchewan lakes region was erroneous (he apparently meant the Great Lakes wild-rice area) and should not have been cited without comment. We seriously question, however, two features of the second part of Dobyns' paper.

1) We doubt that the 1930 census represents the North American Indian population nadir. Dobyns himself claims that 130 years (five generations?) is the average length of time required for an Indian population to reach nadir after contact. The 1930 census, then, is from one or two to ten generations past nadir, and this is significant when the extraordinary increase in the North American Indian population in the past generation (as high as 5% per annum) is taken into account. So late a census figure for his base calculation has probably skewed Dobyns' estimate of aboriginal North American population.

2) We feel that any estimates utilizing a hemispheric depopulation ratio should be regarded as highly tentative. Dobyns' apparent postulation of a more or less equal susceptibility to European diseases in all Indian groups, a postulation which he seems to think justifies a hemispheric depopulation ratio, has not been demonstrated. Only a rabid isolationist would completely discount the possibility of pre-Columbian introductions of Eurasiatic diseases, especially into Pacific coastal populations. A single carrier on a single Japanese fishing junk blown off course might drastically affect hundreds of Indian settlements and eventually produce a relatively immune population in the contact area. Instead of using a hemispheric ratio, Dobyns would have been wiser to utilize ethnohistoric sources to estimate populations area by area. The resulting differences in depopulation ratios might in themselves be instructive, and the final population figures would surely be more reliable. We admit this would be a formidable task, but this does not excuse Dobyns from discussing the limitations of his method more fully.

In closing, let us emphasize the importance of Dobyns' critique and the repercussions it should have upon American history as well as upon anthropological theory. We think, for example, of the history of the fur trade in western Canada. The intense rivalry for pelts that led to the merger of independent traders in 1787 to form the North West Company, an effective competitor against the powerful Hudson's Bay Company, followed immediately the first devastating smallpox epidemic in the West.

Did the loss of so many hunters and artisans force the Indians into a new dependence upon the fur trade, and did the reduction in hunters increase the competition among the traders? Since the Canadian fur trade in turn affected European politics, one wonders how much of 19th-century history might have been different had smallpox not broken out on the Canadian prairies in 1780.

We have two comments on Thompson's paper, one specific and one general:

Thompson's figures for Chipewyan utilization of caribou do not fit the ethnographic data as closely as they should if the population estimate derived from their use is to be credible. When Hearne states that upwards of 20 caribou skins were required for each Chipewyan adult's clothing each year, and Banfield concurs in stating that 12 skins were required for a set of winter clothing alone, Thompson must use 20 as his minimum figure for the clothing needs of each adult per year. There is no justification for arbitrarily deciding that clothing would last three years; we think it highly unlikely a set of caribou-skin clothing could be worn continuously for more than a year and still provide adequate warmth through a sub-Arctic winter. Jenness (1959:153) implies this when he states that a caribou-skin costume would last "at the most" three years, among Eskimo who usually owned more than one set of clothing. Furthermore, if Thompson figures each "tent" to contain a joint family including two conjugal pairs, most tents would require more than four sets of adults' clothing, because the pattern of Chipewyan clothing made it necessary to cut all but small children's parkas and trousers from whole hides. (As for "non-active" adults who would need only half sets of clothing, we doubt such a category existed in aboriginal Chipewyan society.)

Tent covers would probably have to be replaced each year, necessitating 25 hides annually per joint family for this shelter alone. Ewers (1955:132) reports that the Blackfoot preferred to make a new bison-hide lodge cover each year, although many families made a lodge cover last two years. Five years would be far too long for even a tough bison-hide cover to remain serviceable, and we would judge the much thinner caribou-hide tent would not last more than one year. Modern canvas tents seldom survive more than one seven-month field season in western Canada, in our experience.

In addition to the perhaps 140 hides per year that Thompson's Chipewyan joint family would require for clothing and the 25 skins it needed each year for shelter, Thompson should have figured on at least half a dozen hides for storage bags, thongs, and bedding. The Chipewyan, like the Blackfoot, would have used worn-out tent covers for such purposes, but probably would have needed more leather than could be thus salvaged.

Finally, it might be wiser to use Ewers' (1955:168) figure of three pounds of meat per day as the average aborigine's consumption. This figure was gained from Blackfoot who remembered pre-reservation hunting, and in being more generous than Thompson's minimum daily calorie intake figure, it allows for the waste of meat in the periods of caribou migration, when wholesale slaughter left many animals to rot. Ewers' figure, it should be noted, is far below Spencer's (1959: 142) observation of eight pounds of meat as the Eskimo adult's daily consumption.

In summary, Thompson's minimum population figure is probably too generous, and his maximum figure cannot be reconciled with the ethnographic facts.

Our more general criticism of Thompson's paper is that his technique requires data on both pre-contact biota and aboriginal utilization of the biota, and these data must be more detailed and reliable than we are likely to obtain even for living biota. The more guesses that must be made, the more choices between conflicting data, the more presumably comparative figures pulled in to fill gaps, the less reliable the equations and the estimates derived from them become. The Chipewyan case took advantage of a people primarily dependent upon a single species for which there is now unusually complete demographic information. Thompson could neglect fish and rabbits as alternative foods resources for the Chipewyans because caribou hides were as necessary for Chipewyan survival as was meat. In spite of the comparative simplicity of this test case and the abundance of data on the caribou, Thompson's final estimate did not impress us as remarkably accurate. We therefore consider his technique impractical, although we found his paper an interesting statistical exercise.

*by* N. Keyfitz and A. Carmagnani☆

*Chicago, Ill., U.S.A.* 28 II 66

Dobyns' collection of references on the pre-Columbian population of the Americas is welcome, even though it consists for the most part of quotations from observers whose skill and impartiality we have no way of verifying. For the period up to two centuries ago, we know much less about the population of the Americas than about that of Europe or Asia, where at least hearth counts, military registrations, and tax records permit us to ignore patently ignorant or biased casual remarks that happen to have been set down in writing. For the Americas these latter must be valued; they constitute the main basis so far of ancient demographic history.

A demographer wonders, however, whether some more systematic and less polemic arrangement of the citations would not be more informative. Though he can see the need for quoting the estimates of practically anyone who lived before censuses were taken, the value of citing the third-hand opinions of our own contemporaries is less obvious, especially since the method finally chosen disregards nearly all the quotations, both ancient and modern.

The method used is to find from modern censuses the "nadir" or lowest population reached by the Indians in the various countries of the Americas during the period of modern census-taking, and then multiply it by a measure of the depopulation which resulted from disease and the effects of Spanish conquest. The depopulation ratio is obtained from a guess that Central Mexico alone contained 30,000,000 (p. 408) people in Columbus' time and 1,500,000 at its nadir. Since the nadir population of Mexico is estimated to be somewhat less than 1/3 that of the Americas as a whole, the Dobyns method in effect multiplies the 30,000,000 by three to find over 90,000,000 for the total. Both the 30,000,000 and the depopulation rates are highly discussible.

One would have thought that the result would at least be checked against ecological possibilities. Sapper (cited on p. 405) estimated the "potential human carrying capacity of the various geographical regions of the hemisphere" and then assumed "that actual aboriginal populations reached their ecological maximum;" he found a hemispheric population of 40,000,000-50,000,000. This would seem to set an upper limit on the pre-Columbian total, yet Dobyns doubles it. He underplays whatever objective evidence does exist, such as the pre-conquest tax records of Mexico and later records of baptisms and deaths.

What do we know about the population of the Americas in 1492 as a result of Dobyns' work? All we would place even a small bet on is that the true population was greater than Kroeber's low estimate of 8,400,000 and smaller than Dobyns' high one of about 100,000,000. One cannot but admire the man who boldly struggles with inadequate data when no better are to be had; but if he expects our admiration for scientific restraint as

well as for courage, he must present the outcome of his struggle in a form which accords with the modesty of his materials. The scientist does not take a doubtful nadir figure of around 4,500,000, apply to it a depopulation ratio of 20:1 based on an inadequate sample, and come up with the 1492 population of 90,043,000 Indians in North and South America. Instead, he recognizes that his 90,043,000, the result of multiplying two guesses—a nadir population and a depopulation ratio—is not necessarily more accurate than if it were a single guess, frank and open. He faces up to the fact of ignorance, his own as well as that of Kroeber, Rosenblat, and Willcox, and tries to establish the variance to which each of his factors is subject. The most he commits himself to is a range of possibilities; he is satisfied if the evidence he can adduce narrows somewhat the range which expressed the ignorance of his predecessors.

The extreme defect of the Dobyns approach, its lack of theory, is remedied in the article by Thompson. There a simple model is developed and some highly relevant ecological data are assembled. Aside from the remote chance of discovering the manuscript volumes of some pre-Columbian census, our best hope of getting to know more about this largest gap in world demographic history is by a welding together of the hearsay evidence assembled by Dobyns and the theory of Thompson.

*by* Peter Kunstadter☆

*Princeton, N.J., U.S.A.* 28 II 66

Dobyns' review of the past sins of anthropologists, historians, and others who have attempted to estimate the aboriginal American population is a welcome addition to the history of anthropology, and he is correct in pointing to the theoretical importance of the topic.

Without saying anything regarding his own population estimate, one can criticize his method of "depopulation ratios." The aboriginal population is usually unknown, but estimates are often available for the nadir to which the populations sank after contact. Multiplication of the nadir population by the reciprocal of the depopulation ratio,

$$\left(\text{depopulation ratio} = \frac{\text{aboriginal population}}{\text{nadir population}}\right)$$

derived from control areas where both figures are known gives the estimated aboriginal population where only the nadir is known.

The success of the method depends on the accuracy of the measures of aboriginal population and nadir population in the control areas. Of course, the estimates for both of these figures are subject to precisely the same limitations as Dobyns has suggested for *all* aboriginal population estimates. As parts of a ratio, the two figures have a profound influence on one another. The more sensitive, of course, is the figure for the nadir. The nature of the index is such as to lead to the paradoxical conclusion that, all other things being equal, the lower the nadir in the control area, the higher will be the depopulation ratio, and thus the higher will be the estimated aboriginal population. This strikes me as a dangerous method, since it is quite well known that some of the aboriginal populations have become extinct (e.g., the Caribbean Island Indians). Their nadir is 0, and the depopulation ratio

$$\left(\frac{\text{aboriginal population}}{0}\right)$$

is equal to infinity. In general, where the nadir is low relative to the aboriginal population, a small change (especially downward) will have a relatively large effect on the estimated depopulation ratio and projected aboriginal population estimate.

I would not want to risk a general statement that *all* estimates of nadirs are too low (and therefore that all of Dobyns' estimates for aboriginal population are too high), but one argument indicates that this may be the case. This is the difficulty in determining what an Indian is. The census definition is generally a *cultural* or a *social* one, not a *biological* one, but it is the "biological Indian" with whom we must be concerned in making estimates of the aboriginal population size. Estimates for the number of Indians in the United States, for example, define Indians as persons who are recognized as members of Indian communities, or as persons who are listed on tribal rolls as Indians. Many of the people who are genetically partially or wholly Indian are not so counted by censuses, because they are not socially or culturally Indians. There is no a priori reason for assuming that the number of biological Indians was equal to the number of socially or culturally defined Indians at the time the biological Indian population reached its nadir. The same caveat applies to Latin America, where Indian-ness is largely a matter of cultural identification.

A further limitation of the hemisphere-wide depopulation ratio is that we have no a priori reason for assuming that the depopulation ratios are the same in all areas, or under all conditions. In fact Dobyns presents information to the contrary (the Caribbean Islanders can again be used as an example). The argument against the use of a common depopulation ratio is the fact that disease (apparently the major source of depopulation) does not spread in the same way under all patterns of life. Again, I have no general prediction as to whether this would raise or lower the estimate based on a depopulation ratio.

Rather than use a generalized ratio (the limitations of which I have suggested) I wonder if more modern means of population estimation could not be applied to the existing data. Two methods suggest themselves. (1) a more detailed use of modern methods of "ecological potential" (Thompson's article is an example of this approach; for another, see Allen 1965); and (2) more adequate use of life tables (e.g., Coale and Demeny 1966) where there is some information available on age distribution of the population. Use of life tables would allow a more accurate reconstruction of birth and death rates, which would be useful in other attempts at extrapolation from incomplete data; and they would allow more adequate projection figures than the very crude estimates quoted by Dobyns.

Let us examine the argument regarding Las Casas' estimates of depopulation—specifically, the argument that more people died within 40 years as a result of contact with the Spanish than other authorities estimated were living in 1492. We cannot discuss this question profitably unless we have some estimate of "normal" birth and death rates. Given appropriate vital rates, the number of people normally expected to die in 40 years might very well exceed the number of people living at the start of that period.[1] This leads us to ask several kinds of questions. What sort of mortality rates was Las Casas used to in his own native environment, and how did he (or any of the other authorities) distinguish "normal" from "Spanish-caused" mortality? What were the vital rates, and what was the age structure of the population of the New World in 1492? What was the effect of Spanish-induced mortality on marriage and birth rates and on the age-sex pyramid? When we have answers to these sorts of questions we will be able to approach the whole question of depopulation more effectively, because we will have data upon which we can apply more powerful quantitative methods to verify our guesses about how the New World population responded to European-induced mor-

[1] If we assume a stationary (non-growing) population and an uncontrolled birth rate, then the death rate must have been at least 35/1000, and more probably approached 40/1000. A death rate of 40/1000 would mean that 1.6 times as many people died during 40 years as were alive at the start of that period.

tality (see MacArthur 1961, esp. pp. 14-20, as compared with Rivers 1922, esp. his chapter on "The Psychological Factor," for an example of the application of this type of approach to interpretations of demographic history in Melanesia).

In conclusion, modern experience should lead us to recognize the great difficulty in making accurate approximations of the aboriginal population of the New World; even recent censuses based on head counts have occasionally been shown to be quite inaccurate. Any approximation we make should thus include some estimate of the probable error of the figures. I think the best we can hope for at this point is some reasonable estimate of the population in particular areas of theoretical significance (not the whole hemisphere), since our theoretical interest is in the relationship of population to social development in particular areas. This would indicate, for example, that we pay more attention to careful estimation for Mexico or for Eskimo, rather than attempting hemisphere-wide guesses.

Thompson's paper is useful in that it makes explicit the sorts of assumptions which must be made in order to compute the expected human carrying capacity in an area where man is involved in a relatively simple food chain. His frankness in making his assumptions explicit should make it easy to pick holes in them, but more importantly it has the effect of pointing to the sorts of variables and relationships which should be investigated by fieldworkers: the nature of the food and raw material supply and the quantities consumed by man and his domestic animals.

His conclusions regarding the estimated population of the Chipewyan (estimates of 4,670 to 10,294) lead to some interesting questions. These include the question of whether the difference between the two figures can be interpreted as a *range* of Chipewyan population, and if so whether this range results from imprecise data, or whether it is a function of the nature of the food resources and the type of food chain. This further suggests a comparative inquiry between hunting and gathering groups (preferably where the parameters of the food chain are well known) and agricultural groups (again where the parameters of the food chain are well known) in order to test the hypothesis that agricultural groups, because of their greater control of energy resources, have inherently more stationary (i.e., less subject to large fluctuations in size) populations than do hunters and gatherers. Such a test for the hypothesis would have many implications for predicting on strictly demographic-ecological grounds the sorts of problems (e.g., political control over expanding and contracting populations, maintenance of kin-group structures despite wide fluctuations in their size) which would have to be dealt with in societies with different economic-ecological bases.

We can also expand on the implications of one of Thompson's statements: "The advantage of this technique is that there is no need for . . . the census, although census data obviously can be used to validate such an estimate." If the technique has utility, it is in predicting the demographic characteristics of real populations *if they conform to the limits of the model.* One underlying assumption of the whole argument is that human populations will fill to the point of equilibrium the ecological space available to them. So far we have no good evidence to indicate that this is true. Good census information which showed, for example, that the human population was far above the predicted maximum would lead to the conclusion that human populations are not necessarily in equilibrium with their food resources. In fact we know this is the case in some areas (e.g., present-day India), but this is usually explained as being the result of culture lag (medical technology having made more progress and having been diffused more rapidly than food production and distribution technology). Finding that an isolated, "pristine" human population did not conform to the equilibrium model might dispel some of the mysticism surrounding the idea that primitive man is always in balance with nature, while modern man despoils and upsets that balance.

*by* T. J. MAXWELL☆

*Thousand Oaks, Calif., U.S.A.* 21 II 66

Dobyns has provided the reader with a wide range of population estimates for aboriginal societies in North and South America. He has demonstrated the need for more complete checking in the giving of population estimates. It would seem that a population estimate for the entire hemisphere based on a standard depopulation ratio might be possible. More care might be used in arriving at the ratio; however, some variance and inaccuracy is bound to occur. On the basis of Dobyns' own evidence, I would choose a ratio slightly less than 20 to 1, perhaps 17 to 1. The more trustworthy figures would be those including data from records of baptism and confirmation, tax roles, and consensus, and from reports of army conscription quotas, epidemic deaths, warfare losses, migration, population recovery soon after the conquest, family size, and the labor force necessary for public works. One should determine not just population change in order to arrive at a figure for 1500 A.D. for each area.

An important consideration Dobyns has not dealt with is changes in the mode of subsistence: the development of intensive agriculture or the addition of the plow and oxen in post-conquest times would surely have tended to offset the rate of population decrease by increasing the food supply.

Rouse (1952:566-73), in his discussion of aboriginal population in Puerto Rico, suggests a decline (based on the number of identified sites for that period) in the rate of population increase between 1193 and 1317 and an absolute population decrease after 1437 A.D. He attributes the drop in rate of increase in the 13th century to emigration to the Dominican Republic. He explains the decrease in population in the late 1400's in terms of (1) the effects of population pressure brought on by maximum density in 1400, (2) the arrival of Caribs, who raided Puerto Rico constantly in the 15th century and carried off "many people," (3) possible loss due to movement to the mountainous interior of Puerto Rico, and (4) Spanish influence after 1493. He finds a population density of 1.8 for the early historic period by using Brau's (1904) estimate of 16,000. Spinden's estimate, cited by Dobyns, is six times as great, and Las Casas' 800,000 is unbelievable.

What I am suggesting is that (1) there may have been pre-Columbian epidemics, famine, warfare, migrations, and social controls which served as population checks; and (2) after the European diseases had exacted their toll, there may have been some dramatic population recoveries, encouraged by increased available food (fewer mouths to feed) and the introduction of iron tools. These factors would tend to contradict a postulated larger pre-contact population. Documentation, wherever possible, for both the factors discussed by Dobyns and those mentioned above might contribute to the accuracy we strive to achieve.

*by* JOHN PADDOCK☆

*Mitla, Mexico.* 24 II 66

These notes are limited to the Dobyns article. I should like only to add some references and to suggest certain lines of thought opened up by acceptance of the new estimates of aboriginal population in central Mexico put forward by Cook, Borah, and Simpson and defended by Dobyns.

The "Black Legend" of Spanish treatment of the Indians became itself a significant factor in Latin American history. At the conclusion of a thor-

ough recent study closely relevant to the Dobyns essay, Gibson (1964:403) observes that it

> provides a gross but essentially accurate interpretation of relations between Spaniards and Indians. The Legend builds upon the record of deliberate sadism. It flourishes in an atmosphere of indignation, which removes the issue from the category of objective understanding. It is insufficient in its awareness of the institutions of colonial history. But the substantive content of the Black Legend asserts that the Indians were exploited by the Spaniards, and in empirical fact they were.

The historical role of the Black Legend has been considered briefly by me (Paddock 1958), and much more extensively by Hanke (1959). The 17th-century Dominican chronicler of Oaxaca, Francisco de Burgoa (1934), should be added to the list of Spanish friars who contributed to the "Black Legend" by complaining of the treatment given the Indians; although he never gave much space to the theme, Burgoa did repeatedly mention what he considered to be immoral exploitation, especially in the mines.

The accounts of Cortés and Bernal Díaz are often regarded as plainly incredible because they are interpreted as saying that Cortés and a few score Spaniards, with horses, steel armor and swords, and firearms as their only advantages, somehow met and defeated in battle scores of thousands of Indian soldiers. In fact, Cortés shrewdly divided and conquered. The situation awaiting him when he landed in Mexico was ripe with explosive divisions which he perceived and utilized. He was the catalyst, provoking the angry uprising of the great majority of Mesoamerican Indians against the Aztecs to whom they had been paying tribute. There is every reason to believe that Cortés did indeed meet and defeat very large armies; what he and Bernal Díaz often failed to mention is that they did so with the aid of other large armies, made up of the Indian enemies of the Aztecs, who enthusiastically allied themselves with Cortés.

After the conquest, depopulation by disease surely must have been in general more severe among densely settled populations than among scattered ones. Disease transmitted in food and water clearly is less threatening in a Mesoamerican "empty town" (a community made up of scattered hamlets sharing a civil-religious center with only a very small resident population); but the Spanish tended to congregate the Indians in the civic centers, after the model of the crowded European town of the time. Diseases transmitted by non-intestinal vectors prospered even in the empty towns because of the frequent contacts in markets if for no other reason.

Having in mind the devastation of a presumably more or less "immune" European population by the various plagues, it is difficult to see why we should be unable to accept equal and greater devastation by Old World diseases among the American Indians.

On the basis of still unpublished surface surveys of archaeological sites extending about as far south as Ocotlan and covering the north (Etla) and east (Tlacolula) arms of the Valley of Oaxaca, Ignacio Bernal (verbal communication) estimates that there were roughly as many settlements in this area during Monte Albán IIIb-IV, i.e., about A.D. 600-1500, as there are today. A new study, the first ever to give the identity and location of all modern settlements included within the valley (Welte 1966), tends to corroborate this impression.[1] Whereas Bernal found somewhat over 200 prehispanic archaeological sites (1958: 7-8), Welte reports (verbal communication) that within the same area his 1965 survey shows about 200 settlements listed in the 1960 census as *municipios* or recognized subdivisions of them.

In addition to number and population of settlements, a factor subject to comparison is the relative wealth shown in the public buildings of late pre-conquest ruins and of the present-day towns descended from them. In some cases very large church-monasteries were built up to about 1560 (Kubler 1948) in communities that today can actually support only a very modest church—another indication that Dobyns is right— and almost always the impression the visitor gets is that, when a modern town is clearly connected with a pre-conquest ruin, the public buildings of the pre-conquest settlement were much grander than those of today.

In a recent study, Dumond (1961) concluded that swidden agriculture as it has been practiced in recent times in environments similar to that of the ancient Maya could easily have sustained ancient Maya society without progressive damage to the environment's productive capacity.

The Mixteca of western Oaxaca is often characterized, largely because of its erosion, as having been as poor before the Spanish conquest as it is today. But 16th- and 17th-century eyewitness accounts unanimously refer to it as a rich and productive region (Paddock 1964:464-65; 1966*b*:369-70). If the conclusions of the Cook-Borah-Simpson studies are correct there is good reason to suspect that depopulation, largely consequent on disease, caused the erosion, rather than that erosion forced the depopulation (Paddock 1966*c*:234-36).

Through the recent work of MacNeish and others (n.d.), especially Woodbury and Neely (see Paddock 1965:133-36), it is known that in northern Oaxaca and southern Puebla there was in late prehispanic times a vast area under intensive cultivation involving dams, aqueducts, and terraced fields, the whole constituting systems of great complexity. (These procedures in fact go back in the region at least until the first half of the first millennium B.C.) Today there are in that region areas formerly under intensive irrigated-terrace cultivation which have become virtually uninhabited desert. The 95% depopulation concluded by Cook, Borah, and Simpson suggests how this happened, and how great must have been the pre-conquest population density if such lands have not yet been reclaimed.

As villages were devastated by epidemics, they became impractically small administrative and economic units, and Spanish colonial officials consolidated them. Naturally the consolidation was accomplished by moving people in from the places having the most intricate irrigation systems to those where irrigation was least complex and least necessary. The abandoned terraces, breaking down under the impact of desert thunderstorms, left large parts of the Mixteca bare of soil, as they are today. Knowledge of the details of local systems was lost first; then the practice of irrigation in general became less common, and terracing virtually ceased; finally all memory of the former skills disappeared. In those few places where irrigation did go on, Spanish and Arabic practices replaced the indigenous ones.

As the population slowly recovered after 1650, people moved back into certain areas of the Mixteca and began practicing agriculture with the plows and draft animals the Spanish had introduced or animal husbandry with Spanish-introduced sheep and goats. It was these Spanish innovations that completed the catastrophic erosion of the Mixteca that began when terrace systems were abandoned because there no longer existed a population to use them.

[1] Copies of Welte's map may be obtained by writing him at the Oficina de Estudios de Humanidad del Valle de Oaxaca, Apartado Postal 518, Oaxaca de Juárez, Oaxaca, México.

*by* MILAN STLOUKAL☆

*Prague, Czechoslovakia.* 11 II 66

The papers by Dobyns and Thompson are very interesting from a methodological point of view. The problem of the estimation of population is very important for us, especially for the Middle Ages, but also for prehistory, where it meets with many obstacles due to the lack of material. As far as I know, there are three main direc-

tions followed in this research. The first, of which Thompson's work is an example, is to estimate how many individuals a given territory could support. In Europe, we must base our estimate not on the consumption of animal products but on plant production. There is the question how many persons a given area of agricultural land can feed. Other questions are what percentage of the land was cultivated at that time and what the yields were.

The second method, applicable only to the later Middle Ages (11th and 12th centuries), for which some written records are available, is based on the estimation of the number of communities, the number of estates (which implies the number of families), and the number of members of each family. In comparison with the former method, this approach gives higher numbers. Both methods, however, are too much dependent on the personal opinion of the investigator.

In addition to these two methods which are used primarily by historians, there is a third one, frequently used by physical anthropologists, predominantly for the early historical period, for which there are few written records (for example, the 9th century A.D. in Czechoslovakia, the so-called Great Moravian period), but also, of course, for earlier prehistoric periods. This method involves the number of skeletons found in a burial ground, the duration of the use of the burial ground, and an estimate of the mortality rate, which represents the only unknown and could range roughly between 30 and 50 per thousand. This approach is used, in various modification, in most of the European countries where material from prehistoric and early historic burial grounds is studied. While the method provides data only for one site, the sum of the individual results will provide a general picture. This will, of course, depend on whether archaeological excavations supply enough material.

It seems to me that this third approach gives the most reliable results, even though it is necessary to take into account a number of unavoidable mistakes, to the incompleteness of the archaeological material. It would be interesting if an analogous approach could be used in the areas treated by Dobyns and Thompson.

*by* H. Paul Thompson

*Seabrook, Tex., U.S.A.* 25 II 66

On the whole, Dobyns presents an excellent summary and evaluation of the primary aboriginal American population estimates and of the techniques used in arriving at these estimates. It seems to me that the importance of epidemic disease, as a major variable in population size estimation, is undeniable. The idea of a "depopulation ratio" would seem to have, with further refinement, considerable utility. Dobyns' point that an analyst should not ignore population data because he doubts the validity of the recorded figures is well taken. However, I am more skeptical than Dobyns of the figures which eyewitnesses report.

Dobyns stresses the importance of checking for internal consistency in whatever population figures are available. There is, however, a type of error in such records which checking on internal consistency cannot catch. This is a consistent error in estimating numbers due to a cultural bias. For example, U.S.A. census returns consistently show far too many people whose ages are divisible by two or by five. A degree of control may be exercised over such cultural bias by applying external checks to population data. For example, the 700 baptisms per day per priest reduces to a rate of one baptism every two minutes 24 hours a day. The priest credited with baptizing 14,000 persons in a day would have had to baptize a person every six seconds for 24 consecutive hours. Baptism at the latter rate seems somewhat improbable. However, it should be possible to obtain information from Catholic records as to what constituted the minimum ceremonial requirements for a person to be considered baptized. Once these requirements were determined it would be possible to estimate average and maximum baptismal rates. Such rates could then be used to calculate average and maximum converts. These figures might be used as an external check on such numbers as those attributed to Motolinia.

It appears to me that Dobyns intends his depopulation ratio to be a first approximation to a linear pancultural (aboriginal American) rate of population change due to epidemic disease. Additional information about the characteristics of the depopulation ratio might be obtained by applying it to the Chipewyan, an aboriginal population which I have studied in detail from an ecological point of view.

According to Mooney (1928), the population of the Chipewyan (plus Caribou-Eaters) in 1906 (only 24 years from Dobyns' nadir for North American aboriginals) was 2,420 persons. A depopulation ratio of 20:1 yields an estimated aboriginal population of 48,400. This figure is approximately 4.5 times larger than my estimate of the maximum ecological potential (10,657). If we accept the logic of a depopulation ratio and apply Dobyns' 20:1 figure to this maximum estimate, then there could have been no more than 533 persons at population nadir, *if* the aboriginal Chipewyan were an ecologically stable population. Hence, it appears that one of three factors is affecting this population estimate by means of Dobyns' depopulation ratio:

1) The Chipewyan population was far above nadir in 1906. This is certainly possible. However, there is little indication that the Chipewyan had a 5:1 population decrease between 1906 and 1930. If we assume that population nadir occurred around 1781, when a 90% population decrease was reported by Samuel Hearne; and assume by "best" estimate (6,416) of Chipewyan population just prior to the epidemic which caused this decrease; then Dobyns' depopulation ratio applied to the nadir population of 642 leads to a figure of 12,840, only 21% over ecological potential.

2) The Chipewyan population was not at all representative of North American aboriginal populations. This is possible, but statistically unlikely.

3) The depopulation ratio is more sensitive to differences in cultural area and conditions of contact than Dobyns thinks.

The utility of Dobyns' depopulation ratio would be enhanced if:

1) the depopulation ratio were expressed as a function of the generation span from first contact to population nadir. Normally, it is the generation which first comes into contact with an epidemic disease that exhibits the highest loss rate. Successive generations usually show progressively less response to the original form of the disease. It probably would be advisable, therefore, to have different depopulation ratios for epidemic and endemic generations. A free-living population, whether deer or human (if unsupported by modern technology), will usually display ecological cycles in which the population contracts and expands.

2) the effects of what might be termed a "repopulation ratio," expressing the interaction between the "intrinsic rate of increase" (Birdsell 1958) and the rate at which a region acquires new people due to migration, slavery, etc., were considered.

3) the depopulation ratio were considered to be a function of the amount of interpersonal contact. I would imagine that detailed studies would indicate that the depopulation ratio for a group is roughly proportional to population density.

*by* Bruce G. Trigger☆

*Montreal, Canada.* 10 II 66

The destruction by disease of a large percentage of the native population of the Western Hemisphere was undoubt-

edly the most striking and sorrowful consequence of the arrival of the Europeans. It is amazing that this cruel and fantastic example of natural selection has so long escaped the thorough investigation by anthropologists that it deserves. Undoubtedly this is in part the result of anthropology's traditional lack of interest in using historical data to study the past.

Dobyns has produced a well-executed and highly enlightening study. Few will dispute his main conclusion: that the native population of the Western Hemisphere was much larger than most estimates would lead us to believe. I shall attempt to make only a few comments from the viewpoint of the tribe I know best, the Huron of southwestern Ontario. Crude approximations of regional or hemispheric populations can be attempted in a variety of ways, and the results undoubtedly are important for understanding social dynamics. Nevertheless it seems evident that increasingly accurate hemispheric estimates and growing confidence in the results require increasingly accurate estimates for individual tribal groups. The intensive study of those groups for which the best data are available should be of value in making estimates for other groups about which we have little or no information.

Dobyns' article stimulated me to check through the source material on the Huron. Good historical documentation is available for this confederacy between the years 1615 and 1650. The only census ever taken was a rough survey by the missionaries in 1640 (Thwaites 1896-1901, XIX:127). It revealed a population of about 12,000 people, made up of 4,000 families (2,000 fires) living in 32 villages. These figures were collected, however, after several severe epidemics. Earlier estimates of population are either 30,000 or 40,000, but it is unclear to what degree the various citations are independent of the figure of 30,000 originally given to Champlain by the Huron themselves (Grant 1952:313). Although space precludes giving details, two conclusions can be reached concerning the data supplied by our sources:

a) While many figures are given concerning such things as the number of villages in Huronia or the approximate number of people per house, it is very difficult if not impossible to calculate the range in size of villages or in number of people per house. This makes it difficult to convert data from one level to another.

b) Where it is possible to cross-check the sources by converting statistical data from one level to another, great discrepancies appear which give rise to serious doubts concerning the accuracy of the data. Family size, for example, varies from 3 to 7.5 people per family, with nothing to suggest that the variation is the result of disease. The number of families per longhouse seems to vary between 1 and 24, with an apparent average of 8 to 10. Other figures, however, suggest that a village of 100 cabins is equal to 400 households.

My impression of these data is that while they provide general guidelines, they are not reliable enough for a precise and accurate estimate of the number of people living in Huronia between 1615 and 1650. I would want to check these data against figures for comparable groups and above all against archaeological evidence concerning the number of houses per village and the number of hearths per house for as many Huron sites as possible. This makes study of the Huron population an on-going process and the corpus of data an open one. Archaeological data may also help to answer the so-far unconsidered question of whether or not epidemics had thinned the Huron population prior to the arrival of the first French explorers and missionaries. Since European traders had been active along the St. Lawrence River since the early 16th century, this seems an important problem.

Another interesting problem is the number of Huron that survived the destruction of Huronia by the Iroquois in 1649-50. Some were killed, and others remained in small bands in Quebec and Ontario, but large numbers were incorporated, either willingly or by force, into various other tribes including the Five Nations. To date no serious attempt has been made to determine the number of Huron who disappeared through assimilation rather than annihilation, but I suspect that the figure is higher than most people believe. It is also interesting that the Jesuit Relations record that the epidemics killed a relatively large percentage of children and old people. With the latter must have perished much of the traditional lore of the Huron, which was to a great extent the property of the aged (Thwaites 1896-1901, VIII:145-47).

Turning to more general matters, I would suggest that the example of cross-checking given on p. 398 is not a good one, since the Iroquois were never simultaneously at war with all the tribes listed and indeed often had incorporated large numbers of their previous enemies into the confederacy before they went to war with the next. While the Mooney-Kroeber figures for the Iroquois are undoubtedly too low, Dobyns' figures are probably too high. I also feel that Dobyns' analogy between Inca and Aztec society seems forced and pointless in view of what is known about political organization in the two regions, which seems as different as that of Egypt and Mesopotamia between 3000 and 2500 B.C.

I agree with Dobyns that estimating the pre-contact population of the New World is important. Nevertheless, I believe that the estimations of population for even a single well-recorded tribe is far from easy and must be regarded as an on-going task on which all sorts of data, including archaeological data, must be brought to bear. The results are never static, but continue to change as more information becomes available and old data are interpreted in a new light.

## Reply

*by* HENRY F. DOBYNS

Several commentators tax my hemispheric depopulation ratio as overgeneralized. Kunstadter, Bernal, Blasi, Bennett, Dávalos, Forbis, Haviland, Thompson, Trigger, and Kehoe and Kehoe urge the wisdom of area by area estimates for one good reason or another. I quite agree, as Denevan has recognized. Like Thompson, I regard my ratio as only a first approximation and my estimates as highly tentative. I sincerely hope my tentative ratio and approximate population estimates will challenge others to compare them with sets of local data as Blasi, Denevan, Farmer, Fuchs, and Thompson have. Like Bernal, Blasi, and Thompson, I view the aboriginal population density as one of the fundamental determinants of the absolute depopulation and relative numbers of Indian survivors in different regions, a matter relevant to Dunn's comments. I quite agree with Denevan's list of important research questions.

I welcome the consise and eminently useful methodological statement that Cook has contributed, as well as Driver's indictment of past methods of estimation in terms of rules of statistical inference. Thompson's warning about consistent error due to cultural bias is important. Maxwell is right in urging reconstruction of the entire history of population change, and Thompson in differentiating between epidemic and endemic generations and mentioning ecological cycles. That I concentrated upon population zenith and nadir estimates rather than a more complete analysis

reflects the truth of Bennett's observation that the labor in my paper may be reduced to two lines in a census report and of Denvan's observation about scope.

I am grateful to Bernal and Haviland for presenting evidence of dense aboriginal Mesoamerican settlement and to Paddock for expanding upon the Black Legend as it affects the analysis of population size. Had I had access to Bernal's (1962) monumental bibliography while writing, I might have cited the earlier references; Haviland has supplied several that appeared while my article was in press.

Since Dunn applauds my concern with disease as a factor in population trends, I take up his criticisms reluctantly. My discussion surely does not provide the reader with that "full awareness" of the difficulties involved in analyzing the behavior of disease agents in human populations that he would prefer. I had hoped that my earlier papers (Dobyns 1962, 1963*a*, 1963*b*:513) would adequately demonstrate my awareness of the limitations of historical sources.

On the relative mortality of smallpox and malaria, I continue to argue that smallpox was the worse scourge of American population. The major concentrations of aboriginal population in the Americas occurred in the Andean and Mexican highlands at altitudes above the limits of survival of malaria vectors. Every general aboriginal population estimate that I have found reflects this distribution. However efficient malaria may have been, it simply could not reach the major portion of the aboriginal population. Smallpox could, and did, reach the inhabitants of the highlands. Given an approximately equal mortality rate among local populations, smallpox may be assumed to have wrought greater destruction of human life because it could reach many more potential victims. The role played by altitude in limiting the spread of some diseases (Monge 1948:xvi-xvii), e.g., hookworm (Payne, Gonzales M., and Schleicher 1956), along with its role as a cause of chronic mountain sickness (Monge 1928) and contributing factor in some silicosis (Monge 1953), needs to be kept in mind. Denevan supplies evidence for a considerably higher depopulation ratio among Amazon Basin peoples than Andean residents. While I disagree with his Andean figures, I find his Amazon ratio reasonable in terms of exposure of these low-altitude populations to yellow fever and the malaria Dunn emphasizes, as well as smallpox and other disease agents not dependent on low-altitude vectors. Evidence of native extinction in this area continues to accumulate (Evans 1964:436). Dávalos rightly points to the parallel virtual extinction of Mexican coastal aboriginals as contrasted to the survival of those in the highlands.

My conclusion as to the effectiveness of vaccination in reducing smallpox mortality was based on the accepted efficacy of vaccination (Gale 1959: 62), the Ashburn (1947) review of the consequences of vaccination of North American Indians, and the knowledge that the Spaniards instituted the practice promptly in the New World. Thus, when I called "fatal" Kroeber's choice of 1793 population figures as a base for projection, I meant that smallpox raged unchecked in the Americas before 1800, but was steadily checked by vaccination during the 19th century, when a spectacular increase in population occurred.

In suggesting that the smallpox epidemic of 1780 on the Canadian prairies affected the Canadian fur trade and, therefore, European politics, Kehoe and Kehoe point to one of the social responses to fluctuation in Indian population density that Forbis sees as necessary to demonstrate that population density has dynamic functions. The 1780 epidemic certainly was important. Wissler (1936:36) commented on a shift in tribal territory resulting from differential survival. The epidemic affected Indians over much of western North America, and a reconstruction of its full impact would be scientifically valuable. I suspect that this epidemic was the same one that killed over 40,000 individuals in Mexico City in 1779 and perhaps 50,000 in Puebla (Rosenblat 1954:73). Epidemic smallpox was disastrous in the Dominican missions of Lower California in 1780-82 (Aschman 1959: 248). Colonists from the Peninsula carried the disease into Upper California in 1781 (Cook 1939:155). In northern Sonora, epidemic smallpox fatalities occurred at San Ignacio Mission from August 21 to October 1, with 84% of the mortality among children (Pinart n.d.: 20-20v). At Tumacacori Mission, smallpox fatalities occurred from May 28 to July 3, and the crude smallpox mortality rate was 159 per 1,000 out of a 1781 crude mortality rate of 181 per 1,000 population (*San José de Tumacácori* n.d.). Aberle, Watkins, and Pitney (1940:167) considered this 1781 epidemic "the greatest in the modern history of the Southwest" with a mission mortality exceeding 50% of the Pueblo population enumerated in 1760, over 5,000 persons. If the disease did spread north from Mexico City, it travelled at different rates over different trade routes, for it reached northern New Mexico earlier than northern Sonora. The episode began at San Juan Pueblo late in January, peaked during the week of February 15-19, and receded in mid-March (Aberle, Watkins, and Pitney 1940:168). Jemez was so reduced that a mission was no longer maintained there; it became a visitation station (Hodge, Hammond, and Rey 1945: 279 n. 90). A like fate befell Acoma p. 288 n. 97), Santa Clara suffered heavy population loss (pp. 234-35 n. 22), and so on.

The Dakota Winter Counts indicated that many people died of smallpox in two successive winters, 1780-81 or 1781-82 (Mallery 1886: 131). Thompson (1916:322-23) reported smallpox among the Chippewa and Sioux in 1780 and suggested that they had contracted it by wearing clothing taken from white victims who had been ill with the disease. From these two groups, the contagion spread to the other Plains tribes and spread across the Rocky Mountains. Mortality reached an estimated $^3/_5$ of the population, and eyewitnesses reported that in many tents all the inhabitants died (p. 321). The Blackfeet reported that they contracted the disease by attacking a Snake encampment decimated by it (p. 336), although the reported incubation period suggests that they had already contracted it when they carried out their attack (p. 337). This was certainly the same epidemic episode that swept along the shores of the north Pacific (Gibbs 1877:208).

While the 1779-81 epidemic was important in reducing Indian populations in these areas, I doubt that it was the "first devastating smallpox epidemic in the West" that Kehoe and Kehoe label it. Epidemic smallpox was reported in 1764 in New Jersey, Boston, Virginia, and Maryland (Toner 1874:94). It was reported causing heavy mortality among Indians in widely separated regions near the English colonial settlements: the Creeks, Choctaws, Chickasaws, Mohawks, Shawnees (Duffy 1951:340). The disease was also reported from distant points in the Spanish colonial empire, such as the Guarani missions, where smallpox mortality was 8% of the 1764 population and 5% of the 1765 (Hernandez 1912:11), Lima (Lastres 1957: 30), and northern Sonora where 24 smallpox fatalities were recorded at San Ignacio Mission from the end of March to the beginning of June (Pinart n.d.). With the disease raging in epidemic form on the borders of the western areas of aboriginal occupation still not under European observation, it strikes me as quite likely that it moved out to at least some other tribes.

A similar opportunity appears to have existed a few years earlier. Epidemic smallpox struck Canada in

1755 and recurred in 1756 and 1757, spreading from the French to the Indians. The disease struck the Senecas in 1755 and spread to most of the Indians in eastern Canada and New England by the spring of 1756. The Delawares were affected that year, along with the English colonies (Duffy 1951:336). The disease recurred in the English colonies, and among Indians in contact with them, in 1757 (p. 337). English, French, and allied Indians suffered from the disease again in 1758 (p. 337). Georgia and South Carolina were affected in 1759-60, when the Chickasaws and the Catawba and small neighboring tribes suffered an estimated 50% population loss, and the disease spread to the Cherokees and Creeks (p. 338). The Menominees in the north were greatly reduced by the disease at this time, and even a group of Dakotas who travelled to Quebec was infected (p. 339). Reportedly, smallpox made "great ravages" among the Indians of the "Far West" in 1757 (Hamilton 1964:193).

Maxwell objects that I do not deal with changes in mode of subsistence. It is difficult for me to understand how increasing the food supply could "offset" the rate of depopulation caused by infectious disease agents. A population susceptible to a given disease agent is susceptible no matter how much food is available. Increasing food supplies might well strengthen resistance to tuberculosis and various deficiency diseases, and thus indirectly lower mortality from infectious disease agents which kill oftener in a debilitated than in a healthy population. Progressive resistance to a given disease agent in generations after that first decimated by it, a factor suggested by Thompson, probably had more to do with eventual recovery of Indian population than increased food supplies.

Maxwell asserts that food supplies increased in historic times in response to the introduction of ox-drawn plows into Indian agriculture. I agree that plowing probably increased food production *per farm unit* compared to prehistoric gardening. At the same time, I maintain that plowing with oxen constituted a shift from intensive gardening to extensive farming, at least in the densely settled areas of Indian civilization. That shift could occur only because depopulation had greatly diminished the density of settlement, allowing surviving Indians to expand their land holdings so that cultivating their enlarged fields with plow and oxen became feasible. The areas depopulation released from cultivation permitted the rapid growth of herds of cattle and horses and flocks of sheep (Acosta 1954:34, 81). Impressively large areas cultivated in prehistoric times remain uncultivated today, as Paddock points out in expanding upon this point.

Another change in mode of subsistence was the introduction from the Old World of cultigens that augmented agricultural production because they could be grown at different seasons or in different situations from the New World garden plants. Winter wheat and barley gained great importance among, for example, the Northern Piman-speakers, who came to raise it commercially in Mexican times (Russell 1908:76, 90; Castetter and Bell 1942:114-17; Ezell 1961:34). Yet Northern Piman population continued to fall until some time in the past century (Dobyns 1962:28-29), despite winter grains, cattle (Russell 1908:85-86; Underhill 1939:92), and other Old World food reinforcements. I did not deal with changes in subsistence because I considered them to be very much less important than disease agents in affecting historic population trends. Since Maxwell claims that there "may have been some dramatic population recoveries encouraged by increased available food," I would like to see evidence for at least one such case. Recovery is, in any event, unlikely to be quite as dramatic as depopulation. Disease can kill human beings overnight, and en masse, whereas creating a new human being is a highly individualistic process normally requiring nine months, plus several years of post-natal care and protection. Kroeber (1899:268) reported an epidemic that reduced the Smith Sound Eskimo from 253 in 1895 to 229 in 1896; the beginning of recovery was represented by an increase to 234 in 1897. If the birth rate had remained stable, five years would have been required to replace the loss in one epidemic episode.

That famines and warfare occurred in pre-Columbian times, as Maxwell suggests, is clear, but epidemics are another matter. I cited Cook's (1946*a*) analysis of the effect of central Mexican warfare and human sacrifice on population trends. Native chronicles do mention pre-conquest famines among the civilized Indians (Gillmor 1964:104-13; Anderson and Dibble 1953:23), and accounts of tribal life with simple technological outfits make clear the inherent danger of famine (Boas 1901-7:149; Jenness 1922:41-42, 108; Vanstone 1962:19; Holmberg 1950:93-98). Some disease agents affected Indians prior to discovery. Nonetheless, after reviewing the evidence, Cook (1946*b*:335) concluded "that the pathology expected in a semi-tropical region was present in central Mexico" for a long time prior to conquest. Native acquired immunity was demonstrated by comparative European susceptibility. The Indian population was "remarkably free" of severe epidemics or even general endemic disease, and grew "until it pressed close upon the food supply" at the time of the conquest. This conclusion is central to the analysis of historic population trends and disposes, I think, of one of Maxwell's strictures.

The archaeological evidence of intensive pre-Columbian land use that I mentioned briefly, augmented by Bernal's comment on Oaxaca and Paddock's summary of the historic deterioration of densely settled northern Oaxaca-southern Puebla, reinforce my belief that native American disease agents, famines, and warfare together failed to repress New World population growth significantly.

Migration, a significant factor in population trends in a single settlement or chiefdom, played no significant role in pre-contact hemisphere totals. Even a few seafaring castaways on the Northwest Coast (inbound) or Polynesia (outbound) would not have been very significant in the total. As for social controls, which Maxwell adduces as a population check, I doubt that much influence can be attributed to them so long as demographers cannot agree upon effects of social controls upon contemporary populations trends (e.g., Stycos 1963:266; Heer 1964:74-76). Kehoe and Kehoe rightly assert the possibility of pre-Columbian introductions of Eurasiatic diseases into the New World, especially on the Pacific Coast. The hypothetical single carrier on a single Japanese junk should not, however, be inflated from a bare possibility into a probability. The very susceptibility of Amerindians to Eurasiatic disease agents cited by Cook constitutes a major argument for previous New World isolation. The susceptibles in a junk crew would have been few, since some could be expected to have acquired immunity to any given contagion, to sustain an infection across the North Pacific during an off-course voyage.

Häusler and Stloukal both suggest a technique for prehistoric population estimation—counting skeletons recovered from completely excavated cemeteries—that I wish could be employed in the New World. Some of the practical limitations upon its application in America, in my own experience, are the following: The Northeastern Pai (Dobyns and Euler 1960:49) cremated their dead until the post-conquest dominant group imposed Christian ideals toward the end of the past century. After surveying scores of Pai prehistoric occupation sites and excavating a few, I have seen only one skeleton, and I suspect that it was buried by earlier inhabitants of the area in what later became a Pai ceremonial cave.

The ethnographic record is clear that

Northern Piman-speaking Indians cremated slain warriors in historic times, even after they had long been influenced by Roman Catholic missionaries (Cremony 1868:102; Russell 1908:46; 52-53, 194; Underhill 1939: 190). Selective cremation of cadavers of any part of a population causes estimates based upon skeletons of the remainder to be lower than reality. Like the Pai, the prehistoric Pimans practiced general cremation (Sayles 1937:91; DiPeso 1951:195-205; 1953: 242-44).

The Maya practiced mass interment and individual burial in graves, cists, vessels, funeral chambers, and *chultuns*, but also cremated some bodies (Wauchope 1964:363). Tribes that took heads, arms, legs, and so on as war trophies to dance over before discarding them on trash heaps (DiPeso 1956:518) further diminished the skeletal evidence available for systematic estimation.

Another practical problem in applying this technique is the nature of interethnic relations in the New World. The skeletons of prehistoric Indians who did bury their dead have long been fair game for the conquerors. I participated some years ago in salvaging skeletons from the cemetery at San José de Tucson, a mission visitation station established at an old Northern Piman ranchería. A complete excavation of the cemetery could have thrown much light on mortality rates in the mission population. Actually, the excavations salvaged few skeletons, because the non-Indians living in the city that grew up nearby had for decades dug up skeletal materials out of curiosity or in the process of authorized or clandestine treasure-hunting; a street had been cut through the area; a brick manufacturer had long dug clay from the valley floor, including portions of the cemetery; and ultimately bulldozers churned the remainder of the site up completely when the city began to bury its garbage in deep pits dug where prehistoric men had lived for millenia.

Recently members of the Cornell Peru Project conducted excavations in the Marcará River drainage in Ancash Department and recovered skeletal material from a burial hill, where graves had not been conspicuous. In contrast, the stone-roofed two-story burial chambers at a high-altitude site had long since been ransacked. The dirt on the first-story roof outside a second-story door of one of those structures contained more potsherds per cubic foot than any other deposit I touched trowel to, but preservation conditions were those of an open site. Many contemporary Peruvian rural settlements include among their occupational specialties that of *huaquero*—the commercial treasure-hunter. Since no market for skeletons appears to have developed, the search for Inca gold and silver and Chimu pots destroys a good deal of such evidence.

In sum, I have no expectation that the estimation of prehistoric Indian populations from excavated cemeteries can be performed often in the New World, because the circumstances permitting the preservation of full data are likely to be relatively rare.

Kunstadter raises two objections to using a depopulation ratio obtained in one area to estimate aboriginal numbers from a known nadir population in another area. The difficulty of estimating aboriginal size of extinct groups was implicitly recognized in note *d* to Table 2, where an infinite estimate was avoided by substituting a pre-extinction population for true nadir in the Caribbean area. Documentary research leads me to think that differences between socially and biologically defined Indians are more confusing in Anglo and French America than in Hispanic, and during the later period of population recovery rather than the earlier one of depopulation. Whatever truth there may be in Kunstadter's assertion that Indianness is culturally defined in Latin American countries applies particularly to the post-independence period. The Spanish imperialists worked out very precise racial classifications because racial difference seemed tremendously important to them. There was much immediate post-contact debate as to whether Indians possessed souls and should be legally and morally classified as human beings (e.g., Acosta 1954:395). I have considerable confidence in the accuracy of identification of an Indian by a 16th- or 17th-century Spanish gentleman concerned over the purity of his blood lines. Up to the time Indian population reached its nadir in the areas of prehistoric Indian civilization, the distinction between Indian and Mestizo was being made, I suspect, fairly accurately on biological grounds.

Later on, the problem of identifying an Indian became acute, as Fuchs and Kunstadter point out. Thomas (1961) dealt explicitly with many of the problems encountered in attempting to determine the present Indian population of the United States in preparing a population distribution map. He employed the term "societal Indians" for those persons "who are functional members of the Indian community and who are not in the process of joining the larger society" (p. 72). He considered the tribe as a quasi-political entity, so included individuals not even resident in a tribal area. In his discussion of data sources, Thomas decided that the U.S.A. census "usually lists as Indians only those people in Indian areas who are visibly Indian. It misses many people outside of the immediate area." Supplementary sources include tribal rolls reported by the Bureau of Indian Affairs in 1950. In some cases, Thomas obtained U.S.A. Public Health Service figures and estimates made by tribal officials or anthropologists. No tribal roll existed for Papagos, and the Bureau employed different criteria for identifying Indians in California and the Five Civilized Tribes. The Bureau had, moreover, no contact with many southern and eastern groups autoidentified as Indians (p. 73).

One weakness of overgeneralization in employing a ratio to estimate aboriginal population is well illustrated in Driver's stricture that he and Stewart place Indian population nadir in the United States at different dates and values than Rosenblat, whose figure I followed. Kehoe and Kehoe also think 1930 too late, and the population too large, for North American nadir. Applying the 20:1 depopulation ratio to Rosenblat's nadir (1930) figure yields an estimate of 6,647,940 aboriginals; applying it to Stewart's figure for 1920 yields 5,000,000, to Driver's figure for 1900, 4,000,000, and to Thomas' figure of 267,044 for 1910 (1961:76), 5,340,880. In personal communication, Thomas indicates that nadir came between 1840 and 1860, and that a North American native population of the magnitude I have estimated would have consumed "every deer and field mouse north of Mexico with that many hungry Indians!" The difference between these estimates highlights the conclusion that more and increasingly accurate analyses of population trends are needed.

Bennett's remarks are helpful in pointing to methodological considerations in demographic analysis of contemporary populations whose recent trends can be reconstructed from informant accounts. His emphasis on the range of analysis possible with good genealogical data strikes a very responsive chord. When he asserts, however, that some estimates that I have concluded are too small may be accurate approximations of "minimum" population, he seems to brush aside with one stroke the methodological shortcomings that I have tried to indicate and to ignore the temporal difference between aboriginal, disease-reduced, and recovering stages of population history.

In seeking reasons for the skepticism Kroeber and others displayed toward estimates of large pre-Columbian Indian populations, Bennett cites a pre-World War II reluctance to assign

any great antiquity to prehistoric horizons. My experience has been that personal prestige accrues to the archaeologist who excavates a palaeo-Indian site, and the earlier the better. A recent comment by Mason (1966: 194) and Cotter (1966:197) characterizes U.S.A. archaeologists' ideas about age prior to the use of C-14 for dating as exactly the opposite of Bennett's delineation.

In my use of the term North America, to which Driver mildly objects, I not only displayed my acculturation to Latin American usage, but also referred to an area larger than the U.S.A. and Canadian polities. My geographic "North America" includes the aboriginal peoples north of central Mexican civilization in the northern portion of the modern Mexican nation. It takes in everyone on the mainland north of the Cook-Simpson-Borah Central Mexico, so includes some fairly populous sedentary agriculturalists.

In citing social scientists who regard population density as affecting culture, I sought merely to suggest that the most accurate figures possible be employed for such analysis. I set out to try to clean some deadfalls out of an untidy woodlot, as most commentators perceived. This task proved difficult enough not to undertake the further one that Forbis would assign me, "to demonstrate at what points density triggers social and cultural responses." If Forbis be interested in this point, he may easily find analyses other than the ones cited: e.g., Buzzati-Traverso (1965:1440), Todd (1965:161), and McElheny (1966:976-77) on population size as a determinant of scientific research capacity and Maynard (1964: 1) and others on settlement size as a determinant of small-community institutional differentiation.

Inasmuch as Keyfitz and Carmagnani take me to task for daring to suggest an aboriginal population twice the size that Sapper estimated as the ecological maximum, I should state explicitly that I doubt that Sapper said the final word on that subject. His conclusions appeared in 1924, and over 40 years of archaeological research have greatly expanded scientific knowledge of prehistoric land use since then.

Bennett has emphasized this cumulation of data, while Paddock and Bernal and Haviland have summarized important evidence of which Sapper could have had little or no notion. Keyfitz and Carmagnani impress me as rather frivolous in seeking to a 40-year-old figure as limiting an estimate based in large part upon data brought to light more recently. Meighan (1961: 37) claimed that 90% of what was then known about California archaeology resulted from research during the preceding 25 years. The situation elsewhere in the hemisphere is similar.

The same frivolity appears in Keyfitz and Carmagnani's assertion that I underplay "objective evidence" in Mexican tax, baptismal, and death records and their characterization of the depopulation ratios as based on nadirs reached during the period of modern census-taking. As a matter of fact, only the North American nadir population as of 1930 was drawn from "modern" census figures. The Mexican figures which are a cornerstone in the analysis were drawn from the Cook, Simpson, and Borah analyses of precisely those types of records Keyfitz and Carmagnani label underplayed. If this sort of carefree approach toward analysis results from bringing great theory to bear upon demographic analysis, then I must express gratitude to Keyfitz and Carmagnani for considering my approach defective in that regard. Their theory seems to me to equip them with some rather curious blinders.

Thompson reinforces my argument for cross-checking data by questioning some of mine with an illustration of external checking on reported baptismal rates soon after the conquest in central Mexico. This exercise emphasizes the pertinence of knowing "minimum ceremonial requirements" for the sacrament. Mass baptisms (and recording) had to occur if the reported figures are at all valid. Later, ecclesiastical regulations became stricter and mass sacraments less likely. Still, rural portions of the Spanish Empire were long plagued by a shortage of priests, still acute in much of Latin America. A clergyman visiting a settlement may still administer sacraments to many individuals in a very short time, e.g., mass marriage at Vicos (Vázquez 1952:49; Price 1965:318-19).

I am tempted to take up Forbis' question whether population is free of culture; I refrain because it seems to me that he has simply misunderstood my independent/dependent variable terminology.

I hesitate to accept Blasi's population estimation, because I doubt that Jesuit missionaries in the parts of southern Brazil he specifies succeeded in converting half the Indian population between 1628 and 1635. They were not that successful elsewhere. The New World not having been conquered in a day, estimation from differing times is perhaps unavoidable in seeking one hemispheric total, so the hazard Dávalos cites is a real one.

In sum, I find the comments encouragingly indicative of a general willingness to consider the question of aboriginal population magnitude as an open one to be discussed in terms of evidence and methods not heretofore employed. To the extent that I manage to stimulate fresh research on Indian population trends, and to inhibit facile utilization of misleading estimates (cf. Debuyst 1961:11-29), I shall feel amply rewarded for this effort.

## Reply

*by* H. Paul Thompson

Dobyns' comment on the significance of food resources other than caribou loses sight of the point that this technique estimates human population not directly, but by inference; that is, the estimate obtained is for the range of human population required to keep the caribou population in equilibrium. The 5%-10% allowed for woodland caribou, etc., represents the extent to which these other resources could augment or replace the necessary function of the barren-ground caribou.

Driver's comment, "Anyone bold enough to estimate aboriginal population ought to be bold enough to estimate his own error" is appropriate. In terms of the Chipewyan model, I estimate that the mean rate of utilization. $\left(\frac{N_{DH}}{X}\right)$, was 141 ± 13 caribou per tent per year. The error of estimate, 13 caribou, is obtained by assuming that the upper and lower bounds for rates of utilization occur at three standard deviations from the mean. These figures yield a population mean of 6,426 and a range of 5,882 to 7,081.

Dávalos expresses doubts that such an ecological technique could be utilized for estimating aboriginal population of complex cultures. A model for estimating population of prehispanic Mesoamerica would have to be much more complex than the model used for illustration in this article. However, the lack of data on game animals would not present a problem. What would be required would be information on such factors as crop yields, the proportion of time fields lie fallow, man-hours required for cultivation, man-hours required for travel to and from fields, territoriality patterns, et cetera; that is, the kinds of considerations implied by Stloukal's comments on population estimation in Czechoslovakia.

Kehoe and Kehoe's "specific" criticism gives me an opportunity to demonstrate one reason for proposing such a technique for estimation of aboriginal population. They object to certain figures I use in obtaining an estimate of Chipewyan population; they might have gone on, taking advantage of my technique, to estimate Chipewyan population from the figures they prefer. From the figures they offer, we obtain: 171 caribou for

tenting, clothes and miscellaneous items, 52 for food, and 5 for the "off season," a total of 228 caribou per year. This rate of utilization implies 467 tents or a human population of 3,970. Since the Kehoes do not agree with my method of calculating minimum requirements, their ideal utilization would imply that this figure is the "average" population.

I know of no data which would show how long caribou-skin clothing or tents could be repaired and remain usable. The three- and five-year periods represent what I feel to be upper bounds on such usage. I did not intend to imply that a non-active adult would only require half sets of clothing. What I meant was that at the rate at which a superannuated adult would wear out clothing, hide usage for his clothing would be approximately the same as for a child's.

The general criticism put forth by the Kehoes has some validity. However, if we are actually going to analyze such a complex system as human population size, more detailed and wide ranging data must be collected. To an extent, the simplicity of this test case limited the accuracy of the population estimate. If several resources were utilized, as in equation (2), then the intersection of these estimates might be defined as the population range.

As Kundstadter suggests, the difference between the Chipewyan figures can be interpreted as a range, and it is both a result of imprecise data and a function of the nature of food resources and type of food chain. The rate of utilization is based on data which are incomplete. A really good estimate of this rate might be made using data on hide usage per tent (both number and type of use) and on variety and type of food utilization per tent as a function of time; a random sampling of age-sex distribution within tents; et cetera. Even with adequate data, however, Chipewyan population would probably fluctuate as a link in an ecological chain.

The particular form of equation (1) which Cook objects to was selected as a simple means of expressing, in mathematical terms, the explicit nature of the equilibrium assumption of the caribou population. In the expression $\left(\frac{N_{DH}}{X}\right) \cdot X$, the X which occurs symbolically in the denominator would cancel with the unknown X, except that $\left(\frac{N_{DH}}{X}\right)$ is defined as the rate of utilization of a resource per unit of population (X). It is this factor which allows us to solve for the unknown X, the number of tents of the aboriginal Chipewyan population.

Using the data reported by Macdonell, 400 souls in 1801, and Hearne's report of 90% reduction by the 1781 smallpox epidemic, Cook obtains an estimate of Chipewyan population possibly as good as, or better than, the one that I present using the same figures. Inadequate data are difficult to work with.

The 4,000 calories per day per person used for calculating Chipewyan food intake is higher than would be expected in a modern, civilized community. Adult Chipewyan men are in a minority; however, all records indicate that an adult woman would probably have required more calories than an adult male. Women, including pregnant wives and nursing mothers, hauled sledges, carted game from the kill to the camp, made and broke camp. The period of time to which the food intake figures refer is one of migration or relative abundance of caribou. Two additional features might be noticed: (1) A relatively high caloric intake would be required to help sustain body warmth in a subarctic enviroment. (2) The figure of 4,000 calories per day is considerably below the three pounds a day for Blackfeet or the eight pounds a day for Eskimo mentioned by Kehoe and Kehoe.

Both Dunn and Cook express concern over my definition of first contact. If the technique detailed in this paper were one for *counting* an aboriginal population, such comments would cast serious doubt on the validity of this Chipewyan population estimate. However, this technique does *not* count people. Instead, ethnographic data are utilized to obtain rates of utilization of resources. These rates $\left(\frac{N_{DH}}{X}\right)$ are then used to *calculate* a range and mean of the human population exploiting the resources. If these rates of exploitation have not changed appreciably between "truly" aboriginal conditions and my "first contact," then the population estimates obtained by this technique refer to the "true" aboriginal condition and not necessarily to the time defined as first contact. The utilization of caribou by the Chipewyan has indeed changed since aboriginal times; however, this change appears to have occurred quite a bit after what is defined as first contact.

I must agree with Dunn that one man's estimate is not necessarily any more accurate than another man's guess. However, an estimate based on a mathematical model does have the advantage of being derived from explicit assumptions. If one is not willing to accept certain assumptions and the figures they imply, then one may reject such figures without being an expert in a particular area.

I found Bennett's comments on estimating and analyzing aboriginal population dynamics extremely interesting. He is correct in his estimation of the relative ease with which this technique could be converted to evaluate population dynamics. In fact, it would be relatively easy to set up a computerized model, using a simulation language like SIMSCRIPT or IBM's General Purpose System Simulator, of such an equilibrium model to observe the effect of parameter variation. The advantage of using a computer model rather than just varying parameters in the equations, is that you can generate dynamic population curves over time for a more extensive evaluation of the effects of parameter change than is possible by hand calculation.

Bennett's comment on the use of social structure as a basis for population size estimation has an interesting implication for the Chipewyan. The relative lack of emphasis on territorial rights within the Chipewyan may have had an ecological origin in the cyclic movement of the barren-ground caribou through their winter range. The figures tabulated by Macfarlane (1908:168-169) may also be interpreted as indicating this movement of caribou for a particular locale. If the Chipewyan tended to follow the caribou as they moved over their winter range, the development of the concept of rigid territorial rights might be inhibited.

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# The Tip of an Iceberg: Pre-Columbian Indian Demography and Some Implications for Revisionism

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ALTHOUGH controversies about the nature of the American Revolution and related topics have not diminished our interest in early American Indian-white history, we have largely ignored recent demographic studies of the American Indian which may well give new direction to much of the rationale of colonial growth and progress. While it is difficult to make positive judgments, several scholars (mostly nonhistorians) have suggested an entirely new version of early Indian-white relations showing that Europeans had an overwhelming role in triggering an enormous depopulation of native American people.

What is involved here is truly one of the most fascinating numbers games in history, one that may well have a determining influence upon interpretive themes not only of early United States history but also of the history of all the Americas. The basic questions are these: is there evidence to show that there were some one hundred million Indians in the Western Hemisphere at the time of discovery? Further, is it true that this evidence may give us a new figure of nearly ten million Indians in the North America of 1492? And finally, is it now generally accepted by anthropologists that the figures of James Mooney, Alfred L. Kroeber, and Ángel Rosenblat—some one million Indians for pre-Columbian North America and eight to fourteen million Indians for the Western Hemipshere—are now out of date? If the new estimates for native American population (allowing for disagreement among authorities but general agreement that Mooney, Kroeber, and Rosenblat are now of only relative value) are to be considered, we must now cope with new evidence that indicates there were between fifty and one hundred million Indians in possession of the New World on the day that it was "discovered." Thus we have an invasion of Europeans into areas that were even more densely settled than parts of Europe. There is even the possibility that in the late fifteenth century the Western

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Hemisphere may have had a greater population than Western Europe.

There is one stalwart figure who as late as 1967 continued to dispute the new evidence. This is the Latin American scholar Rosenblat. He has accused Sherburne Cook and Woodrow Borah of discarding the testimony of respectable witnesses ("el testimo de respectables testigos") in their computations to arrive at high estimates of preconquest populations in central Mexico, in Hispaniola, and the whole Western Hemisphere. In his assessments of the findings of Cook and Borah, however, he consistently avoids either a discussion of their sophisticated methodology or an evaluation of the financial records they used as a basis of calculation. Rosenblat reluctantly concludes that the use of mathematical formulas in demography has given his work the appearance of error which he sought to avoid. One of the weaker links in his argument is his hasty attempt to discredit the original estimates of Indian mortality made by Bartolomé de Las Casas. In the end, Rosenblat's long defense of the researches of Mooney and Kroeber and his manner of repeating and reprinting his own findings (in three books) to strengthen his argument are unconvincing. Furthermore, Rosenblat seems to have overlooked the findings of certain scholars who disagreed with him. For instance, he makes no analysis of the work of the geographer Karl Sapper who as early as 1924 estimated a total of thirty-seven to forty-eight million Indians for the Americas in 1492.[1]

But let us leave Rosenblat and turn to the debate as it concerned other scholars in the 1960s and 1970s. It is somewhat ironic that the growing number of writers on United States history who have successfully employed quantitative methodology should have left Indian demographic research largely to specialists in other fields such as the Berkeley physiologist Cook, and his coauthors, historian-linguist Lesley B. Simpson and Latin Americanist Borah, and anthropologists Henry R. Dobyns

[1] Ángel Rosenblat's argument rests on the figures of James Mooney and A. L. Kroeber. Mooney, *The Aboriginal Population of America North of Mexico*, Smithsonian Miscellaneous Collections, LXXX (Washington, D. C., 1928), 33, estimates the total pre-Columbian Indian population north of Mexico including Greenland at 1,152,950. Kroeber, *Cultural and Natural Areas of Native North America*, University of California Publications in American Archaeology and Ethnology, XXXVIII (Berkeley, 1939), 131, 166, reduced Mooney's estimate to 1,025,950, nearly 10%, and made a hemispheric estimate of 8,400,000. Rosenblat's hemispheric pre-Columbian estimate is 13,385,000, but his estimate for North America, north of the Rio Grande River, is slightly lower than Kroeber's, an even 1,000,000. His population tables are found in his *La Población Indígena de América desde 1492 hasta la Actualidad* (Buenos Aires, 1945), 92, and in a revised work with unchanged pre-Columbian estimates, *La Población Indígena y el Mestizaje en América*, I: *La Población Indígena, 1492-1950* (Buenos Aires, 1954), 102. Rosenblat defends his estimates in a third book *La Población de América en 1492: viejos y nuevos cálculos* (Mexico, D. F., 1967), especially 1-9, 11-16, 81. Karl Sapper's statistical tables are printed in Julian H. Steward, ed., *Handbook of South American Indians*, V: *The Comparative Ethnology of South American Indians*, Bureau of American Ethnology Bulletin 143 (Washington, D. C., 1949), 656.



and Harold Driver.[2] A good place to begin probing the vital statistics in this revisionism is with Dobyns's article, "Estimating Aboriginal Indian Population."[3] What makes this study especially significant is its support of the methodology pioneered by the Berkeley scholars, Cook, Simpson, and Borah, in appraisals of Indian depopulation in California and Meso-America.[4] Dobyns reaches his hemispheric estimates by determining the demographic nadir or lowest population of Indians in regions of the Americas during the modern era when census data are available. Then he multiplies the nadir figure by a number representing the measure of Indian population loss from disease and other factors. By multiplying exponentially he estimates a population of 90,043,000 native Americans with a high projection of 112,553,750. The higher estimate results from the use of a historic depopulation ratio of 25 to 1, and the lower from a ratio of 20 to 1. Using this method, with an estimated nadir population of 490,000 aborigines in 1930 for North America, Dobyns with a ratio of 20 to 1 estimates a pre-Columbian population of 9,800,000. With the ratio of 25 to 1 he estimates a high of 12,250,000.[5]

Although Driver criticizes Dobyns's estimates by pleading for a lower nadir in estimating the aboriginal population of North America, which might result in a 50 percent cut in numbers, he nonetheless praises the

[2] The key publications of these scholars on the Indian demography debate are in the footnotes which follow.

[3] Henry F. Dobyns, "Estimating Aboriginal American Population: An Appraisal of Techniques with a New Hemispheric Estimate," *Current Anthropology*, VII (1966), 395-416.

[4] *Ibid.*, 403-407. Among the published works of the authors Dobyns relies primarily upon S. F. Cook, *The Extent and Significance of Disease among the Indians of Baja California, 1697-1773*, Ibero-Americana, 2 (Berkeley, 1937), 9-14; Sherburne F. Cook and Lesley Byrd Simpson, *The Population of Central Mexico in the Sixteenth Century*, Ibero-Americana, 31 (Berkeley, 1948), 19-22; Sherburne Cook and Woodrow Borah, *The Aboriginal Population of Central Mexico on the Eve of the Spanish Conquest*, Ibero-Americana, 45 (Berkeley, 1963), 6, 22-44, 157. Dobyns stresses that Cook's researches reveal that a "fatal defect" in both Kroeber's data and method was his ignoring of disease, especially epidemics: "Estimating Aboriginal American Population," *Current Anthropology*, VII (1966), 411.

[5] Dobyns, "Estimating Aboriginal American Population," *Current Anthropology*, VII (1966), 415. This estimate is presumably for Indians north of the Rio Grande, but Dobyns is not entirely clear on this point. Dobyns's final table of estimated populations is reprinted in Virgil J. Vogle, ed., *This Country Was Ours: A Documentary History of the American Indian* (New York, 1972), 253. As early as 1962 Borah estimated a hemispheric pre-Columbian population of 100,000,000. Dobyns states that Borah's depopulation ratio would fall between 20 and 25 and that Borah's estimate for population density per square kilometer implies 2.4 persons. Dobyns estimates 2.1 density: "Estimating Aboriginal American Population," *Current Anthropology*, VII (1966), 414. He cites Woodrow Borah, "¿América como modelo? El impacto demográfico de la expansión europa sobre el mundo no [illegible]," *Cuadernos Americanos*, VI (1962), 176-185.

new methodology.[6] Indeed, an examination of the numerous critiques published with Dobyns's estimates shows there is more agreement than dissent. What is more, the most distinguished student of Indian demography, Cook, approved Dobyns's work in conversations and in published commentary.[7] And I must confess that Dobyns in his articles and in convincing dialogue (in personal conversations and in correspondence) has persuaded me that historians may well have to adopt a whole new view of Indian demography, especially of Indian depopulation resulting from smallpox and other epidemic diseases.[8] Current literature on epidemic diseases among Indian people offers corroborative evidence to show, for example, the powerful impact of measles virus, easily airborne and devastating in its effect, upon certain native American communities, especially those in Alaska.[9]

[6] Driver's commentary is published with other critiques of Dobyns's article in *Current Anthropology*, VII (1966), 429-430. He questions Dobyns's exact geographical definition of North America and argues that his nadir population figure should be lowered (250,000 in 1890 for the area of the United States) and that his overall estimates might thus be cut by as much as 50%. Driver is also critical of Dobyns's estimate of 30,000,000 for the population of central Mexico which in turn was based upon the 1963 estimates of Cook and Borah ranging from 25,000,000 to 30,000,000. In their latest study, Cook and Borah arrive at the figure of 27,650,000. See Sherburne F. Cook and Woodrow Borah, *Essays in Population History: Mexico and the Caribbean*, I (Berkeley, 1972), 115. See also Harold E. Driver, *Indians of North America*, 2d ed. rev. (Chicago, 1969), 63-65, and his "On the Population Nadir of Indians in the United States," *Current Anthropology*, IX (1968), 330.

[7] Cook's published comments on Dobyns's estimates are in *Current Anthropology*, VII (1966), 427-429.

[8] Besides Cook's work, cited in n. 4, see Henry F. Dobyns, "An Outline of Andean Epidemic History to 1720," *Bulletin of the History of Medicine*, XXXVII (1963), 493-515, and Wilbur R. Jacobs, *Dispossessing the American Indian: Indians and Whites on the Colonial Frontier* (New York, 1972), 136, 136n, 191n, 214n. For the North American Indian population control, see *ibid.*, 130, 130n, 163.

[9] I am indebted to John C. Bolton, M. D., of San Francisco, a specialist in the study of modern epidemics, who has introduced me to the vast literature on the subject. Jacob A. Brody, M. D., of Alaska and New York State, and his associates have written several key articles on recent measles epidemics among Eskimos, Aleuts, and Alaskan Indians that demonstrate the severity of measles infection. See, e.g., "Measles Vaccine Field Trials in Alaska," *Journal of the American Medical Association*, CLXXXIX (Aug. 3, 1964), 339-342. Disease patterns of Indians in the southwestern parts of the United States where there have long been close association and intermarriage with non-Indians reveal no special weakness for epidemic diseases but that Indians, like their white neighbors, suffer from diabetes tuberculosis, and cancer, although "hypertension is apparently less frequent than among white persons." See Maurice L. Sievers, "Disease Patterns among Southwestern Indians," *Public Health Reports*, LXXXI (1966), 1075-1083 (quotation, p. 1082). See also S. M. Weaver, "Smallpox or Chickenpox: An Iroquoian Community's Reaction to Crisis, 1901-1902," *Ethnohistory*, XVIII (1971), 361-378, and Mark A. Barrow *et al.*, *Health and Disease of American Indians North of Mexico: A Bibliography, 1800-1969* (Gainesville, Fla., 1972), 57-58. Cook's current research on disease and the New England Indians indicates that earlier population estimates for the North Atlantic littoral (by Mooney and others) are too low. Correspondence in 1973 between W. R. Jacobs and Sherburne Cook.



An overriding consideration in favorably evaluating Dobyns's work is the consistently high estimates of aboriginal population in the formidable study by Cook and Borah, *Essays in Population*. These essays, concentrating on Mexico and the Caribbean, include both a brilliant analysis of methodology and a revealing commentary on historical demographic problems in making such estimates for pre-Columbian populations as the figure of twenty-five million for central Mexico. There is no question that the Cook and Borah estimates in these essays are as high as or higher than those that Dobyns provided for populations of specific areas. In some cases the Cook and Borah estimates skyrocket as high, for instance, as eight million for the pre-Columbian population of Hispaniola (present Haiti and the Dominican Republic) alone.[10] This figure far exceeds the most extravagant population estimates of contemporary Spaniards. Las Casas judged the native population of Hispaniola to be three or four million.[11]

Of course the most pressing question is how such a dense population could have supported itself in the Caribbean? On the basis of evidence given by Carl Sauer, Cook and Borah argue that the people of Hispaniola had perfected the domestication of food plants to the extent that they had a greater yield per hectare than comparable fields harvested in the Europe of 1492.[12] The supply of maize, beans, and cassava, supplemented by protein obtained by fishing and hunting, was more than enough to feed eight million people.

Given this prosperous state of affairs, there is still another obvious query to make. What brought about the sharp depopulation and finally the extermination of aborigines on Hispaniola, a process that was virtually complete by 1570? Cook and Borah, again partly relying on earlier studies by Sauer, argue that the harsh Spanish rule of native people, especially unusually brutal methods of exploiting Indian labor, was in part responsible for depopulation.[13] It was this brutality, resur-

[10] Cook and Borah, *Essays in Population*, 407.

[11] A table of Las Casas's estimates is in Dobyns, "Estimating Aboriginal American Population," *Current Anthropology*, VII (1966), 397. Philip Wayne Powell, *Tree of Hate: Propaganda and Prejudices Affecting United States Relations with the Hispanic World* (New York, 1971), discusses the significance of Las Casas's indictment of Spanish brutality and the origins of the Black Legend of Spanish cruelty. He argues, 139-159, that the causes of Indian depopulation were very complex.

[12] Cook and Borah, *Essays in Population*, 408, citing Carl Sauer, *The Early Spanish Main* (Berkeley and Los Angeles, 1966), 67-69, 157, *et passim*. Sauer, in his recent volume, *Sixteenth Century North America: The Land and the People as Seen by Europeans* (Berkeley and Los Angeles, 1971), 58, 59, 71, 205, 252-253, 286-288, 294-295, stresses the agricultural skills of the North American Indians, as does Wilbur R. Jacobs, "The Indian and the Frontier in American History: A Need for Revision," *Western Historical Quarterly*, IV (1973), 50-56.

[13] Cook and Borah, *Essays in Population*, 409, and Sauer, *Early Spanish Main*, 202-204, 283-289. Cook and Borah, however, still regard disease as the most important cause of Indian depopulation. Letter from Woodrow Borah to W. R. Jacobs, Apr. 15, 1973.

recting the Black Legend of Spanish cruelty (so distasteful to Kroeber that it may have led him to lower his population estimates of aborigines),[14] as well as disease that killed the Indians. Of all the epidemic diseases, smallpox seems to have been the scourge for millions of Indians in the Caribbean as well as in Meso-America and North America.[15]

If there were, indeed, eight million Indians on Hispaniola and some twenty-five million in central Mexico, how many were there in North America, north of the Rio Grande River? Probably at least as many as Dobyns has estimated—9,800,000 to 12,250,000—if we accept his methodology and mathematics. This estimate contrasts strikingly with the earlier ones of Mooney (1,152,950), Kroeber (1,025,950), and Rosenblat (1,000,000).[16]

Here we have it then—a new hemispheric estimate of Indian population that is almost breathtaking in its magnitude. If there ever was a tool for presentism in the writing of early American history in coping with the dispossession of the Indians, this is it. It is hard to imagine that our history can ever be the same again since we can scarcely portray the European invasion of the Western Hemisphere as the relatively quiet expansion of Europeans into sparsely settled lands. What we do with these new data and how we interpret them will be of great consequence, and we may be sure that Indian historians and the increasingly vocal American Indian Historical Society will have perceptive comments on their significance. Even if there is a general consensus that reduces the figure from one hundred million to fifty million—and some qualified investigators concede that we could hardly settle for less than that number—we must now accept the fact that the dismal story of Indian depopulation after 1492 is a demographic disaster with no known parallel in world history.[17] We must also acknowledge that the catalyst of all this was undoubtedly the European invasion of the New World.

[14] Dobyns, "Estimating Aboriginal American Population," *Current Anthropology*, VII (1966), 397, alludes to this point which seems to have validity if one notes the absence of condemnation of Spanish brutality in Kroeber's writings dealing with the Spanish occupation of the New World.

[15] Dobyns, "Estimating Aboriginal American Population," *Current Anthropology*, VII (1966), 410-412.

[16] See n. 1.

[17] Magnus Mörner in his perceptive study, *Race Mixture in the History of Latin America* (Boston, 1967), 50, was one of the first historians of Hispanic America to give tentative acceptance to the figure of 50,000,000, and about the same time J. H. Parry, *The Spanish Seaborne Empire* (New York, 1966), 213-228 (a chapter entitled "Demographic Catastrophe"), accepted the figure of 25,000,000 for the preconquest population of New Spain. Historians of the United States, however, seem to have overlooked the increasing volume of literature on Indian demography, and anthropologists who had a role in the debate have moved on to what appear to be other controversies in demography, in nonhistorical topics such as fecundity and recent trend analysis.



Admittedly, what we are dealing with is the tip of a formidable iceberg. Although there is sufficient evidence to tell us that the iceberg is really there, several questions suggest themselves. There is, for instance, reason to believe that the Indians were on the verge of a population decline before the Europeans arrived. This may well have been the case in central Mexico.[18] The possibility also exists that Indian populations were already in a cycle of depopulation. Even if we accept these arguments, there remains the question of why societies so large should have been so vulnerable. Where, moreover, is there evidence of a material culture to sustain such a large population? In Meso-America the remains of great Indian societies that flourished before and after the Spanish conquest still exist despite efforts to obliterate Indian civilizations. But in North America, at the time of first contacts with the Indians, there was no concerted effort to eradicate Indian culture. Yet the material remains of prehistoric Indian societies are sparse indeed.

If we accept the evidence pointing to disease as the most important factor in causing Indian depopulation, much remains to be done in studying the ability of individual tribes to resist waves of epidemics. Another factor is the significance of such data as the physical distance between population centers. There is also the argument that the infusion of Spanish blood (as well as the blood of other Europeans) into Indian societies helped to preserve them and to strengthen them against the impact of recurring epidemic disease waves. Anthropologist Edward Spicer sets forth a cycle theory of conquest and withdrawal of Europeans who may leave behind invigorated native societies enriched by cultural exchange.[19]

Given such probabilities and possibilities, the natural reaction may well be to doubt the veracity of recent theorizing on high pre-Columbian population estimates. Since most of the new data is based upon calculations which in turn rest upon a sifting of more conventional evidence, the hard documentary sources are lacking. Yet because such documentation is missing we may well be in error if we assume that the new methodologies cannot be trusted. In Indian history, as in the histories of other minorities, we are finding that the conclusive evidence found in Anglo-Saxon sources is often impossible to obtain.

My examination of the work of Cook, Borah, Sauer, Dobyns, and

[18] This point is mentioned in the debate over the 25,000,000 estimate for central Mexico. But the key issue is the reliability of evidence behind the estimate, for once the 25,000,000 figure for central Mexico is accepted, other high estimates based upon exponential calculations follow. For Rosenblat's criticism of Cook and Borah on this estimate see his *La Población de América en 1492*, 78-81.

[19] Edward H. Spicer, *Cycles of Conquest: The Impact of Spain, Mexico, and the United States on the Indians of the Southwest, 1533-1960* (Tucson, Ariz., 1962), 568. He neglects, however, to point out that examples of the withdrawal of Europeans are few indeed.

their critics leads me to believe that they have discovered a great historical iceberg concerning Indian populations. My own investigations, partly based upon examination of modern medical literature concerning Indians[20] and upon field research, tend to bear out their findings, as does archival evidence.

For instance, in my study of native-white contacts in Australia I have found that Australian Aborigines (who have a marked resemblance to certain Indian tribes of the American Southwest) had a swift depopulation after first contacts with whites in the 1830s. English agents, called "Protectors," reported a great "mortality" caused by disease and forced transfer to unfamiliar surroundings.[21] In my field trips to modern reserves of the Aborigines in Darwin, Alice Springs, and at Palm Island (off the coast of Queensland), there was a problem of being admitted at times because of recurring quarantines. Everywhere on reserves, despite medical precautions, native people seemed to be fighting off one epidemic after another of typhoid and other diseases. Leprosy, although partially checked, still persists among the native people of Queensland. Despite these onslaughts of disease, the Aboriginal population is now increasing, largely because of the Australian government's excellent social programs and medical facilities. The overall evidence, however, shows that widespread depopulation took place in early Australian history and that some native people, such as the Tasmanians, were finally wiped off the face of the earth.[22] Considering these facts, it is not surprising that Australia's leading anthropologist, the late A. P. Elkin, saw many parallels in the history of native-white relations in his country and in North America.[23]

In my field work and archival research on the impact of European contact with the native people of Papua, New Guinea, I found that the Melanesians withstood European invasion with more success than either the Australian Aborigines or the American Indians.[24] And the Polynesians, especially those of the Hawaiian Islands that I have studied, also

[20] See examples in n. 9.

[21] Protector George Augustus Robinson's reports and letters of the 1830s detail the depopulation of the Aborigines. See especially his "Reports on the Tasmanian Aborigines," A 612, 70ff, and his letter to Gov. George Arthur, Sept. 9, 1839, Mitchell Library, Sydney, Australia.

[22] Details of this unpleasant story are in Wilbur R. Jacobs, "The Fatal Confrontation: Early Native-White Relations on the Frontiers of Australia, New Guinea, and America—A Comparative Study," *Pacific Historical Review*, LX (1971), 293-309.

[23] See Elkin's classic study, *The Australian Aborigines*, 3d ed. (Sydney, 1954), 29, 44, 156-162 *et passim*, for his strictures on missions and missionaries among Aborigines and California Indians.

[24] The complexities of this contest are discussed in Jacobs, "The Fatal Confrontation," *Pac. Hist. Rev.*, LX (1971), 293-309.



seem to have withstood the disease and the cultural impact of Europeans better than the Indians or the Australian Aborigines. Nevertheless, Anglo-American pressure to dispossess Hawaiian native people of their lands and to stamp out their religion was (and continues to be) unrelenting.[25]

I found additional data bearing on the findings of Cook, Borah, and Dobyns in my field work and examination of archival data relating to the Arawaks, who were all but exterminated on Jamaica, Hispaniola, and other Caribbean Islands in the sixteenth century. Examination of the artifacts at the Arawak Museum and Burial Ground, maintained by the Institute of Jamaica, indicates that these peaceful, friendly people were skilled fishermen. It is certain that they ate large amounts of shellfish which, in addition to their skills in gardening, could have enabled them to feed a large population. One big Jamaican village site has five thick layers of shells, pottery, and bones dating back to about 1000 A.D. There are many other such sites, especially along the Jamaican coastline, formerly occupied by Arawaks.[26]

Although Cook and Borah find that the Arawaks were gone by about 1570 in Hispaniola, a handful of them survived in Jamaica as late as 1598. Spanish governmental manuscript material, preserved at the Institute of Jamaica, reveals that in 1598 the government attempted to protect the scattered survivors "from the many that there were" by giving them an independent village sanctuary. This effort, however, was bitterly resisted by the local Spanish ranchers who succeeded in keeping the Indians in virtual slavery as fieldhands or cattleherders.[27] Later English records, some as late as 1700, show Indians as slaves or servants on Jamaican plantations, but there is no way to determine if these Indians were surviving Arawaks or other tribesmen.[28] Many Indians,

[25] On dispossession of native Hawaiians from their lands see, e.g., translations of "Native Testimony," I: Land of Papua, in Gov. Kekuanaoa's sworn testimony, Mar. 18, 1846, Hawaiian Archives, Honolulu. The "Thaddeus Journal," 1819-1820, Hawaiian Mission Children's Society, Honolulu, has entries through March and April 1820 which reveal the almost astonishing fear and hostility New England missionaries had toward native religions. Modern American Mormon missionaries are among the most enthusiastic proselytizers among native Hawaiian people. Their success is assured by the popularity of their operation of a Polynesian Cultural Center on Oahu.

[26] I am indebted to Professor Richard S. Dunn of the University of Pennsylvania for assistance in locating this Arawak burial site and for other help in carrying out my investigations in Jamaica.

[27] Translations from the Archivo De Indias, Seville, AGI, 54-3-28, Dec. 26, 1598, Audiencia de Santa Domingo Isla de Jamaica, Pietrsz Bequest, Institute of Jamaica, MST 29, Vol. 2. This manuscript volume also contains documents concerning the unsuccessful governmental project to create a special Indian village sanctuary.

[28] See the English estate inventories preserved at the Jamaica Archives, Spanish Town, Jamaica. The following inventories are representative of those listing Indian slaves: Inventory, Book 5, 48B; Inventory of James Pinnock Junior, enrolled at

after seventeenth-century wars in the British North American colonies, were sent as slaves to the British West Indies.

Thus, although the evidence is fragmentary and scattered among a number of different sources, it does show that native peoples have greatly suffered under the impact of the European invasion. In some cases, as clearly illustrated by the experience of Australian Aborigines and the Arawaks of the Caribbean, there was a sharp decline in native population after first contacts with whites, even the wiping out of indigenous native communities that accompanied dispossession and seizure of their homelands.[29] But a still larger transformation took place in the Western Hemisphere in the great demographic disaster involving the disappearance of millions of Indians following the first invasions of Europeans. The dimensions of this disaster have now been outlined by Cook, Borah, and Dobyns. It does indeed appear to be the tip of an iceberg of tremendous proportions.

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Mar. 22, 1700, *ibid.*; Inventory of Elizabeth Potts, Widow [Mar. 1700], 67, 67B, *ibid.*; Inventory of chattels and debts of Thomas Harry [Jamquier], [Mar. 1675], Inventory Book I, 1674-1675. The latter inventory is on microfilm.

[29] Jacobs, *Dispossessing the American Indian*, 19-30, 126-172, discusses the significance of interrelationships between dispossession and depopulation of native peoples. See also Douglas L. Oliver's learned study, *The Pacific Islands*, rev. ed. (New York, 1961), 1-80.